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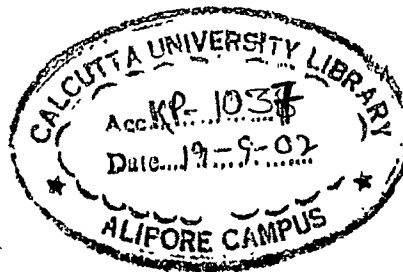
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American Political Science Association

1527 New Hampshire Avenue,
Washington, DC 20036

Editor's Notes

Responding to my editor's notes in the last issue, one reader asked, "Do you have any statistics (or approximate idea) about what proportion of the 2,000 scholars who have reviewed for the *APSR* are drawn from outside the United States or Canada?" Since I was making the point that *APSR* reviewers were a large and diverse group, this is a fair question, and we have now gathered the data to answer it.

So far during my term as editor, 2,043 different scholars have provided reviews.[1] One hundred and ninety-one of these (9.3%) are from 25 countries other than the United States.[2] As the inquiry seems to assume, many of our reviewers are from Canada, but their number (44 or 2.2%) is actually lower than that for the United Kingdom (50 or 2.4%).[3] An additional 8 (.4%) reviewers are from Australia and 4 (.2%) are from Ireland, for a total of 95.9% of reviewers from primarily English-speaking countries. The remaining 4.1% are from 21 other countries, including 11 from Germany, 10 each from Israel and Norway, 8 from the Netherlands, 7 from Switzerland, 5 each from France, Italy, and Japan, 4 from Sweden, and the rest scattered among Austria, Belgium, Brazil, China (including Hong Kong), Denmark, Finland, New Zealand, the Philippines, Poland, South Korea, Spain, and Taiwan. The reviewers from the United States have come from forty-five states and the District of Columbia.

Considering only the 191 reviewers from countries other than the United States, the United Kingdom provides 26.2%, Canada 23%, 4.2% are from Australia, and 2.1% from Ireland. Thus, of our non-U.S. reviewers, only 55.5% are from primarily English-speaking countries. Germany contributes 5.8% of non-U.S. reviewers; Israel and Norway, 5.2% each; the Netherlands, 4.2%; Switzerland, 3.7%; France, Italy, and Japan, 2.6% each; Sweden, 2.1%. The rest are from the other nations mentioned above, including a combined total of 4 (2.1%) from China and Taiwan.

Compared to reviewers, a somewhat larger proportion of scholars who have submitted manuscripts to the *APSR* over the past five and a half years are from

countries other than the United States. Sixteen percent (245) of the 1,535 submitting authors are from 39 other countries, most prominently, 38 (2.5%) from the United Kingdom,[4] 34 (2.2%) from Canada, 27 (1.8%) from Israel, 19 (1.2%) from Germany, and 16 (1%) from the Netherlands.[5] The remaining authors are from all the other countries mentioned above for reviewers (except the Philippines) plus Botswana, Colombia, the Czech Republic, Greece, India, Jamaica, Malaysia, Mexico, Nigeria, Pakistan, Romania, Singapore, South Africa, Turkey, and Venezuela. The minor disproportion between reviewers and authors from other countries is inevitable: Despite the knowledge and resources available to the editor, editorial board, and *APSR* interns, including bibliographic databases and information available on the Internet, we cannot know about the qualifications of as many scholars in other countries as read and aspire to publish in the *Review*. When new people submit manuscripts, however, or volunteer to review, they are added to our database and considered as potential reviewers of future manuscripts.

Considering only the 245 authors from other countries, 15.5% are from the UK, 13.9% from Canada, 11.0% from Israel, 7.8% from Germany, 6.5% from the Netherlands, 4.9% from Australia, 4.1% from Japan, 3.7% from Norway, 3.3% from Spain, 2.9% from Sweden, 2.4% from Taiwan, and 2.0% each from Denmark and France, with the balance distributed over the other countries mentioned above. In the United States, we have received manuscripts from all fifty states and the District of Columbia.

Slightly different figures are produced when considering submissions (rather than authors), since American authors tend to submit more papers than others do, or reviews (rather than reviewers), since we receive more reviews from Americans. But one would not draw very different conclusions looking at those data. While 84% of all authors are from the United States, 85.4% of the 2,117 new manuscripts received over the past five and a half years were from here, and while 90.7% of all reviewers are in the United States, 93.9% of the 7,024 reviews originated here.[6]

Scholars in most countries contribute reviews in similar proportion as authors from each of those countries submit manuscripts, reflecting the comple-

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[1] Data reported here cover the period from August 15, 1995, through February 2, 2001.

[2] Where scholars are "from" refers only to mailing addresses, since we have no systematic information on the nationality of authors or reviewers. All addresses used for this analysis are our most current (addresses known to be outdated have been deleted). For the purpose of learning where people are "from," a certain amount of error is introduced by the fact that individuals may be in a location only temporarily, and they may have been at a different location when a particular review was written or an article submitted. I do not think this introduces any systematic bias into the analysis; we sometimes receive submissions or reviews from non-American scholars when they are visiting at a university in the United States, and American scholars occasionally send manuscripts or reviews from temporary locations abroad. Since current addresses are used for this report, they may include temporary addresses for particular individuals.

[3] UK reviewers are from England (44), Scotland (5), and Northern Ireland (1).

[4] UK authors are from England (31), Scotland (3), and Wales (4).

[5] In the case of papers with multiple authors, only corresponding authors are counted in these figures. We do not always have addresses for others and, in any case, counting all authors would weight the results toward countries with more multiple-author manuscripts.

[6] Another reason that the proportion of reviews from the United States is larger than the proportion of reviewers who are here is that the data reported in this analysis include the 33 members of the editorial board, all of whom are located most of the time in the United States; in general, members of the board review far more often than other reviewers (they produced a combined grand total of 676 reviews over the last five and a half years).

mentary nature of the processes. For example, scholars in Canada comprise 2.2% of all submitting authors and 1.9% of all submissions; they are 2.2% of all reviewers and contribute 1.7% of all reviews. "On the other side of the pond," scholars from the UK are 2.5% of all submitting authors and contribute 2.2% of all submissions; they are 2.4% of all reviewers and contribute 1.3% of all reviews. We can, of course, expect fewer submissions and reviews to originate in countries whose native language is not English, but Israel, the Netherlands, and Norway present similar relative proportions: Scholars from Israel are 1.8% of all submitting authors and contribute 1.7% of all submissions; they are .5% of all reviewers and contribute .4% of all reviews. Scholars from the Netherlands are 1.0% of all submitting authors and contribute .9% of all submissions; they are .4% of all reviewers and contribute .2% of all reviews. Scholars from Norway are .6% of all submitting authors and contribute .6% of all submissions; they are .5% of all reviewers and contribute .3% of all reviews.

Since we are generally less certain of the availability and addresses of reviewers in other countries, we almost always try to reach them by e-mail before sending papers for review. If we are unable to establish e-mail or fax contact with a reviewer not located in the United States, we will not send the manuscript even if we have an apparently reasonable mailing address.[7]

We are more likely to use reviewers abroad in some subfields than in others. For obvious reasons, this is very common for comparative politics papers. We also often draw on reviewers in other countries for political theory manuscripts. Even in studies of American political behavior we may send a manuscript on partisanship, voting behavior, or economic attitudes, for example, to a scholar abroad who has studied similar attitudes or behavior in other countries. We also regularly use reviewers from other disciplines, e.g., classics and philosophy, economics, history, psychology, and sociology, and we receive manuscripts from scholars in all those fields (and others).

Sometimes we are unable to find a scholar who has been recommended by the editorial board or whom we discover in our literature searches. Thanks to the resources available on the Internet, however, we are able to turn up e-mail and postal addresses for almost all scholars we have sought out, regardless of their country, continent, or discipline. The *APSR* and political science more generally benefit significantly from our receiving both manuscripts and reviews from scholars all over the world, and we are grateful for their contributions (as well as for those of Yahoo!™, AltaVista®, Google™, assorted other Internet search engines and resources, and the computer and e-mail systems that make this possible). It is easy to forget how recently this type of international contact has become so commonplace and so regularized.

Another reader wrote in offering to review manuscripts; within two days of receipt of his letter, a

suitable manuscript arrived and I was able to take up his offer. We are always happy to hear from scholars who would like to get involved in the review process; a vita should also be sent so we can make appropriate manuscript assignments.

Complete data on geographic distribution of *APSR* authors, reviewers, submissions, and reviews (on which these notes are based) are posted on our web site (www.ssc.msu.edu/~apsr), with a link from the electronic version of these editor's notes (which can be reached from the March 2001 issue table of contents).

I will be happy to hear from readers who have other comments or questions about the *APSR* or its review process. Please write to apsr@ssc.msu.edu.

Scholars wishing to submit manuscripts are reminded that the editor and editorial office will change this fall. Please see the June issue for specific date and address information.

Ada W. Finifter
Editor

INSTRUCTIONS TO CONTRIBUTORS

General Considerations

The *American Political Science Review* strives to publish scholarly research of exceptional merit, demonstrating the highest standards of excellence in conceptualization, exposition, methodology, and craftsmanship. Because the *Review* reaches a diverse audience of scholars and practitioners, contributors should demonstrate how their analysis illuminates a significant research problem, or answers an important research question, of general interest in political science. For the same reason, authors should strive for a presentation that will be understandable to as many scholars as possible, consistent with the nature of their material.

Since the *Review* publishes original research, authors should not submit articles containing tables, figures, or substantial amounts of text already published or forthcoming in other places, or that are included in other manuscripts submitted for review, whether to book publishers or periodicals (including online journals), or are otherwise committed. In many such cases, subsequent publication of this material would violate the copyright of the other publisher. The *APSR* also does not consider papers currently under review at other journals or that duplicate or overlap with parts of larger manuscripts that have been submitted to other publishers (including publishers of both books and periodicals). Since space is at a premium, submission of articles substantially similar to manuscripts submitted or published elsewhere, or to part of a book or other larger work, is also strongly discouraged. If there is any doubt about whether these policies would apply to a particular case, the author should discuss any such publications related to a submission in a cover letter to the Editor. The Editor should also be notified of any related submissions to other publishers, whether for book or periodical publication, that occur while an article is under review at the *APSR* and that would fall within the scope of this policy. Copies of related publications may be requested.

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[7] There are, however, a few non-U.S. reviewers who have reviewed so often that we no longer inquire before sending manuscripts.

For manuscripts containing quantitative evidence and analysis, authors should describe their empirical procedures in sufficient detail to permit reviewers to understand and evaluate what has been done and, in the event the article is accepted for publication, to permit other scholars to carry out similar analyses on other data sets. For example, for surveys, at the least, sampling procedures, response rates, and question wordings should be given; authors are encouraged to calculate response rates according to one of the standard formulas given in The American Association for Public Opinion Research, *Standard Definitions: Final Dispositions of Case Codes and Outcome Rates for RDD Telephone Surveys and In-Person Household Surveys* (Ann Arbor, MI: AAPOR, 1998). This document is available on the Internet at <http://www.aapor.org/ethics/stddef.html>. For experiments, provide full descriptions of experimental protocols, methods of subject recruitment and selection, information about any payments and debriefing procedures, and other relevant details. It is desirable for articles to be self-contained, and authors should not refer readers to other publications for descriptions of these basic research procedures.

Please indicate variables included in empirical analyses by capitalizing the first word in the variable name and italicizing the entire variable name the *first* time each is mentioned in the text. Authors should also be consistent in using the same names for variables in text and tables, and avoid overuse of acronyms and computer abbreviations when discussing variables in the text. All variables appearing in tables should have been mentioned in the text and the reason for their inclusion in the analysis discussed.

As part of the review process, authors may be asked to submit additional documentation if procedures are not sufficiently clear; the review process works most efficiently if such information is given in the initial submission. If readers are advised that additional information is available from the author, printed copies of that information should be submitted with the manuscript. If the amount of this supplementary information is extensive, please inquire about alternate procedures.

The *APSR* uses a double-blind review process. Authors should follow the guidelines for preparing anonymous copies in the "Specific Procedures" section below.

Manuscripts that are largely or entirely critiques or commentaries on previously published articles will be reviewed using the same general procedures as for regular articles with one exception. In addition to the usual number of reviewers, such manuscripts will also be sent to the scholar(s) whose work is being critiqued, in the same anonymous form as they are sent to other reviewers. Comments from the original author(s) to the editor will be invited as a supplement to the advice of regular reviewers. There are several purposes for this notice to the original author(s): (1) to encourage review of the details of analyses or research procedures that might occasionally escape the notice of disinterested reviewers; (2) to enable prompt publication of critiques by supplying critiqued authors with early notice of their existence and, therefore, more adequate

time to reply; (3) as a courtesy to critiqued authors. Authors of critiques should therefore send as many additional copies of their manuscripts as will be required for this purpose.

Manuscripts being submitted for publication should be sent to Professor Ada W. Finifter, Editor, *American Political Science Review*, Department of Political Science, Michigan State University, 303 South Kedzie Hall, East Lansing, MI 48824-1032. Correspondence concerning manuscripts under review may be sent to the same address or, preferably, by e-mail to apsr@ssc.msu.edu. Please note that the *APSR* does not process *new* manuscripts during August; *new manuscripts* should be mailed to arrive either before or after. *Invited revisions and reviews* may be submitted at any time and will be processed upon arrival.

Manuscript Formatting

Manuscripts must not be longer than 45 pages *including text, all tables and figures, notes, references, and appendices*. The page size guideline is based on the U.S. standard 8.5 X 11 inch paper and *must be adjusted accordingly by authors who are submitting manuscripts printed on longer paper. Font size must be at least 11 point for all parts of the paper.* The entire paper must be double-spaced, with the sole exception of tables for which double-spacing would require a second page otherwise not needed. All pages should be numbered in one sequence, and text should be formatted using a normal single column no wider than 6½ inches, as is typical for manuscripts (rather than the double-column format of the published version of the *Review*), and printed on one side of the page only. Include an abstract of no more than 150 words. The *American Political Science Review* style of embedded citations should be used, and there must be a separate list of references at the end of the article. Please do not use footnotes for simple citations. These specifications are designed to make it easier for reviewers to read and evaluate papers. Papers not adhering to these guidelines are subject to being rejected without review.

For submission and review purposes, authors are encouraged to place footnotes at the bottom of the pages instead of using endnotes and may locate tables and figures (on separate pages and only one to a page) approximately where they fall in the text. However, manuscripts accepted for publication must be submitted with endnotes, and with tables and figures on separate pages at the back of the manuscript with standard indications of text placement, e.g., [Table 3 about here]. In deciding how to format your initial submission, please consider the necessity of making these changes if your paper is accepted. Authors of accepted manuscripts are also required to submit camera-ready copy of graphs or other types of figures. Instructions will be provided.

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Please follow these specific procedures for submission:

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7. Authors should not fax abstracts, send any part of the paper itself by e-mail, or send the abstract by e-mail before their submission packet is in the mail.

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American Political Science Association

1527 New Hampshire Avenue,
Washington, DC 20036

Editor's Notes

As most readers already know, my second term as editor of the *APSR* is drawing to a close. Authors, reviewers, publishers, and others wishing to contact the editorial office or the book review office are asked to note the transition information below.

FUTURE SUBMISSIONS, REVIEWS, CORRESPONDENCE, AND BOOKS FOR REVIEW

The new Managing Editor of the *American Political Science Review*, Professor Lee Sigelman, will assume his duties on September 1, 2001. All issues of the 2001 volume, however, are in the charge of the current editor, and the December 2001 issue will be prepared in the Michigan State University office in the fall. During August 2001, we will be packing and shipping editorial files from Michigan State University to the new office at George Washington University. Correspondence concerning manuscripts currently or previously under review may be sent to the Michigan State University address (or by email to apsr@ssc.msu.edu) through August but may have to be forwarded toward the end.

New Submissions

New manuscripts to be considered for publication should be sent to the current editor *only* if they will arrive by July 31, 2001. That address is Professor Ada W. Finifter, *American Political Science Review*, Department of Political Science, Michigan State University, 303 South Kedzie Hall, East Lansing, MI 48824-1032.

New manuscripts that will arrive on or after September 1, 2001, should be sent to Professor Lee Sigelman, *American Political Science Review*, Department of Political Science, George Washington University, 2201 G Street N.W., Washington, DC 20052.

Please note that the *APSR* observes an August "moratorium" on new manuscripts. For this reason, please do not send new manuscripts that will arrive during August to either office. Because of the change in editorship, any new manuscripts that arrive at the Michigan State University office after July 31, 2001, will be shipped or forwarded by regular mail to the new editor.

Revised Manuscripts

Authors of invited revisions whose revised manuscripts are ready for resubmission during August and early September should note instructions and suggestions about the editorial transition contained in their correspondence with the current editor. In the case of any uncertainty about where to submit an invited revision, please send e-mail to apsr@ssc.msu.edu, write to the

Michigan State University office at the address above, or call 517/432-1265.

Reviews

Reviewers receiving manuscripts for review during the summer months are asked to take special note of the transition information that will be contained in their manuscript packages.

Books for Review

Publishers wishing to submit books for review may continue sending them to the current book review office at the University of California, Riverside, only if they will arrive by July 27th. After that please send all books for review to Professor Lee Sigelman, *American Political Science Review*, Department of Political Science, George Washington University, 2201 G Street N.W., Washington, DC 20052.

Ada W. Finifter
Editor

INSTRUCTIONS TO CONTRIBUTORS

General Considerations

The *American Political Science Review* strives to publish scholarly research of exceptional merit, demonstrating the highest standards of excellence in conceptualization, exposition, methodology, and craftsmanship. Because the *Review* reaches a diverse audience of scholars and practitioners, contributors should demonstrate how their analysis illuminates a significant research problem, or answers an important research question, of general interest in political science. For the same reason, authors should strive for a presentation that will be understandable to as many scholars as possible, consistent with the nature of their material.

Since the *Review* publishes original research, authors should not submit articles containing tables, figures, or substantial amounts of text already published or forthcoming in other places, or that are included in other manuscripts submitted for review, whether to book publishers or periodicals (including online journals), or are otherwise committed. In many such cases, subsequent publication of this material would violate the copyright of the other publisher. The *APSR* also does not consider papers currently under review at other journals or that duplicate or overlap with parts of larger manuscripts that have been submitted to other publishers (including publishers of both books and periodicals). Since space is at a premium, submission of articles substantially similar to manuscripts submitted or published elsewhere, or to part of a book or other larger work, is also strongly discouraged. If there is any doubt about whether these policies would apply to a

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Governance in a Partially Globalized World
Presidential Address, American Political Science Association, 2000
ROBERT O. KEOHANE *Duke University*

Facing globalization, the challenge for political science resembles that of the founders of the United States: how to design institutions for a polity of unprecedented size and diversity. Globalization produces discord and requires effective governance, but effective institutions are difficult to create and maintain. Liberal-democratic institutions must also meet standards of accountability and participation, and should foster persuasion rather than rely on coercion and interest-based bargaining. Effective institutions must rely on self-interest rather than altruism, yet both liberal-democratic legitimacy and the meaning of self-interest depend on people's values and beliefs. The analysis of beliefs, and their effect on institutional outcomes, must therefore be integrated into institutional analysis. Insights from branches of political science as diverse as game theory, rational-choice institutionalism, historical institutionalism, and democratic theory can help political scientists understand how to design institutions on a world—and human—scale.

Talk of globalization is common today in the press and increasingly in political science. Broadly speaking, globalization means the shrinkage of distance on a world scale through the emergence and thickening of networks of connections—environmental and social as well as economic (Held et al. 1999; Keohane and Nye [1977] 2001). Forms of limited globalization have existed for centuries, as exemplified by the Silk Road. Globalization took place during the last decades of the nineteenth century, only to be reversed sharply during the thirty years after World War I. It has returned even more strongly recently, although it remains far from complete. We live in a partially globalized world.

Globalization depends on effective governance, now as in the past. Effective governance is not inevitable. If it occurs, it is more likely to take place through interstate cooperation and transnational networks than through a world state. But even if national states retain many of their present functions, effective governance of a partially—and increasingly—globalized world will require more extensive international institutions. Governance arrangements to promote cooperation and help resolve conflict must be developed if globalization is not to stall or go into reverse.

Not all patterns of globalization would be beneficial. It is easy to conjure up nightmare scenarios of a globalized world controlled by self-serving elites working to depress wages and suppress local political autonomy. So we need to engage in normative as well as positive analysis. To make a partially globalized world benign, we need not just effective governance but the *right kind* of governance.

My analysis begins with two premises. The first is that increased interdependence among human beings produces discord, since self-regarding actions affect the welfare of others. At worst, the effects of international

interdependence include war. As international relations "realists" have long recognized, interdependence and lack of governance make a deadly mixture. This Hobbesian premise can be stated in a more positive form: Globalization creates potential gains from cooperation. This argument is often seen as "liberal" and is associated with Adam Smith and David Ricardo, but it is actually complementary to Hobbe's point. The gains of cooperation loom larger relative to the alternative of unregulated conflict. Both realists and liberals agree that under conditions of interdependence, institutions are essential if people are to have opportunities to pursue the good life (Hobbes [1651] 1967; Keohane 1984; Keohane and Nye [1977] 2001).

My second premise is that institutions can foster exploitation or even oppression. As Judith Shklar (1984, 244) expresses it, "no liberal ever forgets that governments are coercive." The result is what I will call the governance dilemma: Although institutions are essential for human life, they are also dangerous. Pessimistic about voluntary cooperation, Hobbes firmly grasped the authoritarian horn of the governance dilemma. We who are unwilling to accept Hobbe's solution incur an obligation to try to explain how effective institutions that serve human interests can be designed and maintained. We must ask the question that Plato propounded more than two millenia ago: Who guards the guardians?

Clearly, the stakes are high: no less than peace, prosperity, and freedom. Political science as a profession should accept the challenge of discovering how well-structured institutions could enable the *world* to have "a new birth of freedom" (Lincoln 1863). We need to reflect on what we, as political scientists, know that could help actors in global society design and maintain institutions that would make possible the good life for our descendants.

In the first section of this essay I sketch what might be called the "ideal world." What normative standards should institutions meet, and what categories should we use to evaluate institutions according to those standards? I turn next to what we know about real institutions—why they exist, how they are created and maintained, and what this implies about their actual

Robert O. Keohane is James B. Duke Professor of Political Science, Duke University, Perkins Library 214, Durham, NC 27708-0204 (rkeohane@duke.edu).

operation. In the concluding section I try to bring ideal and reality together to discuss institutional design. Are there ways by which we can resolve the governance dilemma, using institutions to promote cooperation and create order, without succumbing to exploitation or tyranny?

DESIRABLE INSTITUTIONS FOR A PARTIALLY GLOBALIZED WORLD

Democratic theorists emphasize that citizens should reflect on politics and exercise their collective will (Rousseau [1762] 1978), based on what Jurgen Habermas (1996, 296) has called "a culturally established background consensus shared by the citizenry." Governments derive their just powers from the consent of the governed, as the American Declaration of Independence proclaims, and also from their reflective participation.

To the potential utopianism of democratic thought I juxtapose what a former president of this association, who was also my teacher, called the liberalism of fear (Shklar 1984). In the tradition of realistic liberalism, I believe that the people require institutional protection both from self-serving elites and from their worst impulses, from what James Madison ([1787] 1961) in *Federalist 10* called the "violence of faction." Madison and Shklar demonstrate that liberalism need not be optimistic about human nature. Indeed, at the global scale the supply of rogues may be expected to expand with the extent of the market. Institutional protection from the arbitrary exercise of coercion, or authoritative exploitation, will be as important at the global level as at the level of the national state.

The discourse theory of Habermas restates liberal arguments in the language of communicative rationality. Legitimacy, in this view, rests on institutionalized procedures for open communication and collective reflection. Or, as Habermas (1996, 304) quotes John Dewey ([1927] 1954, 208), "the essential need is the improvement of the methods and conditions of debate, discussion, and persuasion." The ideal that Habermas, John Rawls (1971), Robert Dahl (1976, 45–6), and many other political philosophers have upheld is that of rational persuasion—changing others' minds on the basis of reason, not coercion, manipulation, or material sanctions. Persuasion in practice is much more complex than this ideal type, but seeking to move toward this ideal seems to me to be crucial for acceptable governance in a partially globalized world.

With these normative considerations in mind, we can ask: What political institutions would be appropriate for a partially globalized world? Political institutions are persistent and connected sets of formal and informal rules within which attempts at influence take place. In evaluating institutions, I am interested in their *consequences, functions, and procedures*. On all three dimensions, it would be quixotic to expect global governance to reach the standard of modern democracies or polyarchies, which Dahl (1989) has analyzed so thoroughly. Instead, we should aspire to a more loosely coupled system at the global level that attains the major objectives for which liberal democracy, or polyarchy, is designed at the national level.

Consequences

We can think of outcomes in terms of how global governance affects the life situations of individuals. In outlining these outcome-related objectives, I combine Amartya Sen's concept of capabilities with Rawls's conception of justice. Sen (1999, 75) begins with the Aristotelian concept of "human functioning" and defines a person's "capability" as "the alternative combinations of functionings that are feasible for her to achieve." A person's "capability set represents the freedom to achieve: the alternative functioning combinations from which this person can choose" (p. 75). Governance should enhance the capability sets of the people being governed, leading to enhancements in their personal security, freedom to make choices, and welfare as measured by such indices as the UN Human Development Index. And it should do so in a just way, which I think of in the terms made famous by Rawls (1971). Behind the "veil of ignorance," not knowing one's future situation, people should regard the arrangements for determining the distribution of capabilities as just. As a summary set of indicators, J. Roland Pennock's (1966) list holds up quite well: security, liberty, welfare, and justice.

Functions

The world for which we need to design institutions will be culturally and politically so diverse that most functions of governance should be performed at local and national levels, on the principle familiar to students of federalism or of the European Union's notion of "subsidiarity." Five key functions, however, should be handled at least to some extent by regional or global institutions.

The first of these functions is to limit the use of large-scale violence. Warfare has been endemic in modern world politics, and modern "total warfare" all but obliterates the distinction between combatants and noncombatants, rendering the "hard shell" of the state permeable (Herz 1959). All plans for global governance, from the incremental to the utopian, begin with the determination, in the opening words of the United Nations Charter (1945), "to save succeeding generations from the scourge of war."

The second function is a generalization of the first. Institutions for global governance will need to limit the negative externalities of decentralized action. A major implication of interdependence is that it provides opportunities for actors to externalize the costs of their actions onto others. Examples include "beggar thy neighbor" monetary policies, air pollution by upwind countries, and the harboring of transnational criminals, terrorists, or former dictators. Much international conflict and discord can be interpreted as resulting from such negative externalities; much international cooperation takes the form of mutual adjustment of policy to reduce these externalities or internalize some of their

costs (Keohane 1984). Following the convention in the international relations literature, I will refer to these situations, which resemble classic prisoners' dilemmas, as collaboration games (Martin 1992; Stein 1983).

The third function of governance institutions is to provide *focal points* in coordination games (Fearon 1998; Krasner 1991; Martin 1992; Schelling 1960). In situations with a clear focal point, no one has an incentive to defect. Great efficiency gains can be achieved by agreeing on a single standard—for measurement, technical specifications, or language communication. Actors may find it difficult, for distributional reasons, to reach such an agreement, but after an institutionalized solution has been found, it will be self-enforcing.

The fourth major function of governance institutions for a partially globalized world is to deal with system disruptions. As global networks have become tighter and more complex, they have generated systemic effects that are often unanticipated (Jervis 1997). Examples include the Great Depression (Kindleberger 1978); global climate change; the world financial crisis of 1997–98, with its various panics culminating in the panic of August 1998 following the Russian devaluation; and the Melissa and Lovebug viruses that hit the Internet in 2000. Some of these systemic effects arise from situations that have the structure of collaboration games in which incentives exist for defection. In the future, biotechnology, genetic manipulation, and powerful technologies of which we are as yet unaware may, like market capitalism, combine great opportunity with systemic risk.

The fifth major function of global governance is to provide a guarantee against the worst forms of abuse, particularly involving violence and deprivation, so that people can use their capabilities for productive purposes. Tyrants who murder their own people may need to be restrained or removed by outsiders. Global inequality leads to differences in capabilities that are so great as to be morally indefensible and to which concerted international action is an appropriate response. Yet, the effects of globalization on inequality are much more complicated than they are often portrayed. Whereas average per-capita income has vastly increased during the last forty years, cross-national inequality in such income does not seem to have changed dramatically during the same period, although some countries have become enormously more wealthy, and others have become poorer (Firebaugh 1999). Meanwhile, inequality within countries varies enormously. Some globalizing societies have a relatively egalitarian income distribution, whereas in others it is highly unequal. Inequality seems to be complex and conditional on many features of politics and society other than degree of globalization, and effective action to enhance human functioning will require domestic as well as international efforts.

Whatever the economic effects of globalization, social globalization certainly increases the attention paid to events in distant places, highlighting abuses that are widely abhorrent. Such issue advocacy is not new: The transnational antislavery movement between 1833 and 1865 is an important historical example. Yet, the expansion of concern about human rights during the past two decades has been extraordinary, both in the scope of rights claimed—and frequently codified in UN agreements—and in the breadth of transnational advocacy movements and coalitions promoting such rights (Keck and Sikkink 1998). Concern about poverty, however, has not been matched by effective action to eliminate the source of human misery (World Bank 2000).

Procedures

Liberal democrats are concerned not only with outcomes but also with procedures. I will put forward three procedural criteria for an acceptable global governance system. The first is *accountability*: Publics need to have ways to hold elites accountable for their actions. The second is *participation*: Democratic principles require that some level of participation in making collective decisions be open to all competent adults in the society. The third is *persuasion*, facilitated by the existence of institutionalized procedures for communication, insulated to a significant extent from the use and threats of force and sanctions, and sufficiently open to hinder manipulation.

Our standards of accountability, participation, and persuasion will have to be quite minimal to be realistic in a polity of perhaps ten billion people. Because I assume the maintenance of national societies and state or state-like governance arrangements, I do not presume that global governance will benefit from consensus on deep substantive principles. Global governance will have to be limited and somewhat shallow if it is to be sustainable. Overly ambitious attempts at global governance would necessarily rely too much on material sanctions and coercion. The degree of consensus on principles—even procedural principles, such as those of accountability, participation, and persuasion—would be too weak to support decisions that reach deeply into people's lives and the meanings that they construct for themselves. The point of presenting ideal criteria is to portray a *direction*, not a blueprint.

Now that these normative cards are on the table, I turn to some of the positive contributions of political science. In the next section I ask how we can use our knowledge as political scientists to design sustainable institutions that would perform the functions I have listed. In the final section I explore how these institutions could facilitate the democratic procedural virtues of accountability, participation, and persuasion. These issues are all part of one overriding question: How can we design institutions that would facilitate human functioning, in the sense of Aristotle or Sen?

INSTITUTIONAL EXISTENCE AND POWER

How can authoritative institutions exist at all? This is a question that Rousseau ([1762] 1978, I, 1) claimed not to know how to answer and that students of international politics have recently debated. No student of international relations is likely to forget that institu-

tions are fragile and that institutional success is problematic.

To address this issue, I begin with the contributions of rational-choice institutionalism, which has insistently sought to raise the question of institutional existence and has addressed it with the tools of equilibrium analysis (Shepsle 1986). To design appropriate and legitimate global institutions, we need to fashion a rich version of institutionalist theory, which uses the power of the rationality assumption without being hobbled by a crude psychology of material self-interest. But before discussing such a theory, it is important to indicate briefly why a simple functional answer is not sufficient.

The Inadequacy of Functional Theories

One can imagine a simple functional theory of global institutions by which the demand for governance, generated by globalism, creates its own supply. Such an account has the defining characteristic that the real or anticipated effects of a process play a causal role (Cohen 1978). A functional account can only be convincing if the causal mechanism for adaptation is clearly specified. In biology, one such mechanism is Darwinian evolution, which in its strong form implies environmental determinism. The selection environment determines which individual organisms, or other units, survive. Although the individual units may undergo random mutations, they do not act in a goal-directed fashion, and they do not fundamentally affect the environment that selects them. But environmental determinism and the absence of goal-seeking behavior are not assumptions that seem to fit human social and political reality (Kahler 1999). Hence, evolutionary arguments in the social sciences have mostly stayed at the level of metaphor. They certainly do not provide us with warrant for a functionalist account of how governance arrangements for globalization would emerge, since the causal mechanism for selection seems even weaker at the global level than with respect to competition among states.

The other causal mechanism for functional theories involves rational anticipation. Agents, seeing the expected consequences of various courses of action, plan their actions and design institutions in order to maximize the net benefits that they receive. Ronald Coase (1960) and Oliver Williamson (1985) use functional theory in Cohen's sense to explain why firms exist at all. "Transactional economies" account for choices of markets or hierarchies (Williamson 1975, 248). That is, the more efficient organizational arrangement will somehow be selected.

But there is a micro-macro problem here, since arrangements most efficient for society are not necessarily optimal for the leaders of the organization. At the level of societies, as Douglass North has pointed out, the history of real economies is one of persistent inefficiency, which he explains essentially in terms of the free rider problem. Even if an institutional innovation would increase efficiency, no one may have the incentive to develop it, since institutional innovation is a

public good (North 1981, 68; 1990, 93). Indeed, rent-seeking coalitions have incentives to resist socially beneficial institutional innovations that would reduce their own advantages (Olson 1982).

Functional solutions to the problem of institutional existence are therefore incomplete. There must be political entrepreneurs with both the capacity and the incentives to invest in the creation of institutions and the monitoring and enforcement of rules. Unless the entrepreneurs can capture selective benefits from their activities, they will not create institutions. And these institutions will not be effective unless sufficient compliance is induced by a combination of material and normative incentives. To use economic language, problems of supply (Bates 1988; Shepsle and Weingast 1995) as well as demand have to be solved.

Mancur Olson's (1965) analysis of the logic of collective action has two major implications for the governance of a globalized world. First, there is no guarantee that governance arrangements will be created that will sustain high levels of globalism. As Western history reveals, notably in the collapse of the Roman Empire and in World War I, extensive social and economic relations can be undermined by a collapse of governance. At the global as well as national level, political scientists need to be as concerned with degrees of governance as with forms of governance (Huntington 1968, 1).

The second implication of Olson's insight is that we cannot understand why institutions vary so much in their degree of effectiveness simply by studying institutions. To focus only on existing institutions is to select on the dependent variable, giving us no variance and no leverage on our problem. On the contrary, we need to explore situations in which institutions have *not* been created, despite a widespread belief that if such institutions were created, they would be beneficial. Or we can compare situations in which institutions exist to earlier ones in which they were absent (cf. Tilly 1975, 1990).

Institutional Theory and Bargaining Equilibria

Rational-choice institutionalism in political science insists that institutions, to persist, must reflect bargaining equilibria of games in which actors seek to pursue their own interests, as they define them. This perspective, stated elegantly by Kenneth Shepsle (1986), is not new in its essentials. Indeed, in investigating the effects of constitutions, Aristotle held that those vesting authority in the middle class will promote rationality and the protection of property rights (*Politics* IV, xi, 4–15).[1] He sought to explain variations in constitutional forms by referring to variations in social conditions (*Politics* IV, iii, 1–6). And he argued that a stable constitution is not only one that a majority seeks to maintain but also one for which "there is no single section in all the state

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[1] All references to Aristotle's *Politics* are to Barker's (1948) translation.

which would favor a change to a different constitution" (IV, ix, 10).

In the terms of rational-choice institutionalism, Aristotle was interested both in institutional equilibrium and equilibrium institutions. So were Smith (1776) and Madison ([1787] 1961). The eighteenth century view, which resonates with rational-choice institutionalism, was that the "passions" of people in bourgeois society can be interpreted in terms of their interests (Hirschman 1977, 110) and can be moderated by wise institutions.

Yet, rational-choice institutionalism has been more rigorous and more relentless than its predecessors in insisting on explaining, by reference to incentives, why institutions exist. Because rational-choice theorists seek to explain in formal terms why institutions exist, they have to confront directly two critical questions. (1) Under what conditions will political entrepreneurs have incentives to create institutions? (2) What makes such institutions stable?

Since institutions are public goods, they are likely to be underproduced and, at the limit, not produced at all. Hence there must be selective incentives for politicians to invest in institutional innovations (Aldrich 1995). In addition, significant advantages must accrue to institutional innovators, such as conferring on them control over future rules or creating barriers to entry to potential competitors. Otherwise, latecomers could free ride on the accomplishments of their predecessors, and anticipation of such free riding would discourage institutional innovation. Another barrier to entry for latecomers may be ideology. Insofar as only a few ideologies, quite distinct from one another, can exist, first movers would gain an advantage by seizing favorable ideological ground (cf. Hinich and Munger 1994). The implication for our problem of institutional design is that first-mover advantages are essential if institutional innovation is to occur.

The European Union (EU) provides a compelling example of first-mover advantages in international organizations. New members of the EU have to accept, in their entirety, the rules already established by their predecessors. As a result, the innovators of the European Community—the six founding members—gain persistent and cumulative advantages from having written the original rules. These rules are important. Even if implementation is often slow, during the 1990s all members of the EU had implemented more than 75% of EU directives, and more than half had implemented more than 85% (Martin 2000, 174). First-mover advantages are also evident in the processes of writing national electoral rules: Those who win an earlier election create rules that subsequently favor their party, policy positions, and personal careers (Bawn 1993; Remington and Smith 1996).

The second key question is that of stability. If institutional rules constrain majorities, why do these majorities not simply change the institutional rules to remove the constraint? If they do, what happens to the "structure-induced equilibrium" that solves Arrow's paradox of social choice (Riker 1980)? In other words,

why do institutions not simply "inherit" rather than solve Arrow's paradox (Aldrich 1993)?

The general answer seems to be that institutions generate rules that resolve Arrow's paradox, for example, by giving agenda-setting power to particular agents (Shepsle 1986) or by requiring supermajorities to change institutional arrangements. These rules ensure that majorities cannot alter them easily when the median voter's preferences change.

First-mover advantages and agenda control provide incentives for institutional innovation and help to stabilize institutions. They operate somewhat differently, however, in coordination and collaboration situations, as described above. In situations of coordination, institutions, once accepted, are in equilibrium. Participants do not have incentives to deviate unilaterally from widely accepted standards for Internet connectivity or airline traffic control. Institutions to solve collaboration problems are much more fragile. After an agreement on institutions to solve collaboration problems is reached, participants typically have incentives to defect if they expect to avoid retaliation from others (Martin 1992).

Students of international relations have used this distinction to show how much more difficult it is to maintain collaboration institutions: Monitoring and enforcement are essential. Furthermore, during the bargaining process "hold-outs" may be able to negotiate better terms for themselves in collaboration than in coordination situations, since threats to remain outside collaboration-oriented institutions are more credible than threats to remain outside a widely accepted coordination equilibrium. In international relations, the side-payments negotiated by China and India to join the ozone regime, and the refusal so far of developing countries to be bound by emissions restrictions in a climate change regime, illustrate this point.

If we keep our normative as well as positive lenses in focus, we will see that this apparent advantage of coordination institutions has a dark side. Initiators of coordination institutions can exercise great influence over the choice of focal points, thereby gaining an enduring first-mover advantage over their rivals (Krasner 1991). Collaboration institutions do not offer such first-mover advantages, since participants can defect at lower cost. Collaboration institutions therefore provide fewer opportunities, as compared to coordination institutions, for coercion of latecomers. Real institutions usually combine coordination and collaboration functions, and therefore also contain a mixture of destabilizing (or liberating) elements and stabilizing (or potentially oppressive) ones.

Institutions, whether emphasizing coordination or collaboration, necessarily institutionalize bias, in favor of groups that have agenda control or wish to maintain the status quo. It is therefore not surprising that advocates of social equality, such as Thomas Jefferson, and democrats such as Rousseau, are often suspicious of institutions. Barriers to competition confer monopolistic privileges and therefore create normative problems. Yet, institutions are essential for the good life.

Normatively, those of us who believe in Shklar's

(1984) "liberalism of fear" both support institutions and are cautious about them. We support them because we know that without well-functioning political institutions, life is indeed "nasty, brutish, and short." But we are suspicious, since we understand how self-serving elites can use institutions to engage in theft and oppression. In a partially globalized world, we will need institutions of broader scope. But as in national democracies, eternal vigilance will be the price of liberty.

Rational-choice theory has led to fruitful inquiries into the issue of why institutions exist, because it relentlessly questions any apparent equilibrium. The skeptical question—why should institutions exist at all?—has ironically led to a deeper understanding of institutions than has the assumption that we could take their existence for granted and focus on how they work.

The Limits of Rational Egoism

Commenting on Toqueville, Albert Hirschman (1977, 125) has pointed out a normative problem with the emphasis on self-interest that I have thus far emphasized: "Social arrangements that substitute the interests for the passions as the guiding principle of human action for the many can have the side effect of killing the civic spirit." There is also an analytical problem: We know from a variety of work that this egoistic picture is seriously incomplete.

Rationalist theory often carries with it the heavy baggage of egoism. People are viewed as self-interested individuals whose incentives are strictly shaped by their environment, including the rules of the institutions in which they are located. The most sophisticated version of this argument does not make the essentialist claim that "human nature" is fundamentally egoistic but gives priority instead to an instrumentalist logic. Hans J. Morgenthau (1967, chap. 1), for example, argues that since power is a necessary means to other goals in international politics, we can analyze leaders' behavior in terms of power even if they do not seek power for its own sake (see also Wolfers 1962, chap. 7). For rationalist students of American and comparative politics, political leaders may have a multiplicity of goals, but since continuation in office is a necessary means to achieve any of them, it can be regarded as a universal objective of politicians, whether purely instrumental or consummatory (Geddes 1994; Mayhew 1974).

Thoughtful theorists of rational choice recognize that the assumption of egoism oversimplifies social reality. Norms of reciprocity and fairness often affect social behavior (Levi 1997; Ostrom 1990). The theoretical predictions derived from the assumption of egoism encounter serious predictive failure in experimental settings (Ostrom 1998). And survey research shows that citizens evaluate the legitimacy of the legal system on the basis not only of their own success in dealing with it but also of their perceptions of its procedural fairness (Tyler 1990).

Sometimes the assumption of egoism is defended on the ground that only with such simple models can solutions be found to strategic games. But the folk theorem of game theory demonstrates that an essen-tially unlimited number of equilibria appear in all interesting games. When the equilibrium rabbit is to be pulled from the hat, we are as likely to get a thousand rabbits as one. Equilibria multiply like rabbits in Australia and are about as useful. As Elinor Ostrom (1998, 4) commented in her address to this association three years ago, the assumptions of rationality, amoral self-interest, and lack of influence from social norms lead to explanatory chaos: "Everything is predicted: optimal outcomes, the Pareto-inferior Nash equilibria, and everything in between."

In addressing the problems of institutional design, it is a good thing that people are not purely egotistical. It would be difficult to understand the creation of major political institutions—from the U.S. Constitution to UN-sponsored human rights agreements—if we took egotism and the free-rider problem too seriously. We are indeed wise to assume that institutions, to be in long-term equilibrium, must be broadly consistent with the self-interest of powerful actors. But we cannot understand the origins of institutions if we banish principled action from our analytic world.

Egoists have a hard time overcoming problems of mistrust, because they know that everyone has an incentive to disguise his or her preferences. Only costly signals will be credible; but the cost of signalling reduces the prospective value of cooperation and limits the agreements that can be reached. Egoists also have difficulty solving bargaining problems, since they do not recognize norms of fairness that can provide focal points for agreement. Cool practitioners of self-interest, known to be such, may be less able to cooperate productively than individuals who are governed by emotions that send reliable signals, such as love or reliability (Frank 1988). In Sen's (1977, 336) phrase, people in purely rational-choice models are "rational fools," incapable of distinguishing among egoistic preferences, sympathy, and commitments.

As Sen makes clear, rejecting the premise of egoism does not imply rejecting the assumption of rationality—more or less bounded in Herbert Simon's (1985) sense. Nor does it imply altruism: People can empathize with others without being self-sacrificing. What it does is demand that norms and values be brought back into the picture. Committed individuals, seeking policy goals as well as office for its own sake, and constrained by norms of fairness or even by more transcendental values, can nevertheless calculate as rationally as the egoists of economic theory.

In thinking about a partially globalized world, one might be tempted to dismiss half the governance dilemma by pointing out that because international institutions are very weak, they are unlikely to be oppressive. For example, contemporary opponents of globalization and associated international institutions have sought to portray the World Trade Organization (WTO) as some sort of bureaucratic monster, although my own university's budget allocates more money in two weeks than WTO spends in a year.[2]

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[2] The budget of the WTO for 2000 is 127,697,010 Swiss francs, or approximately $73.8 million, at September 2000 exchange rates.

True as it is, this appeal to institutional weakness is not fully convincing. The problem is not that international organizations are huge and oppressive but that they are seen as serving the vested interests of the powerful and privileged. And they do. Indeed, they are institutions of the privileged, by the privileged, and all too often for the privileged. There are severe restraints on the powers of the international civil servants who lead these organizations, but few such checks limit the ability of the strongest states, such as the United States, to dictate policies and veto personnel. Yet, in the absence of such institutions, dictation by strong states would be even more direct, less encumbered by rules. Like Churchill's aphorism about democracy, an institutionalized world is probably the worst form of governance—except for the alternatives.

Ironically, it is the privileged who often appeal to altruism—their own, of course—as the guarantee against the abuse of power. Political scientists have spent too much time debunking altruism as a general motivating factor in politics to be detained long by such claims. Anyone my age has lived through the disastrous failures of social systems, notably in Russia and China, based on the premise that human nature can be remolded. The reality is that the worst people thrive under the cover of such grand visions. In any event, the heterogeneity of the world's population makes it impossible to imagine any single ideology providing the basis for a coherent, value-based system of global governance. The answer to global governance problems does not lie in revelation.

Faced with the governance dilemma, those of us interested in governance on a world scale could retreat to the pure self-interest model. With that set of assumptions, we would probably limit world governance. We would sacrifice gains that could result from better cooperation in order to guard against rule by undemocratic, self-serving institutions responsive, in opaque ways, to powerful elites. If we were successful, the result would be to limit global governance, even at the expense of greater poverty and more violent conflict. We might think ourselves wise, but the results would be sad. Due to excessive fear, we would have sacrificed the liberal vision of progress.

Institutions, Expectations, and Beliefs

It may seem that we are at an impasse. Sober reliance on limited institutions based on pure self-interest could lead to a low-level "equilibrium trap." But we may be tempted to settle for such an equilibrium rather than accept oppressive global institutions.

There may be a way out of this impasse. That path is to pay more attention than we have to expectations of how others will behave and, therefore, to underlying values and beliefs. Expectations are critical determi-

nants of action. They depend heavily on trust, reputation, and reciprocity, which depend in turn on networks of civic engagement, or social capital. Building such networks is an incremental process. Engagement in a just set of social relations helps create personal integrity, which is the basis for consistent principled action (Grant 1997). Networks of civic engagement are not easily divided into "international" and "domestic" but, rather, cross those lines (Keck and Sikkink 1998). Rational strategic action depends on the expectations and incentives that these networks create.

Until recently, students of international politics paid too little attention to beliefs. The realists insisted on the dominance of interests and power, which they traced to material factors. Marxist and neoclassical political economists also relied on material forces for their explanations. Students of institutions, such as I, sought to gain credibility by showing that our theories are as realistically based in interests and power as those of our realist adversaries, that we are not tainted by the idealist brush. Ironically enough, however, the theory of strategic interaction on which we all rely has insistently argued that beliefs are crucial to understanding any game-theoretic situation (Morrow 1994; Wendt 1999).

The fact that strategic action depends on expectations means that understanding historical and cultural context is critical to any analysis of how institutions operate. Peter Katzenstein (1993, 1996) has used the differing responses of Germany and Japan to military defeat and economic revival to make this point in a cogent and forceful way. Historical explorations of institutional phenomena and negotiations may draw effectively on rational-choice theory, but they must go well beyond its premises to describe multidimensional human behavior (Bates et al. 1998). Indeed, political scientists have quite a bit to learn from international law, which studies rational strategic action in the context of rules and rule making, deeply structured by interests and power but also reflecting the influence of ideas on interests and on how power is exercised (Grewe 2000).

A major task before our discipline is how to connect rational strategic action with beliefs and values. In her presidential address three years ago, Ostrom (1998) linked rational-choice theory with the laboratory experiments of cognitive science to show that institutional incentives, fundamental norms of trust, and the practice of reciprocity (Axelrod 1984; Ostrom 1990) all provide crucial foundations of cooperation. "At the core of a behavioral explanation," Ostrom (1998, 12) said, "are the links between the trust that individuals have in others, the investment others make in trustworthy reputations, and the probability that participants will use reciprocity norms." That is, principled values, "congealed" in institutions, provide the basis for meaningful rational actions and direct such actions in ways that we can describe and explain (Riker 1980).

Robert Putnam's *Making Democracy Work* exemplifies a productive analysis of the connections among values, social norms, and rational behavior. Putnam argues that "networks of civic engagement" produce

better government. Why does he think so? Not because engaged people necessarily work altruistically for the common good but because these networks increase costs of defection, facilitate communication, and create favorable expectations of others' likely actions (Putnam 1993, 173–4).

Understanding beliefs is not opposed to understanding interests. On the contrary, interests are incomprehensible without an awareness of the beliefs that lie behind them. Indeed, even the financial self-interest so dear to political economists implies acceptance of norms that would be incomprehensible in many societies, whether those imagined by Jean-Jacques Rousseau or studied by twentieth-century anthropologists. The values and beliefs that are dominant within a society provide the foundations for rational strategy.

Even beliefs about beliefs can be as solid as any material interests. As Barry O'Neill shows brilliantly in a book awarded the Woodrow Wilson Prize, prestige refers to "beliefs about beliefs"—whether people think that others hold a high opinion of someone (O'Neill 1999, 193). Prestige is a "social fact," like a dollar bill (Searle 1995): Although it is genuinely real, its importance does not lie in its material manifestation but in the beliefs people hold. Both money and prestige matter a great deal in politics, but only insofar as people hold beliefs about others' beliefs.

To see how beliefs relate to issues of institutional design, think about two possible worlds of the future. In one of them, the "normative anarchy" portrayed by the "political realism" of the late twentieth century (Waltz 1979) prevails. That is, there is no consensus about principled beliefs on the basis of which governance across national boundaries can take place and transnational networks of people with similar beliefs are virtually nonexistent. The only norm on which there is general agreement is the "antinorm" of sovereignty: the principle that the rulers of each state are supreme internally and independent from external authority (Bull 1977). Since I expect self-interested agents to continue to dominate among politicians, I would expect, in this world, familiar patterns of modern Western international politics to persist. Rationally egoistic politicians would have few incentives to fight for principles of human rights, since to do so they would have to overcome both collective action problems and ridicule from "realistic" statesmen and academics.

Now consider another world, in which certain principles have become generally accepted—as opposition to slavery became generally accepted in the nineteenth century and as certain human rights seem to be becoming accepted now (Keck and Sikkink 1998). In this world, transnational advocacy networks would be active. Behavior in this world would, of course, be different from that in the first world. Even those who do not subscribe to these principles would have to calculate the costs of acting counter to them.

Now let us go a step farther and imagine that the principled innovators of the new principles, and the value-based transnational networks, disappear, to be replaced by purely rational egoists. Would the egoists seek immediately to overturn these norms of human

rights? Probably not, unless they had compelling internal reasons, as tyrants, to do so. Ordinary egoists, governing nontyrannically, would have interests in mimicking the principled leaders whom they succeed. Furthermore, the egoists would face serious collective action problems in overturning norms of human rights: Their counterpart egoists would have an interest in defending those rights in order to enhance their reputation as principled agents. As a result, egoistic self-interest would counsel them to uphold the norms established and even to bear some costs in order to send credible signals that they believe in the norms (even though, by assumption, they do not). The effect of former principles would persist for a while, although it would eventually fade.

What this thought-experiment illustrates is a simple but fundamental point. Beliefs in norms and principles—even beliefs only held in the past—can profoundly affect rational action in the present. Joseph Schumpeter ([1942] 1950, 137) made the famous argument sixty years ago that capitalism requires precapitalist values: "The stock exchange is a poor substitute for the Holy Grail." The facile response to his argument at millenium's end could be: "Yes, but he didn't take into account NASDAQ." More seriously, however, the varieties of capitalism in the world today, from Japanese corporatism to American legalism to Russian organized theft, make it clear that what is economically rational in each context varies enormously. Schumpeter was wrong about the staying power of capitalism but right about the dependence of institutions, capitalism included, on beliefs.

INSTITUTIONAL DESIGN: BRINGING IDEALS AND REALITY TOGETHER

I began by sketching an ideal vision—a liberal and democratic vision—of how institutions should work. On the liberal side, it includes what one might call the liberalism of progress, represented by such eighteenth-century thinkers as Smith and Madison. But it also includes Shklar's liberalism of fear, which emphasizes the potential depravities of human nature and the pathologies of human institutions and is deeply cognizant of imperialism, totalitarianism, and the Holocaust (Arendt [1951] 1958). The liberalism of fear is horrified by the atrocities of Rwanda and Bosnia, but these atrocities do not shake its liberalism, which was forged in the searing recognition that human action can be horrible.

The liberalism of progress and the liberalism of fear are two sides of the same coin. They both seek to understand how otherwise unattractive human passions can nevertheless promote the general good. Madison is the American father of such a realistic liberalism, but it has deep roots both in English utilitarianism, going back to Hobbes, and in French thought (Hirschman 1977; Keohane 1980). Neither Madison nor Smith indulged in the more utopian dreams of the liberalism of progress. Even though potential gains from trade, combined with advancing technology, make it possible for all economies to

prosper simultaneously, the Hobbesian desire for "power after power" gets in the way. So does greed. People often seek to gain distributional advantages not by being more productive but by gaining control of public policies in order to capture rents. Nevertheless, mercantilist theory has been proved bankrupt, and the institutions of liberal democracy have limited, although they have not eliminated, the success of rent-seeking. Smith and Madison would not be fully satisfied, but they would be gratified by the partially successful institutionalization of their ideas.

Together the liberalism of progress and the liberalism of fear emphasize the need for institutions. Smith's liberalism calls for institutions to promote exchange; Shklar's for institutions to control human vices and those individuals among us whose vices are most dangerous to others. For these institutions to be morally acceptable, they must rest both on humane beliefs and substantial mutual trust. The Mafia is not better than anarchy; the people who live under either find themselves impaled on one horn or the other of the governance dilemma.

Democratic theory is even more demanding. From a democratic standpoint it is not enough to have nonoppressive institutions that enforce rules. Accountability, participation, and persuasion are also essential. International institutions will probably never meet the standards of electoral accountability and participation that we expect of domestic democracies (Dahl 1999), so at best they will be low on a democratic scale. It is unfair to demand too much of them. But in the liberal-democratic tradition that I embrace, voluntary cooperation based on honest communication and rational persuasion provides the strongest guarantee of a legitimate process. In this section, I return to the issues of accountability, participation, and persuasion that I introduced earlier.

Accountability is not necessarily electoral, so it is essential to explore other forms of it if we are to increase accountability in global governance. Participation will probably continue to be largely local, so global governance implies viable forms of local self-governance. Finally, for global governance to be legitimate, global institutions must facilitate persuasion rather than coercion or reliance on sanctions as a means of influence. Here there seems to be considerable scope for improvement, so I will emphasize persuasion in the following discussion.

Accountability

The partially globalized world that I imagine would not be governed by a representative electoral democracy. States will remain important; and one state/one vote is not a democratic principle. National identities are unlikely to dissolve into the sense of a larger community that is necessary for democracy to thrive.

Accountability, however, can be indirectly linked to elections without a global representative democracy. Control by democratic states over international institutions can be exerted through chains of delegation. A complementary measure is to strengthen mechanisms

of domestic accountability of governments to their publics. Such practices can reinforce accountability insofar as transparency ensures that people within the several states can make judgments about their own governments' performance.

Nonelectoral dimensions of accountability also exist.[3] Some international regimes seek to regulate the activities of firms and of governments, although they are weaker than their domestic counterparts, and they do not meet democratic standards as well as the "best practices" domestically. Global governance, combined with modern communications technology (including technologies for linguistic translations), can begin to generate a public space in which some people communicate with one another about public policy without regard to distance. Criticism, heard and responded to in a public space, can help generate accountability. Professional standards comprise another form of nonelectoral accountability.

Finally, markets provide a third dimension of nonelectoral accountability. Since people do not bring equal wealth to the marketplace, markets are not democratic. But they do hold firms and other institutions with hard budget constraints accountable to their consumers and investors in ways that are often more rapid and effective than electoral democracy. Advocates of principle-based change have learned to use markets on issues as diverse as promotion of infant formula in poor countries, environmental protection, and labor standards.

These mechanisms of accountability exist, in fragmented ways, at the global level, but they are *disarticulated*. They do not come together in a clear pathway by which laws are enacted and implemented. Chains of delegation are long, and some of their links are hidden behind a veil of secrecy. Incentives for politicians to hold leaders of other governments accountable are lacking. Publics, professional groups, and advocacy networks can only punish leaders inconsistently. Governments, nongovernmental organizations, and firms that do not rely on brand names may be immune from market-based sanctions. In devising acceptable institutions for global governance, accountability needs to be built into the mechanisms of rule making and rule implementation.

Participation

Individual participation is essential to democratic governance. In the past, meaningful participation has only been feasible on a face-to-face basis, as in the New England town meeting, and it has been argued that, "in its deepest and richest sense, community must always remain a matter of face-to-face intercourse" (Dewey [1927] 1954, 211). Yet, the costs of communication between any two points of the world no longer depend on distance, and within 50 years we can expect the forms of such communication to change in extraordinary ways. Although it is difficult to imagine good

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[3] For a very sophisticated discussion of different forms of accountability, see Scharpf 1999, especially chapter 1.

substitutes for the multiple dimensions—verbal, visual, and tactile—by which communication occurs when people are close to one another, the potential of communications technology should not be underestimated.

More serious barriers to global democratic participation can be found in numbers and cultural diversity. Meaningful collective participation in global governance in a world of perhaps ten billion people will surely have to occur through smaller units, but these may not need to be geographically based. In the partially globalized world that I am imagining, participation will occur in the first instance among people who can understand one another, although they may be dispersed around the world in "disaporic public spheres," which Arjun Appuradai (1996, 22) calls "the crucibles of a postnational political order."

Whatever the geographical quality of the units that emerge, democratic legitimacy for such a governance system will depend on the democratic character of these smaller units of governance. It will also depend on the maintenance of sufficient autonomy and authority for those units, if participation at this level is to remain meaningful.

Persuasion and Institutions

Since the global institutions that I imagine do not have superior coercive force to that of states, the influence processes that they authorize will have to be legitimate. Legitimacy is, of course, a classic subject of political philosophy and political science (Rousseau [1762] 1978, book 1, chap. 1; see also Hobbes [1651] 1967, chaps. 17–18; Locke [1689] 1967, chap. 9; Weber [1920] 1978, 214). In the liberal tradition that I embrace, voluntary cooperation based on honest communication and rational persuasion provides the strongest guarantee of a legitimate process (Habermas 1996; Rawls 1971). To understand the potential for legitimate governance in a partially globalized world, we need to understand how institutions can facilitate rational persuasion. How do we design institutions of governance so as to increase the scope for reflection and persuasion, as opposed to force, material incentives, and fraud?

"Persuasion" means many things to many political scientists. I will define it with reference to two other processes, *bargaining* and *signalling*. In a bargaining situation, actors know their interests and interact reciprocally to seek to realize them. In a signalling situation, a set of actors communicates to an audience, seeking to make credible promises or threats (Hinich and Munger 1994). Both processes essentially involve flows of information. If successful, these flows enable actors to overcome informational asymmetries (Akerlof 1970) as well as private information (Fearon 1995) and therefore reach mutually beneficial solutions. Neither bargaining nor signalling as such involves any changes in *preferences over attributes*, that is, over the values involved in choices.

If targets of influence change their choices as a result of bargaining and signalling, they do so by recalculating

their own strategies as a result of new information they receive about the strategies of others. That is, they become aware that others will not behave as they had previously expected. In bargaining, a quid pro quo is involved; in signalling, threats and promises may be unilateral.

Persuasion, as I will use it, involves changing people's choices of alternatives *independently of their calculations about the strategies of other players*. People who are persuaded, in my sense of the word, change their minds for reasons other than a recalculation of advantageous choices in light of new information about others' behavior. They may do so because they change their preferences about the underlying attributes. They may consider new attributes during processes of choice. Or they may alter their conceptions of how attributes are linked to alternatives.

Unlike bargaining on the basis of specific reciprocity, persuasion must appeal to norms, principles, and values that are shared by participants in a conversation. Persuasion requires giving reasons for actions, reasons that go beyond assertions about power, interests, and resolve (Elster 1998; Risse 2000). Karl Deutsch (1953, 52) argued long ago that to be susceptible to persuasion, people "must already be inwardly divided in their thought," that there must be "some contradictions, actual or implied, among their habits or values." These contradictions, sharpened by discussion, may lead to reflection and even attitude change.

Persuasion is a major subject of study in social psychology (McGuire 1985; Petty and Wegener 1998). Thousands of experiments later, the essential message from this field is that, even in the laboratory, it is difficult to find strong and consistent relationships that explain attitude change. As William McGuire (1985, 304) puts it, "human motivation is sufficiently complex so that multiple and even contradictory needs may underlie any act."

What we do know about persuasion in politics indicates that it consistently involves various degrees of agenda control and manipulation. Rational or open persuasion, which occurs when people change their choices of alternatives voluntarily under conditions of frank communication, is an interesting ideal type but does not describe many major political processes. Yet, the ideal is important, since it is so central to the liberal-democratic vision of politics. Indeed, thinking about persuasion helps restate the central normative question of this address: How can institutions of governance be designed so as to increase the scope for reflection, and therefore persuasion, as opposed to force, material incentives, and fraud?

If governance were exercised only by those with direct stakes in issues, such a question might have no answer. Actors could be expected to use their resources and their guile to achieve their desired objectives. And the institutions would themselves "inherit" the inequalities prevalent in the societies that produced them, as Rousseau and many successors have pointed out (Aldrich 1993; Riker 1980). Indeed, choices of electoral institutions can often be traced to the policy, party, and

personal preferences of the politicians who created them (Bawn 1993; Remington and Smith 1996).

One feature of both democratic governance and contemporary international institutions, however, is that decision making is not limited to the parties to a dispute. On the contrary, actors without a direct stake in the issues under consideration may play important roles, as members of the mass public in democracies and legislators often do on issues arising for decision through voting. In general, the legalization of rules—domestically, and more recently in international politics—requires the formation of durable rules that apply to classes of cases and puts interpretation and rule-application into the hands of third parties, whose authority depends on maintaining a reputation for impartiality (Goldstein et al. 2000). Legalization also increases the role of precedents. Precedents matter, not because loopholes are impossible to find or because they cannot be overruled, but because the status quo will prevail in the absence of a decision to overturn it.

Some third parties will have calculable interests that closely parallel those of the principal disputants or advocates. Others may have strongly held beliefs that determine their positions. Some may accept side-payments or succumb to coercive pressure. But still others may lack both intense beliefs and direct stakes in the outcome. Legal requirements or internalized normative standards may inhibit them from accepting inducements for their votes. Even more important, uncertainty about the effect of future rules may make it difficult for them to calculate their own interests. Rule makers face a peculiar form of "winner's curse": They risk constructing durable rules that suit them in the immediate instance but will operate against their interests in the unknown future.

Insofar as uncertainty is high, actors face a situation similar to one covered by a "veil of ignorance" (Rawls 1971). In game-theoretic terms, the actors may still have preferences over outcomes, but these preferences over outcomes do not directly imply preferences over strategies, since actors do not know their future situations. In experiments, introducing a veil of ignorance in prisoners' dilemma games without communication induces a dramatic increase in the willingness of subjects to cooperate (Frohlich and Oppenheimer 1996). It is reasonable to hypothesize that under conditions of uncertainty in the real world, the chain of "inheritability" will be broken, and actors' preferences about future outcomes will not dictate their choices of alternatives in the present.

Under conditions of authority for impartial third parties, or high uncertainty about future interests, opportunities for persuasion are likely to appear, even if everyone is a rational egoist. Egoists have a long-term interest in rules that will correspond to an acceptable general principle, since they may be subject to these rules in the future. Various principles could be chosen—expected utility maximization, the maximum principle (Rawls 1971, 152), minimax regret (Riker 1996), or utility maximization subject to a floor minimum, which is the prevailing choice in laboratory experiments (Frohlich and Oppenheimer 1992). Insofar as the consequences and functions of institutions are not seriously degraded, institutions that encourage reflection and persuasion are normatively desirable and should be fostered.

CONCLUSION

I have asked how we can overcome the governance dilemma on a global scale. That is, how can we gain benefits from institutions without becoming their victims? How can we help design institutions for a partially globalized world that perform valuable functions while respecting democratic values? And how can we foster beliefs that maintain benign institutions? My answers are drawn, mostly implicitly, from various schools of work in political science.

From rational-choice institutionalism, we learn both the value of institutions and the need for incentives for institutional innovation. These incentives imply privileges for the elite, which have troubling implications for popular control.

From a variety of perspectives, including game theory, the study of political culture, and work on the role that ideas play in politics, we learn how important beliefs are in reaching equilibrium solutions, and how institutionalized beliefs structure situations of political choice.

From traditional political theory, we are reminded of the importance of normative beliefs for the practice of politics—and for institutions. It is not sufficient to create institutions that are effective; they must be accompanied by beliefs that respect and foster human freedom.

From historical institutionalism and political sociology, we understand how values and norms operate in society. Without such understanding, we can neither comprehend the varying expectations on which people rationally act nor design institutions based on normative views. We abdicate our responsibility if we simply assume material self-interest, as economists are wont to do.

From democratic theory, we discover the crucial roles of accountability, participation, and especially persuasion in creating legitimate political institutions.

These lessons are in tension with one another. Institutional stability is often at odds with innovation and may conflict with accountability. Protection against oppression can conflict with energetic governance; a practical reliance on self-interest can conflict with the desire to expand the role of persuasion and reflection. Governance, however, is about reconciling tensions; it is Max Weber's ([1919] 1946) "boring of hard boards."

As students of political philosophy, our objective should be to help our students, colleagues, and the broader public understand both the necessity for governance in a partially globalized world and the principles that would make such governance legitimate. As positive political scientists, we need to continue to analyze the conditions under which different forms and levels of governance are feasible. As practitioners of a policy science, we need to offer advice about how

institutions for global governance should be constituted. This advice must be realistic, not romantic. We must begin with real people, not some mythological beings of higher moral capability. But we need also to recognize, and seek to expand, the scope for reflection and the normative principles that reflective individuals may espouse. We should seek to design institutions so that persuasion, not merely interests and bargaining, plays an important role.

The stakes in the mission I propose are high, for the world and for political science. If global institutions are designed well, they will promote human welfare. But if we bungle the job, the results could be disastrous. Either oppression or ineptitude would likely lead to conflict and a renewed fragmentation of global politics. Effective and humane global governance arrangements are not inevitable. They will depend on human effort and on deep thinking about politics.

As we face globalization, our challenge resembles that of the founders of this country: how to design working institutions for a polity of unprecedented size and diversity. Only if we rise to that challenge will we be doing our part to ensure Lincoln's "rebirth of freedom" on a world—and human—scale.

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Back to Kant: Reinterpreting the Democratic Peace as a Macrohistorical Learning Process

LARS-ERIK CEDERMAN *University of California, Los Angeles*

*T*he contemporary international relations literature links the democratic peace hypothesis to Kant's famous peace plan. Yet, whether attempting to prove or disprove the hypothesis, most quantitative studies have lost sight of important dimensions of the Kantian vision. I reinterpret the democratic peace as a dynamic and dialectical learning process. In order to assess the dynamic dimension of this process (while controlling for exogenous dialectical reversals), I rely on quantitative evidence drawn from popular data sets. In conformance with the Kantian perspective, the conflict propensities among democracies exhibit a steadily falling trend since the nineteenth century. Yet, in partial opposition to Kant's expectations, other dyads also experience a significant, although weaker, pacifying trend. A series of tests shows that these findings are robust to epochal effects, various control variables, and "maturity effects" measuring the age of democratic dyads.*

After more than a decade of intense scholarly debate, the claim that democratic states hardly ever fight each other remains contested. Although most analysts appear to support the democratic peace hypothesis, a small but determined minority of realist scholars does not accept it. Despite this fundamental disagreement, both camps agree that Immanuel Kant laid the intellectual foundation of the hypothesis in the late eighteenth century.[1] Whether aiming at corroboration or refutation, most contemporary scholars appear to believe that they are operationalizing and testing some version of the Kantian thesis. Yet, although some analysts have come closer to Kant's original conjecture by embracing more of its analytical dimensions, neither side of the debate succeeds in fully capturing the dynamic and dialectical logic of the process.

Does it really matter whether Kant has been misunderstood? I argue that it does. Rather than engage in exegesis for its own sake, I maintain that only partial representations are responsible for many of the empirical and theoretical disputes haunting the current debate. By squeezing Kant's fundamentally dynamic argument into a Procrustean bed of static regression equations, today's researchers typically expect the "Kantian effect" to be time-invariant.

It is worth revisiting Kant's original peace plan.

[1] It has been suggested that other authorities deserve credit for the formulation. Gates, Knutsen, and Moses (1996) assert that Kant drew on Rousseau. Yet, among the classical authors, Kant's treatment remains the most influential (see Ray 1995).

Instead of viewing the democratic peace argument as merely a "second-image" claim about the pacific effect of a particular regime type on dyadic relations, I propose a Kantian reinterpretation of the democratic peace hypothesis as a dynamic and dialectical learning process. It is dynamic rather than stationary in that states alter their behavior as a consequence of taking past experience into account. Thus, the effect of democracy changes over time. The process is dialectical because catastrophic reversals, such as world wars, drive home the point that there is little choice but to eliminate violence in interstate relations.

Once recast in these terms, the democratic peace argument can be tested in a way that reflects at least some of the conjecture's subtlety. Indeed, Kant ([1784] 1970a, 50) anticipated that, one day, it would become feasible to evaluate the empirical validity of his postulates:

> The real test is whether experience can discover anything to indicate a purposeful natural process of this kind. In my opinion, it can discover *a little*; for this cycle of events seems to take so long a time to complete, that the small part of it traversed by mankind up till now does not allow us to determine with certainty the shape of the whole cycle, and the relation of its parts to the whole.

Writing in the late eighteenth century, Kant was in a less privileged position than social scientists are today. Yet, most of his predictions have had an almost uncanny tendency to be borne out by history, despite the scarcity of information on which he based his theorizing.

In this article, I focus primarily on the dynamic dimension of the famous peace plan and merely correct for exogenously given dialectical disturbances. The empirical part draws on data from standard quantitative sources that have so far been analyzed in exclusively static terms. The evidence strongly supports the core of the Kantian learning hypothesis. Since the first half of the nineteenth century, democratic states appear to be much better learners in their mutual relations than when faced with other states, or than nondemocratic states in their own interactions. There is no support, however, for the (possibly Kantian) view

that learning can only take place among democracies. Indeed, some learning appears to spill over into other relations.

Additional tests indicate that these results hold whether we control for historical period or certain liberal or geopolitical variables, and whether we limit the scope to the post-1945 period. In the latter case, it is even possible to find evidence for a "maturity effect" that distinguishes old democratic dyads from newer ones. These results cast doubt on realist attempts to explain away the democratic peace as a merely geopolitical side-effect of the Cold War.

The article is organized as follows. In the following section I revisit Kant's peace plan. I then criticize the modern democratic peace literature in the light of Kant's writings. Next, some operational hypotheses are derived, and a basic statistical model is presented. The findings are then exposed to a series of robustness checks. In conclusion I sum up the results and discuss future empirical and theoretical elaborations.

KANT'S PEACE CONJECTURE

In his famous essay "Perpetual Peace," Kant ([1795] 1970b) sets out the main argument in three "definitive articles." These should be seen as joint conditions for truly lasting peace. The first condition requires states to be republics, which in today's terminology could be translated as liberal democracy.[2] The second calls for a confederation of free states to form in order to preserve peace. The third definitive article contends that a limited sense of world citizenship is needed to secure the two first conditions.[3]

By arguing that only transcendence of power politics can deliver stable peace, Kant parts company with generations of realist thinkers who have placed their hope in the balance of power as the best way to prevent war. The three definitive articles, far from being a mere utopian shopping list, are bundled together as a hypothetical development. Together these arguments anticipate a complex diffusion process whereby the rule of law and norms of peaceful change can spread both up from the domestic realm and down from interstate relations. Keenly aware of the constraints imposed by power politics, Kant proposes a sophisticated and nondeterministic account of how and why world history is likely, although not certain, to converge slowly on the three conditions.

To grasp the logic of these arguments, it is necessary to go beyond "Perpetual Peace." In particular, "Idea for a Universal History with Cosmopolitan Purpose" introduces crucial mechanisms without which it is

impossible to do justice to the Kantian argument (Kant [1784] 1970a). Kant's reasoning depends on the idea of progress through learning. Ultimately, peace will emanate from individuals' realization that war is both destructive and immoral. But before this lesson can be learned, conflict is needed to drive home the point. Moreover, this learning process can only develop in a very specific context. Liberal democracy allows individuals to realize their full "civilized" potential. At the interstate level, democratization requires a peaceful confederation of democracies (*Staatenbund*) to defend these state-level achievements. In other words, individual learning presupposes collective learning, both at the state level and within the entire international system.

Because of its highly interconnected nature, the argument appears unrealistic unless all conditions are satisfied simultaneously. At first blush, the peace plan indeed seems to suffer from a chicken-or-egg dilemma (Hurrell 1990; see also Waltz 1962), but this is a misunderstanding. As Huntley (1996, 50) remarks, "the problem only emerges when achievement of the rule of law is conceived as a chronologically discrete event, rather than, as for Kant, an unending process taking place at both [the domestic and international] 'levels' simultaneously."

As a way to transcend the security dilemma, Kant proposed a set of mechanisms that, together, generate peace without resorting to teleological speculation. After ruling out revolution as a means of democratization, Kant hypothesized that, in the long run, warfare itself will force authoritarian rulers to liberalize their states, for "civic freedom cannot now be interfered with without . . . a decline of the power of the state in its foreign relations. Therefore this freedom is gradually extended. If one obstructs the citizen in seeking his welfare in any way he chooses, . . . one also hampers the vitality of all business and the strength of the whole [state]" (Kant [1784] 1970a, 50). This process implies a gradual percolation of democratic norms from the citizens up to the political leaders (p. 46). But democracy also spreads from the top down, which accelerates individual learning and, thus, creates a more solid foundation for freedom: "It is only through [a good political constitution] that the people can be expected to attain a good level of moral culture" (Kant [1795] 1970b, 113).

At the interstate level, the second definitive article outlines a diffusion process responsible for the emergence of the peaceful confederation:

> For if by good fortune one powerful and enlightened nation can form a republic (which is by its nature inclined to seek perpetual peace), this will provide a focal point for federal association among other states. These will join up with the first one, thus securing the freedom of each state in accordance with the idea of international right, and the whole will gradually spread further and further by a series of alliances of this kind (Kant [1795] 1970b, 104).

To see why Kant thought that interdemocratic peace will materialize, it is helpful to examine the first definitive article. In an often-cited argument, Kant suggests that democratically elected leaders will have

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[2] But see Brown 1992; Gates, Knutsen, and Moses 1996; and Owen 1997, 15–7. These authors make much of Kant's rejection of democracy. In Kant's lifetime, "republic" was associated with direct democracy in the classical Greek sense, but today's liberal and representative democracies come very close to what Kant meant by republican polities. See Chan 1997, 64, and Huntley 1996, 48.

[3] For general introductions to and translations of Kant's political philosophy, see, e.g., Friedrich 1949; Hinsley 1963; and Reiss 1970. The original texts can be found in Kant 1968. In addition, there are several cogent discussions specific to international relations: see Doyle 1983a, 1983b; Huntley 1996; Hurrell 1990; and Waltz 1962.

to take their peoples' pacific preferences into consideration before going to war (Kant [1795] 1970b, 100). Yet, the effect of democracy is not limited to this simple cost-benefit mechanism. Kant sees no reason why the upward spread of norms has to stop at the democratic state's borders. Once the pathway of normative progress is opened, the rule of law will creep into interstate relations, and this will obviate, or at least reduce, the need to rely on threats and enforcement (Kant [1784] 1970a, 51).

The external dimension of the peaceful confederation is also provided for in Kant's argument. The pacific league aims at abolishing all wars, which makes it more than a temporary defensive alliance, but it will have to serve as a protective mechanism when the confederation is exposed to external challenges from nondemocratic states: "It is necessary to establish a federation of peoples in accordance with the idea of the original social contract, so that states will protect one another against external aggression while refraining from interference in one another's internal disagreements" (Kant [1797] 1970c, 165). Although Kant opposed violent means to establish democratic rule, he thought it entirely legitimate that the democratic confederation be defended through armed conflict if necessary.

As a complement to the first two definitive articles, the third one rests on self-interested mechanisms rather than on teleology. At least to begin with, the normative evolution of cosmopolitan law that secures transnational "hospitality" follows from economic interdependence: "For the spirit of commerce sooner or later takes hold of every people, and it cannot exist side by side with war .... Thus states find themselves compelled to promote the noble cause of peace, though not exactly from motives of morality" (Kant [1795] 1970b, 114; see also Kant [1784] 1970a).

A KANTIAN CRITIQUE OF THE DEMOCRATIC PEACE LITERATURE

In recent years there has been a surge of interest in the relationship between war and democracy. Following Michael Doyle's (1983a, 1983b) pioneering theoretical efforts (see also Rummel 1979), countless quantitative studies (for a review, see Chan 1997), game-theoretic interpretations (e.g., Bueno de Mesquita et al. 1999), and case studies (e.g., Elman 1997) have appeared. Although considerable controversy continues to surround the operationalization of the key variables, analysts on both sides of the liberal-realist divide interpret the democratic peace hypothesis as a relational statement that implies the (almost total) absence of warfare between democracies. Nearly all studies frame the hypothesis in dyadic terms (Chan 1997, 62–5), although some contend that there is also a monadic effect (e.g., Rousseau et al. 1996) and a systemic component (e.g., Doyle 1997; Gleditsch and Hegre 1997; Huntley 1996).

The first quantitative studies homed in on regime type as the only component of the Kantian peace, but methodologically self-conscious scholars have recently expanded the analytical scope well beyond this simple, monocausal hypothesis. For example, Bruce Russett and his colleagues have explicitly proposed and tested a multicausal interpretation of the democratic peace. Oneal and Russett (1997, 268) reevaluate Kant's message as saying that "peace can be built on a tripod of complementary influences: republican constitutions (i.e., representative democracy), international law and organization, and 'cosmopolitan law' (economic interdependence)" (see also Oneal, Oneal, and Russett 1996). This promising research program has been extended to include direct statistical analysis of the "third organizational leg" (Oneal and Russett 1998; Russett, Oneal, and Davis 1998).

Despite these theoretical advances, international relations scholars typically refer to Kantian variables rather than to Kantian theory. As they admit themselves, what is needed is a process theory that articulates the dynamics and dialectics of Kant's peace postulates. I shall compare the Kantian process to the modern literature with respect of each of these dimensions.

Dynamics

Both proponents and critics appear to agree about the law-like status of the democratic peace hypothesis. On the liberal side of the debate, Jack Levy (1988, 661–2) celebrates it as "the closest thing we have to an empirical law in the study of international relations" (see also Doyle 1996, 364). In practice, this boils down to testing the claim wherever democracy can be said to be present in the historical record. Some analysts have even gone so far as to apply it to the city-states of classical Greece and other premodern societies (e.g., Russett 1993; cf. Chan 1997, 69), but most studies are limited to the period, or parts of it, covered by the Correlates of War (COW) database (Small and Singer 1982), starting in 1816.

Not even Doyle (1983a, 1983b), whose careful analysis traces the systemic dimension of Kant's process, does full justice to its dynamic quality. One must agree with Huntley (1996, 64) that Doyle's "depiction insufficiently recognizes the generative capacity of anarchy that the liberal peace demonstrates." According to this critique, Doyle's argument implies that the democratic peace "has revealed no new, potentially transformational manifestations of anarchy's effects on international politics. Rather, liberal states have overcome a static structural condition" (p. 64).

Realist skeptics also tend to view the democratic peace as a static effect. Taking Levy's observation about empirical law as his starting point, Layne (1994) attempts to refute this putative law by investigating "near misses." Gowa (1999, 67) is even more explicit: "The democratic-peace hypothesis predicts that the war and lower-level dispute rates of democratic dyads should be uniformly lower than are those of their non-democratic counterparts. Thus, the finding that relative dyadic dispute rates vary across time is inconsistent with it."

In a rare exception to the dominant static perspec-

tive in the quantitative literature, Gaubatz (1996) compares Kant's predictions with the historical record and claims that they lack support. His somewhat mechanical measures could be criticized, but his study is refreshing because of its graphical presentation of statistical trends. Another attempt to rearticulate the diachronic logic of Kant's peace plan was made by Modelski (1990), who refers explicitly to both evolution and learning (see also Modelski 1991). Yet, his cyclic argument appears to have little to do with Kant's open-ended evolutionary theme.[4]

Since learning is a, notoriously tricky concept to define (Levy 1994), it is necessary to make a brief conceptual detour. Learning can be seen as a special case of social evolution (which is not to be confused with its biological counterpart). Although Kant's work antedates Darwin's conceptual breakthrough, it anticipates modern evolutionary theory (Modelski 1990). In brief, evolutionary processes presuppose four ingredients: variation, selection, retention, and a population of units on which these mechanisms can act (Campbell 1969; Kahler 1999; Nelson 1995).

Learning, then, can be defined as a particular type of cultural-evolutionary process in which actors use inferences drawn from their own (or vicariously experienced) environmental variation in an attempt to select more effective cognitive constructs for future decision making (cf. Breslauder and Tetlock 1991; Levy 1994; Reiter 1996, 19–20). Depending on whether learning is primarily rational or extrarational, these constructs are either interest-based or rule-based. Whereas rational (interest-based) learning presupposes an adjustment of beliefs and/or preferences, extrarational (rule-based) learning pertains to normative processes that become internalized as a part of the choice set itself (Elster 1989; Ullmann-Margalit 1977).

Both individuals and organizations can learn. Unlike individual learning, the collective counterpart does not necessarily depend on, but may entail, thought processes in human brains. Organizations also benefit from learning mechanisms that function through turnover of subunits or structural transformations of the organization itself (Hedberg 1981; Levitt and March 1988).

Kant refers to both natural and cultural evolution, the latter of which implies not only individual but also organizational learning. His peace process relies on natural selection and simpler learning, especially in its initial phases, followed by more complex modes of learning. For example, Kant ([1784] 1970a) assumes democracies to be more effective at fighting wars, which facilitates their ecological survival in natural-selection terms. Moreover, the cost-benefit argument for the democratic peace represents the first step of a normative learning process.

Clearly, Kant's notion of learning cannot be reduced to purely instrumental rationality, since its later phases feature rule-based dynamics that modify the feasible

set of options. The Kantian logic goes well beyond the individual level, for his theory crucially singles out liberal democracy as the most fertile soil for the pacific seed: "Kant does not accept the naive liberal assumption that the 'people' are always peaceful or virtuous. Progress towards perpetual peace is ultimately dependent on the moral progress of individuals. Yet such progress in turn can only come about within a good political constitution" (Hurrell 1990, 196).

The state should be seen as more than a passive environment within which collective learning may take place. Indeed, Kant ([1784] 1970a, 49) suggests,

> as long as states apply all their resources to their vain and violent schemes of expansion, thus incessantly obstructing the slow and laborious efforts of their citizens to cultivate their minds, and even deprive them of all support in these efforts, no [civilizing] progress can be expected. For a long internal process of careful work on the .part of each commonwealth is necessary for the education of its citizens.

As we have seen, the peaceful transformation of human affairs crucially depends on the interstate context as well. The second definitive article outlines the democratic confederation as the environment within which states are socialized into curbing their aggressive tendencies (Kant [1797] 1970c). Since nondemocratic states are structurally handicapped to experience moral learning, either at the elite or popular level, swift pacific progress in interstate relations requires that all parties be democracies: "The socializing influence is produced by the compelling examples of states expanding the *rule of law* in their *mutual relations* with increasing success" (Huntley 1996, 58, emphasis in original). The last step, therefore, implies that democratic states learn not only internally, as individual organizations, but also externally in their relations with other democracies.

Dialectics

Since most liberals interpret the democratic peace hypothesis as a universally applicable causal law, they are vulnerable to realist challenges that highlight presumed anomalous cases at any point in history. Even a small number of conflictual democratic dyads threatens the liberal position, which explains why case-focused controversies have been so intense (Elman 1997, 44; Ray 1995).

Given the assumption of time-invariance, it is hardly surprising that insufficient attention has been given to the historical distribution of the alleged exceptions. And if an historical asymmetry is noted, few if any analysts have followed up this observation in building and testing theories. Most of these recent debates concern nineteenth-century cases, such as the British-American wars, the Fashoda crisis, the Spanish-American War, and early-twentieth-century conflicts, such as the outbreak of World War I (e.g., Layne 1994; Oren 1995).

Liberals usually are more mindful about tracing the temporal sequence of cases than are realists. For example, Russett (1993, 20) notices that "almost all of

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[4] Recently, more flexible dynamic and spatial inference methods have been employed (see Gleditsch and Ward 2000; McLaughlin, Gates, and Hegre 1999).

the few near misses are in the nineteenth century" (see also Ray 1995, 125). Nevertheless, this realization does not seem to inform empirical works that typically treat the democratic peace as if it applies universally.[5]

In sum, the common adherence to induction and nomological causation often obscures the possibility that an "anomaly" may be an early instance of failed learning or perhaps even a dialectical reversal. Such an interpretation might seem to make the democratic peace unfalsifiable, but what matters here is the macrohistorical trend rather than short-term disturbances. The *longue durée* only becomes visible by considering the temporal pattern of data. By contrast, isolated cases and static regressions are particularly sensitive to historical myopia.

In fact, Kant warns against interpreting sudden reversals as a sign that peace will not materialize in the long run. In his account, it is paradoxically violent conflict that drives the collective learning process:

> Wars, tense and unremitting preparations and the resultant distress which every state must eventually feel within itself, even in the midst of peace—these are the means by which nature drives nations to make initially imperfect attempts, but finally, after many devastations, upheavals and even complete inner exhaustion of their powers, to take the step which reason could have suggested to them even without so many sad experiences—that of abandoning their lawless state of savagery (Kant [1784] 1970a, 47).

The dialectical nature of this explanation expresses itself through linked mechanisms operating at two levels. At first, the suffering is borne by the populations, whose war-weariness tends to increase as warfare grows more destructive. Then, increasingly desperate to extract more resources from their societies for bellicose purposes, political leaders adapt to intensified warfare by reluctantly democratizing state structures. This democratization inadvertently liberates the learning process, thus reinforcing the dialectical feedback loop (Hurrell 1990, 197).

As opposed to Kant's conjecture of an inherently dynamic process, contemporary international relations research has by and large lost track of the diachronic qualities. Although liberals and realists agree on the democratic and dyadic dimensions of the hypothesis, most empirical studies miss its dynamic and dialectical dimensions. To fill this gap, I will evaluate a simplified model that traces Kant's learning effect.

A MODEL OF KANT'S COLLECTIVE LEARNING PROCESS

Despite his penchant for abstract reasoning, Kant firmly believed that his predictions could in principle be assessed empirically with the benefit of hindsight, and doing so would require an aggregational approach: "History is concerned with giving an account of [human actions], no matter how deeply concealed their causes may be, and it allows us to hope that, if it examines the free exercise of the human will *on a large scale*, it will be

able to discover a regular progression among freely willed actions" (Kant [1784] 1970a, 41). In justifying this statistical view, he explains that "although we are too short-sighted to perceive the hidden mechanism of nature's scheme, this idea may yet serve as a guide to us in representing an otherwise planless *aggregate* of human actions as conforming, at least when considered as a whole, to a *system*" (p. 52).

What, then, are the observable consequences of the Kantian scenario? First, learning implies behavioral modification over time. Second, at least in the long run, there should be a differentiation between interdemocratic relations and all other exchanges. Furthermore, the dialectical nature of the process implies that reversals may interrupt the trend toward peace. If Kant's speculation holds up, then conflict frequencies among democracies will decrease gradually, as opposed to the more slowly evolving conflict patterns associated with other types of relations.

To assess these empirical implications, I rely on systematic conflict data covering most of the period since Kant wrote his peace plan in the late eighteenth century. Although Kant's theory encompasses a systemic dimension, I follow the vast majority of available quantitative studies by selecting dyad-years based on interstate relations as the unit of analysis (though see Gleditsch and Hegre 1997). Since the data do not contain any democratic dyads before 1837, the sample starts with that year and runs through 1992, which is the last year for which all measures are available.[6] Interaction opportunities clearly influence states' decisions, so this study is limited to "risky dyads," featuring at least one great power or geographically contiguous states (see Maoz and Russett 1992).

The dependent variable measures military conflict in general rather than war, since the latter occur too rarely to provide a basis for temporal trends (Farber and Gowa 1995; Gowa 1999, 48f). Fortunately, Kant's normative arguments apply to lower-level conflict as well, since prohibitive norms are expected to form a security community that ultimately rules out violence of any sort.[7] I follow the quantitative literature in employing militarized interstate disputes (MIDs) drawn from the Correlates of War (COW) Project. A MID involves display of weapons as well as threats or actual use of military force, even if no deaths result; 1,000 battle deaths are required to qualify as a war in the COW sense (see Gochman and Maoz 1984).

Data on regime type were obtained from the Polity III data set (Jaggers and Gurr 1996). Jaggers and Gurr (1995) provide a composite measure of democracy that reflects the constraints on political participation, leader

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[5] But see Owen (1994, 1997), who contends that the very meaning of democracy has evolved over time.

[6] This censoring of the sample may seem to introduce selection bias, but my aim is not to estimate universal laws; rather, I want to investigate a specific macrohistorical process.

[7] In a short-run perspective, however, there may be an important difference. Some rationalistic deterrence theories expect increased dispute activity to accompany decreased warfare (cf. Chan 1997, 66–8). Furthermore, Kant's ([1784] 1970a) dialectical war-based argument requires warlike conflict rather than lower-level violence because regime change is unlikely to follow from mere disputes (cf. McLaughlin, Gates, and Hegre 1999).

FIGURE 1.   Dispute Probabilities as Moving Averages, 1837–1992

Dispute Probability
per Dyad-Year

Note: The curves represent 20-year moving averages of the probability of militarized interstate disputes per dyad-year for each category of risky dyads.
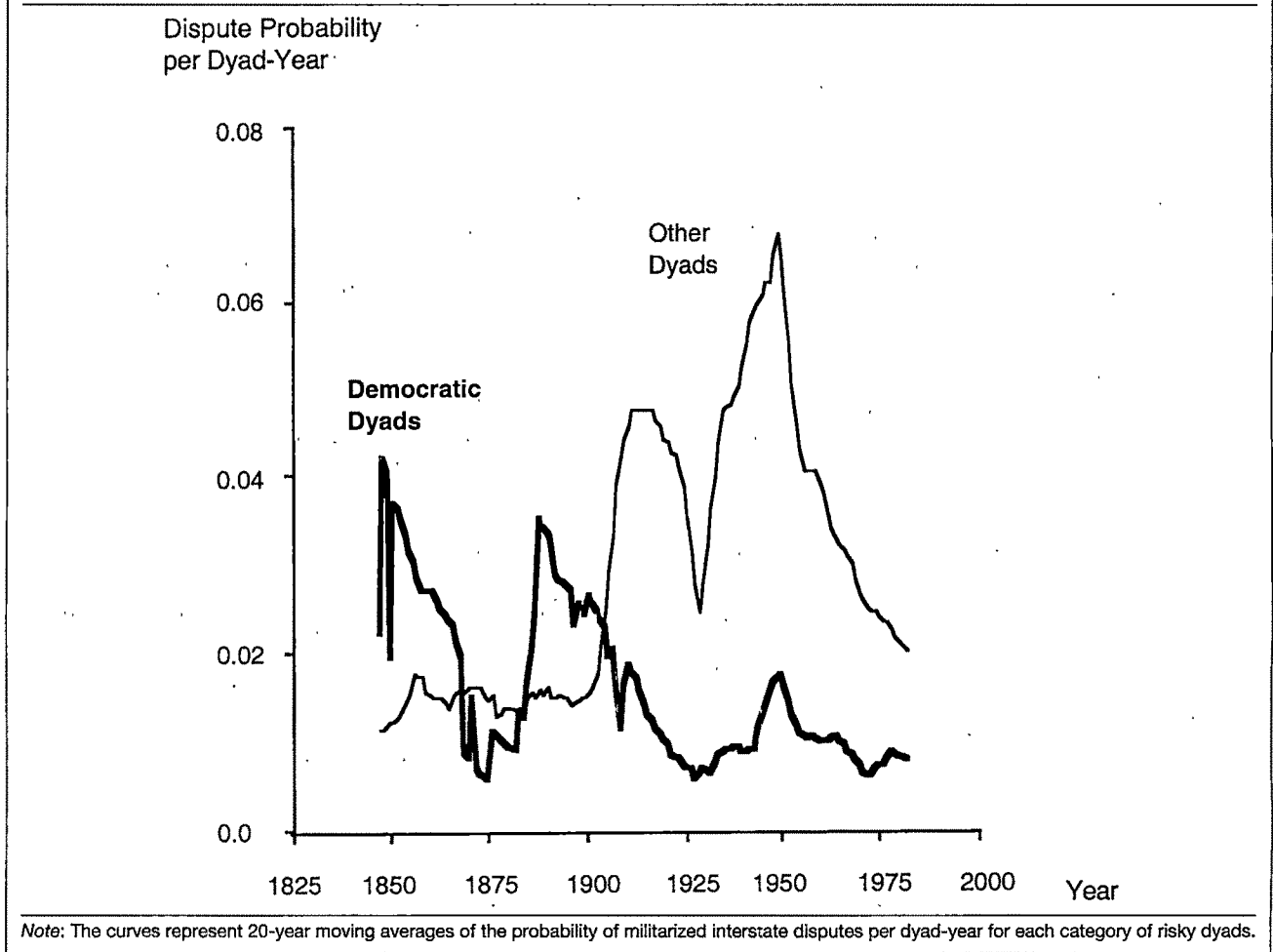
selection, and other institutional constraints. To simplify the dynamic interpretation and to make my analysis comparable with recent studies (e.g., Faber and Gowa 1995; Gowa 1999), I code democracy as any dyad in which both states attain at least a six on Gurr's democracy scale.[8]

Before applying regression analysis to the statistical material, it is instructive to explore the descriptive statistics. Figure 1 presents a moving-average sweep of the dispute propensity per dyad-year over 20-year periods. The bold curve traces the probability for democratic dyads, and the thin curve indicates the trend for all other dyads. The residual category includes purely nondemocratic dyads and mixed ones comprising one democracy and one authoritarian state. With a few temporary exceptions, the picture reveals a descending pattern for the democracies' interactions. The conflict propensity among the other dyads, in contrast, appears to be rising, with peaks at the time of the world wars, although most of the Cold War period is characterized by a dramatic downward slope. From

the beginning of the twentieth century, the democratic curve is consistently lower than that of the other dyads.

At face value, these initial findings appear to confirm a Kantian interpretation of the democratic peace. The democracies seem to be better learners in their mutual relations than other types of dyads. The high starting point of the democratic curve is more puzzling from a Kantian perspective, however, and I will return to this issue. More important, the graph casts doubt on the realist thesis that nuclear weapons, or some other attribute of the Cold War, drive the process, because the democratic conflict rates began to descend after World War I (e.g., Gowa 1999). These findings are suggestive, but it would be a mistake to put too much weight on them at this point. After all, they stem from descriptive statistics, so no safe inference can be drawn from them, and they have not been exposed to statistical control. The rest of this section introduces a basic model that traces the dynamics of conflict, followed by a separate section that evaluates the causal robustness of the initial results.

How can one account for the temporal trends in Figure 1? Is it possible to formalize the temporal dependence along Kantian lines? If the dialectical theme is ignored for the moment, a simplified reading

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[8] Gurr's autocracy measure can be safely omitted because of its strong correlation (−0.83) with the democracy index (Gowa 1999, 50). I do not use multinomial indices since they complicate comparisons across learning curves (cf. Maoz and Russett 1993).

of the peace plan could take reinforcement learning as the starting point. That is, actors learn primarily from positive experiences.[9] In our context, the argument assumes that pacific relations generate benefits in terms of wealth and security that gradually will be factored into the .decision-making calculus of those states capable of learning. In the end, these changes will also trigger a normative process that reconfigures the choice set, such that violence becomes unthinkable (see Deutsch et al. 1959; Kaysen 1990). Because of my macro focus, it is impossible to tell one effect from the other, so this puzzle must be left to future research (see Levy 1994, 304–6). Of immediate importance, however, is whether the conflict propensity will fall over time conditional on the learning capacity of the actor-pair in question.

Because Kantian learning amounts to a fundamentally reciprocal experience in confidence-building, the process depends crucially on both sides' learning capacity. A breach of trust can easily destroy the gains of long cooperation. This type of organizational learning features "transactive memory" (Wegner 1986), since it relies on knowledge and norms embedded in the actors' supraindividual routines and habits (Argote 1999, 82–6; Hedberg 1981, 6). If institutions, customs, and standard operating procedures are the repositories of conflict-reducing techniques, then the process will proceed at the speed of the slowest learner. According to this logic, dyadic learning will operate rapidly only when both states are democracies, and it will occur much more slowly, if at all, in other cases.

This preliminary account says little about the functional form of the dyadic learning curves, but there is a rich literature to build upon, although most of it applies to very different phenomena, such as improvements in production methods (see Argote 1999) or rats finding their way through mazes (see Macy 1990). Whether individual or collective, reinforcement learning can be assumed to follow a straightforward pattern, at least as a theoretical starting point. In their classical stochastic model, Bush and Mosteller (1955) postulated that the probability of choosing the reinforced behavior p_t at time t follows the simple recursive rule:

$$p_{t+1} = p_t + c(1 - p_t),   \text{(1)}$$

where c is a positive constant less than one. Thus, the reinforcement effect decays asymptotically as the propensity approaches unity. Using this equation, Macy (1990, 816) interprets p_t as the probability that a set of social actors will contribute to a public good (i.e., refrain from free-riding). The solving of the security dilemma by democratic states can be seen as a special case of this general situation.

Adapting this simple formula yields an even simpler expression. Assuming that $\pi_t = 1 - p_t$ is the probability of two states engaging in conflict, reinforced behavior pertains to the opposite course of action, that is, refraining from conflict, with probability $p_t = 1 - \pi_t$.[10]

After some algebraic manipulations, substitution in equation 1 yields:

$$\pi_{t+1} = (1 - c)\pi_t.$$

Assuming that the initial conflict propensity is π_0, the explicit relationship becomes $\pi_t = (1 - c)^t \pi_0$, which can be written as:

$$\pi_t = \pi_0 \exp(kt),   \text{(2)}$$

where $k = \ln(1 - c)$ is another negative constant. This formula expects learning to reduce the initial conflict propensity π_0 according to an exponential decay process with a negative coefficient k. As t goes to infinity, the conflict rate asymptotically approaches zero.

In addition to the suggestive shape of Figure 1, there is ample evidence from other areas that confirms the generality of this particular functional form. In economics, much research has centered on efficiency gains within single firms (e.g., Alchian 1963). Rather than being focused on an isolated organization's production of an easily quantifiable good, the Kantian process corresponds more closely to environmental learning that features interorganizational diffusion processes (Levitt and March 1988). Such learning includes knowledge transfer through personal contacts, observation, documentation, and embeddedness in social networks (Argote 1993, 41; 1999, chap. 1).

The crux is that theories of this kind are particularly hard to operationalize because of the fuzzy nature of "knowledge," as opposed to such easily measurable variables as cumulated output. Therefore, economists often rely on calendar time as a proxy for long-term knowledge acquisition (e.g., Rapping 1965; cf. Argote 1999, 15). Although other solutions are possible, my analysis starts with this simple form of temporal dependence. The next section also explores the influence of "dyadic" time.

Having justified the negative exponential form in equation 2 as my point of departure, I now turn to the problem of estimation. Assuming that t can be measured as calendar year Y and that the dummy variable D stands for democracy, it is straightforward to estimate the curve by using a standard logit model:

$$\Pr(X,\beta) \frac{1}{1+e^{-X\beta}},   \text{(3)}$$

where

$$X\beta = \beta_0 + \beta_1 D + \beta_2 D \times Y + \beta_3(1-D)Y.   \text{(4)}$$

For small probabilities $\Pr(X,\beta)$ we can assume that (refer to Figure 1)

$$\Pr(X,\beta)/\{1 - \Pr(X,\beta)\} \approx \Pr(X,\beta),$$

which implies that $\Pr(X,\beta) \approx \exp(X\beta)$.[11]

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[9] Note, however, that social psychologists claim people learn more effectively from failures than from successes (Levy 1994, 304).

[10] This formalization does not attempt to disaggregate the interde-

pendencies of dyadic learning. An extended model could draw on Signorino's (1999) creative attempt to marry a strategic perspective with statistical validation.

[11] Similarly, Beck, Katz, and Tucker (1998, 1268) show that the logit and cloglog transforms are "almost identical" for event probabilities below 0.25 and "extremely similar" for probabilities below 0.5.

TABLE 1. Logit Analysis of Dispute Propensity

Variable	Static Model (All Risky Dyads)			Dynamic Model (All Risky Dyads)		
	Coeff.	Std. Err.	Prob.	Coeff.	Std. Err.	Prob.
Constant (b_0)	−3.58	(0.02)	0.0001	−8.75	(1.08)	0.0001
Democratic (b_1)	−1.10	(0.11)	0.0001	22.92	(6.15)	0.0003
Year (Dem.) (b_2)				−0.0096	(0.0032)	0.0028
Year (Other) (b_3)				0.0027	(0.0006)	0.0001
Log-likelihood	−16,835.9			−16,805.1		
Sample size	73,320			73,320		

The democratic dummy variable allows estimation of two separate exponential learning curves. Setting $D = 1$ yields the curve for interdemocratic relations,

$$\text{Pr}_{dem}(Y,\beta_0,\beta_1,\beta_2) \approx \exp(\beta_0+\beta_1+\beta_2 Y).$$

Setting $D = 0$ defines the function for all other dyads,

$$\text{Pr}_{other}(Y,\beta_0,\beta_3) \approx \exp(\beta_0+\beta_3 Y).$$

Whereas β_2 measures the decay constant k for democratic relations as defined in equation 2, β_3 estimates the corresponding quantity for all other dyads. Likewise, from $\beta_0 + \beta_1$ and β_0 we can obtain estimates for the respective curve's intercepts, π_0.

In terms of the current notation, Kant's liberal learning theory generates three propositions:

P1: $\beta_2 < 0$,
P2: $\beta_2 < \beta_3$, and
P3: $\beta_3 \geq 0$.

The first proposition states that, over time, democracies learn to become more peaceful in their mutual relations. The second postulates that learning proceeds more quickly in democratic relations than in other dyads. The third requires that all other relations fail to exhibit any learning whatsoever. Taken together, these constitute a first, crude assessment of Kant's complex learning theory.[12]

Table 1 compares the results generated by the dynamic base model to those of the corresponding static framework. The latter confirm what most quantitative studies have already shown, namely, that joint democracy has a negative and highly significant effect on dispute behavior.[13] More important, the dynamic

analysis vindicates all three Kantian propositions. There is strong evidence for P1 with a clearly negative coefficient at −0.0096 at a high level of significance. Moreover, given the positive estimate for the other dyads, P2 holds as well and a Wald test reveals that the difference between the two slope coefficients is significant ($p = 0.0002$). Finally, this estimation corroborates P3 because of the positive sign of b_3.[14]

Since the two models are nested, it is also possible to conduct a likelihood-ratio test to establish whether the variables of the dynamic model are jointly significant. The test statistic is 30.8, so it is clear that the difference is highly significant ($p < 0.0001$).

Based on these estimation results, Figure 2 plots the two conflict curves for the entire sample period, using a thick line for democratic dyads and a thin line for others. As would be expected from Figure 1, the democratic probability function starts at a high level but decreases steadily and is well below the other conflict curve.

The visual impression confirms the numerical findings as well as the intuition drawn from the moving averages of Figure 1. It is indeed possible to fit a learning curve in the case of democratic relations. Based on equation 2, the democratic learning rate can be computed as $c = 1-\exp(k) = 1-\exp(b_2) = 0.0096$, that is just below one percentage point. In the long run, this makes an important difference; if the initial dispute frequency is about 0.03 (refer to Figure 1), the rate can be expected to shrink 150 years later to as little as $(1.0-0.0096)^{150} = 0.007$.

In contrast to the earlier graph, however, the democratic curve in Figure 2 no longer starts at a much higher level than the other learning curve. If we are to believe the estimated exponential curves, the discrepancy in Figure 1 is mainly an artifact of the small number of observations for the mid-nineteenth century.[15] To the extent there is a substantive reason for

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[12] Since Kant did not specify the original relationship between democratic and other relations, these propositions say little about the starting point of learning curves. If we assume that large states are more prone to conflict than small ones, Kant's ([1784] 1970a) suggestion that democratic security communities tend to develop around a great-power core can be used to explain why initial conflict frequencies are more elevated for democratic states.

[13] Macrohistorical processes of this type violate the assumption of both temporal and spatial independence of the observations, which implies that the estimates of the standard errors might be inflated. To fix this problem, Beck, Katz, and Tucker (1998) introduce an ingenious "peace year" correction for duration dependence, but they point out (p. 1273) that their method is incompatible with models containing "variables that vary by time but not across units," which is what the current model does. Thus, I choose to disregard their fix. In a study based on time-varying parameters that escapes some of these

problems, Cederman and Penubarti (1999) confirm the qualitative results of Table 1.

[14] Sensitivity analysis distinguishing between purely nondemocratic and mixed dyads yields similar findings. The purely nondemocratic curve becomes even steeper, however, and the corresponding curve for mixed dyad relations loses most of its slope.

[15] The early sample period contains relatively few data points. In 1837, there are only 156 observations, and only the British-American dyad is democratic. Not until 1848 were these two democracies joined by France and Switzerland, creating five democratic dyads. In

FIGURE 2. Estimated Learning Curves for Democratic and Other Dyads

Note: The curves represent the probability of militarized interstate disputes per dyad-year for each category of risky dyads (including the two world wars), based on the parameter estimates reported for the dynamic model in Table 1.
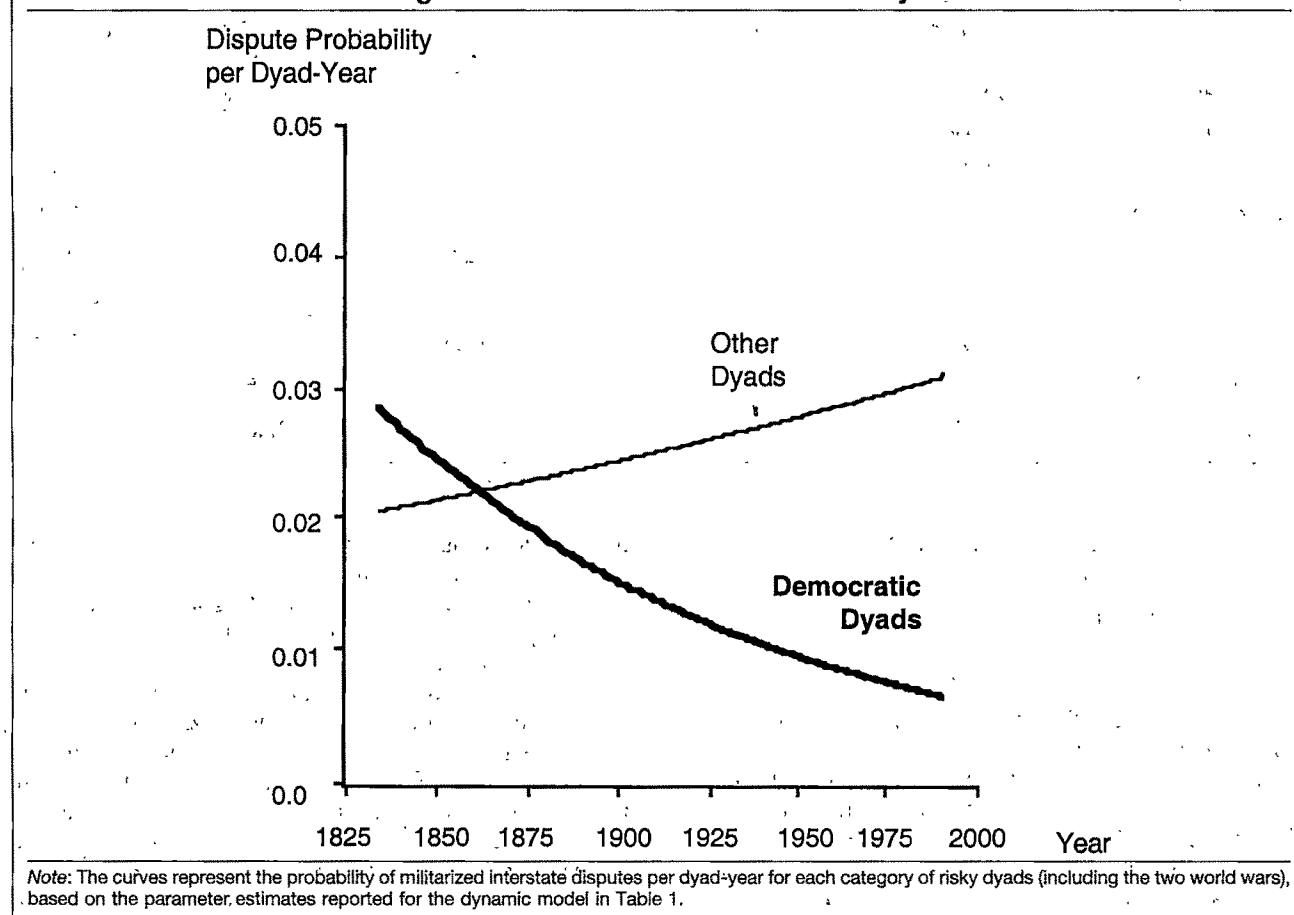
initial "democratic belligerence," however, it may derive from that fact that the first democracies—in particular the United States, Britain, and France—were involved in colonial and postcolonial competition, which produced many conflict opportunities and delayed the emergence of a democratic security community. Because the United States was a secessionist republic, Anglo-American relations only gradually developed from the original "vertical" mode into a "horizontal" one that permitted norms of equality to take root. Moreover, Owen (1994) shows that the very meaning of democracy has evolved historically. In the absence of a stable intersubjective understanding of the term, there could be no learning process. In earlier times, democracies tended to be remarkably jingoistic, as evidenced by the Spanish-American War, but more recent history reflects a steady trend toward pacifism.

ROBUSTNESS OF THE INITIAL RESULTS

Does Kant's democratic learning effect disappear when we control for epochal effects? Do the results become spurious once exposed to competing explanations? Do

the propositions hold across the sample? The following analysis addresses each question in turn.

Epochal Effects

Kant's dialectical argument refers to catastrophic reversals of the peace process, which justifies special treatment of the world wars. An explicit tracing of the causal effect of these disturbances lies beyond the scope of the study, but it is appropriate to exclude these comparatively short periods from the sample since they detract from the macro trend. My focus is on historical progress rather than temporary shocks. In any case, during these general wars, dyadic interactions became almost meaningless (Gowa 1999, 47).

In addition, I control for systemic effects for the remaining periods. There may be both geopolitical and liberal explanations for why these epochs differ, and the challenge is to establish that the learning effect persists despite such changes.[16] Rather than partition the sample itself, I introduce two dummy variables, INTER (for the interwar period, 1921–38) and COLD (for the Cold War, 1948–92), to capture the indepen-

1900, the number increased to 18 out of 338 dyads. Only eleven disputes between democracies occurred before 1900, many of them pitting the United States against the United Kingdom. The findings in Table 1 depend crucially on these early years, which will be explored in the next section.

[16] Highlighting systemic differences related to polarity and weapons technology, Gowa (1999) partitions her sample into pre–World War I and post–World War II eras and drops the interwar years. I see no compelling reason to exclude the interwar period.

TABLE 2. Logit Analysis of Dispute Propensity with Period Effects

Variable	Static Model (Risky Dyads without World Wars)			Dynamic Model (Risky Dyads without World Wars)		
	Coeff.	Std. Err.	Prob.	Coeff.	Std. Err.	Prob.
Constant (b_0)	−4.16	(0.06)	0.0001	9.71	(3.56)	0.0063
Democratic (b_1)	−1.09	(0.12)	0.0001	24.60	(7.34)	0.0008
Year (Dem.) (b_2)				−0.0205	(0.0041)	0.0001
Year (Other) (b_3)				−0.0074	(0.0002)	0.0001
Interwar (b_4)	0.06	(0.11)	0.27	0.43	(0.15)	0.0040
Cold War (b_5)	0.51	(0.07)	0.0001	1.26	(0.20)	0.0001
Log-likelihood		−12,606.2			−12,579.1	
Sample size		67,395			67,395	

dent effects (β_4 and β_5) of the two last historical periods, respectively:[17]

$$X\beta = \beta_0 + \beta_1 D + \beta_2 D \times Y + \beta_3(1 - D)Y$$
$$+ \beta_4 INTER + \beta_5 COLD. \qquad (5)$$

Thus, the intercept β_0 (together with β_1, the coefficient for the democracy dummy) measures the starting point of the pre–World War I learning curve. For the interwar period, β_4 has to be added to the intercept, and in the case of the Cold War, the adjustment is β_5.

Table 2 lists the findings from the static and dynamic versions of this modified framework. In the time-invariant model, the democracy effect changes little compared to the static model in Table 1. Contrary to realist expectations, and despite the presence of bipolarity and nuclear weapons, the Cold War period actually appears to be characterized by a *higher* level of conflict than both the pre–World War I and interwar eras. The coefficient associated with the latter period does not reach significance, however.[18]

In the dynamic model, the interdemocratic learning effect becomes twice as strong compared to the non-periodized results in Table 1. Interestingly, the other dyads now experience a negative downward slope, which disconfirms P3. Their learning is still clearly slower compared to democracies (Wald test with $p <$ 0.0005). As in the static model, the results indicate that the Cold War period exhibited more conflict, and now the coefficient for the interwar years turns significant as well.[19]

Propositions 1 and 2 receive further support, but the period-dependent analysis suggests that the learning

process is stronger in, but not isolated to, democratic relations. This means that the strong version of Kant's theory that relies on P3 is quite fragile. The weakness of this proposition can be attributed partly to the sharply dichotomous democracy coding used in this study, but learning in the residual category is too strong to be a mere artifact of operationalization. It is thus necessary to reassess the most categorical version of Kant's theory in favor of a weaker reinterpretation. Kant viewed democracy as an initial condition triggering the peace process, followed by a gradual normative diffusion throughout the international system. Rather than being a necessary condition, pacific development proceeds at differential speed, although interdemocratic relations are still the fastest way to transcend conflict.[20] This, of course, comes as no surprise to liberal scholars, who have argued that growing war-weariness and conflict inhibition can trickle into all interstate interactions (e.g., Mueller 1989).

Alternative Explanations

Aside from factoring in historical influences, I have so far refrained from incorporating any explicit control variables. As we have seen, however, Kant's multi-causal version of liberalism is embedded in a geopolitical environment. Rather than necessarily compete with all realist and liberal accounts, his framework can coexist with power-related processes and liberal domestic-level factors other than regime type.

To investigate whether the learning hypotheses remain valid under such conditions, I will introduce three control variables; the first two capture geopolitical determinants, and the last is an additional liberal factor. First, using COW data (Small and Singer 1990), the dummy variable *ALL* flags whether the two states

[17] I exclude the two years after each world war since the interstate system can be assumed to require time to adjust after such shocks. This modification improves the fit somewhat without changing the qualitative results.

[18] This is one of the few cases in which the "shadow" of the world wars matters, since significance can be easily obtained if the sample includes 1919 and 1920 as well.

[19] Despite bipolarity and nuclear weapons, this era appears to be more rather than less conflictual than other periods, possibly due to decolonization or increased deterrence-induced subwar conflict. Detailed analysis of this issue lies beyond the scope of this study.

[20] This interpretation is strengthened by an extended robustness test that disaggregates dyads into mixed and purely nondemocratic pairs. Indeed, such an elaboration reveals that the slopes vary gradually, from a strong −0.021 for democracies, through −0.012 for mixed dyads, to −0.005 for authoritarian states. A double Wald test significantly separates the democratic learning rate from the other two ($p = 0.001$ and 0.014), which generalizes P2.

TABLE 3. Logit Analysis of Dispute Propensity with Control Variables

Variable	Static Analysis (Risky Dyads without World Wars)			Dynamic Analysis (Risky Dyads without World Wars)		
	Coeff.	Std. Err.	Prob.	Coeff.	Std. Err.	Prob.
Constant (b_0)	−4.87	(0.11)	0.0001	20.08	(5.10)	0.0001
Democratic (b_1)	−1.07	(0.15)	0.0001	25.57	(8.93)	0.0042
Year (Dem.) (b_2)				−0.0269	(0.0052)	0.0001
Year (Other) (b_3)				−0.0013	(0.0027)	0.0001
Interwar (b_4)	0.29	(0.14)	0.037	0.90	(0.19)	0.0001
Cold War (b_5)	0.91	(0.11)	0.0001	2.09	(0.26)	0.0001
Alliance (b_6)	−0.52	(0.09)	0.0001	−0.50	(0.09)	0.0001
Capability (b_7)	2.25	(0.10)	0.0001	2.24	(0.10)	0.0001
Development (b_8)	−0.12	(0.03)	0.0001	−0.09	(0.03)	0.0025
Log-likelihood		−8,801.4			−8,768.1.	
Sample size		43,783			43,783	

are members of at least one common military alliance. Second, the index *CAP* measures the capability balance between the stronger and the weaker party of a dyad, (1 = perfect symmetry, 0 = total asymmetry) (Bennett and Stam 1999; Singer, Bremer, and Stuckey 1972). This measure is based on six power components: military expenditure, military personnel, iron and steel production, energy consumption, total population, and share of urban population. Third, complementing the liberal theme of democracy, the variable *DEV* is a proxy for the lowest dyadic level of economic development. This measure is based on energy consumption per capita, which is available as a part of the COW capability data (Bennett and Stam 1999).[21]

In sum, the expanded explanatory scope, which also features the period dummies as in equation 5, yields a new regression equation:

$$X\beta = \beta_0 + \beta_1 D + \beta_2 D \times Y + \beta_3(1 - D)Y + \beta_4 INTER$$

$$+ \beta_5 COLD + \beta_6 ALL + \beta_7 CAP + \beta_8 DEV. \quad (6)$$

According to recent quantitative studies, the alliance measure should be negative, which reflects the intuition that aligned states are less likely to engage in mutual conflict. This is so because they have to worry about an external enemy and often share other interests (e.g., Russett, Oneal, and Davis 1998). The effect of the capability symmetry is less intuitive, but the literature offers both theoretical and empirical hints. Whereas a balance-of-power interpretation suggests that conflict becomes rarer as bilateral power relations become more symmetrical, an alternative version of realism, sometimes referred to as power transition theory, anticipates precisely the opposite: The more

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[21] A measure of GDP per capita is preferable, but its availability is very limited for the earlier part of the sample. I initially used urbanization data drawn from COW, with qualitatively similar results, but closer scrutiny revealed that this measure is of such poor quality that it cannot be relied upon.

evenly balanced are resources, the less certain is the outcome of a military clash, and thus the more likely that aggressors will be tempted to take risks (Organski 1968). The latter interpretation has received empirical confirmation in the studies of Russett and his colleagues (Oneal and Russett 1997; Russett, Oneal, and Davis 1998), who found that resource symmetry fosters conflict. Finally, from a Kantian standpoint, economic development should reduce conflict. Kant's political strand of liberalism emphasizes the opportunity costs of broken trade interdependencies, although, on a more general interpretation, his "spirit of commerce" signifies wealth-generated pacification of market economies (cf. Doyle 1983a, 213; McMillan 1997, 36; for statistical evidence, see Bremer 1992; Oneal and Russett 1997). Obviously, the two can be expected to be closely correlated, since wealthy states usually trade the most, so in either case development should have a strong, conflict-reducing influence.

The static regression reported in Table 3 reinforces the baseline expectations. Democracy has almost the same effect as in Table 2. The coefficients of the other variables are also as anticipated: The influence of both alliances and development is negative, and symmetric capabilities increase conflict frequency. In the dynamic model, P1 and P2 continue to hold. A Wald test of the second of these yields strong significance ($p < 0.003$). Hence, even in the presence of statistical control, democratic states still experience a noticeably faster learning rate among themselves than do other dyadic combinations. In contradiction to P3, there is again evidence for a weak, but far from negligible, learning process in the residual dyad category ($b_3 = -0.0013$).

Despite the presence of the dynamic terms, the control variables and the dummy variables remain unchanged and point in the expected direction. In consequence, these results still contradict the attempts to extricate the democratic peace from its geopolitical

context in order to reject it. Gowa (1999), for example, seeks to show that the democratic peace can be explained as an artifact of power-based interests (cf. Weede 1984). Yet, the fact that the initial findings refuse to disappear despite the explicit inclusion of realist control variables, such as common alliance ties and the capability balance, together with systemic dummy variables, increases our confidence that Kant was right. Regime-type dependent learning appears to unfold in tandem with realist and other liberal processes.

Dyadic Time

So far, I have made the simplifying but unrealistic assumption that learning proceeds at a constant rate throughout the entire sample period. Even though the most effective way to relax this assumption is to select a more flexible functional form, I opt for a simpler way to test the temporal robustness of results.[22] This is an important task, because careful sensitivity analysis indicates that the results obtained thus far, and especially the dynamic ones shown in Table 1, depend quite heavily on a rather small number of observations in the nineteenth century.[23]

It is desirable to develop a more robust model that holds evenly across the sample. This is why considering other temporal measures becomes crucial. It goes without saying that a severely left-truncated sample will make it very hard to separate the slope of the two learning curves, especially since the democracy exponential is bound to flatten out once learning is saturated. This means that P2 will be undermined without any substantive reason to reject Kantian theorizing. At the same time, as the unfolding of world history introduces more and more democracies, there are sharply differing experiences with this regime type. Some areas of the world are dominated by young democracies, and in other regions democratic rule has had the time to "mature." In particular, since the early years after World War II coincide with decolonization, the difference between "old" and "young" democratic dyads becomes important.

To solve this problem, I extend Kant's learning perspective by including dyadic (as opposed to calendar) time. This not only makes it possible to differentiate between democracies with a long history of cooperation and those that have only recently started to interact but also promises to improve the model's cross-temporal applicability. Technically, the elaboration requires a democratic maturity variable, M, which is incremented for each year that both states remain democratic. It is set to zero for completely new dem-

ocratic dyads and when the relation ceases to be mutually democratic.[24]

Because I have added this reformulated learning effect, there is no longer any need to separate the slope variables. In any case, tables 2 and 3 show that world-historical learning applies to both relational types. Thus, I include an undifferentiated variable to indicate the calendar year, Y. The modified equation can be written as:

$$X\beta = \beta_0 + \beta_1 D + \beta_2 Y + \beta_3 M + \beta_4 INTER + \beta_5 COLD.$$

This implies that the Kantian refinement depends on the following propositions:

P4: $\beta_2 < 0$,
P5: $\beta_3 < 0$.

The first of these requires that all dyads exhibit learning, whether democratic or not. The second postulates that each year of common democratic history makes democracies more peaceful in their mutual relations. These propositions should hold at any point in the historical record.[25]

Table 4 presents the results of this refined analysis. The first three columns illustrate what happens when the model is applied to the entire sample (except for the world wars and their two-year "shadows"). As can be seen, both P4 and P5 stand, although the maturity effect acts much more swiftly than the corresponding world-historical trend.[26] In order to gauge the recent validity of the refined learning model, the middle three columns focus on the Cold War era (which obviates the need for period dummies). Again, the two key coefficients are negative and significant, as postulated by P4 and P5, which further strengthens the maturity argument. The regression results in the last three columns add the same three control variables used in Table 3. Again, the main results are unchanged, although the development variable becomes insignificant. Nevertheless, given the latter's preliminary operationalization, it is premature to dispose of economic liberalism as a factor for the post–World War II period.[27]

A graphical representation can be used to interpret these findings. Figure 3 plots the estimated learning curves resulting from the middle three columns of Table 4, that is, the dynamic model without control variables. Despite the steady decrease in the Cold War

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[22] I have pursued this project elsewhere: Cederman and Penubarti (1999) employ a more elaborate technique based on time-varying parameter estimation.

[23] Running the dynamic base model from Table 1 on a truncated sample from 1880 seriously weakens all the propositions. For example, the slope difference between democratic and other dyads becomes insignificant ($p = 0.065$).

[24] The counter is activated for the first democratic dyad in 1837 and incremented for all periods except during the world wars. Thus, authoritarian invasions of such countries as Denmark and France are not allowed to bias the results during the World War II period.

[25] For the early sample period, a Kantian interpretation is agnostic about the democracy intercept. For the later stages, however, we must require that $\beta_1 < 0$.

[26] Further checks reinforce confidence in the cross-temporal validity of the model. It hardly makes any difference whether the sample starts at 1880 or 1921 (which means that the interwar dummy has to be omitted). In both cases, P4 and P5 hold, and again the latter has the strongest influence, although the slope varies slightly from case to case.

[27] It would be particularly interesting to use explicit GDP measures and trade data, but that goes beyond the scope of this article. Another possibility is that development has a strong but time-varying influence on conflict patterns. See Cederman and Penubarti (1999) for a model that relaxes the functional form of the control variables.

TABLE 4. Dynamic Analysis of Dispute Propensity with Democratic Maturity Effect

	Risky Dyads except World Wars			Risky Post-World War II Dyads					
				Without Controls			With Controls		
Variable	Coeff.	Std. Err.	Prob.	Coeff.	Std. Err.	Prob.	Coeff.	Std. Err.	Prob.
Constant (b_0)	10.1	(3.6)	0.0045	43.9	(5.44)	0.0001	46.11	(6.59)	0.0001
Democratic (b_1)	−0.62	(0.17)	0.0002	−0.67	(0.19)	0.0005	−0.78	(0.23)	0.0007
Year (b_2)	−0.0076	(0.0019)	0.0001	−0.024	(0.003)	0.0001	−0.025	(0.003)	0.0001
Dem. Maturity (b_3)	−0.0251	(0.0075)	0.0008	−0.029	(0.009)	0.0013	−0.028	(0.011)	0.0097
Interwar (b_4)	0.45	(0.15)	0.003						
Cold War (b_5)	1.25	(0.20)	0.0001						
Alliance (b_6)							−0.57	(0.09)	0.0001
Capab. (exp.) (b_7)							2.20	(0.12)	0.0001
Development (b_8)							−0.044	(0.030)	0.14
Log-likelihood		12,574.3			8,163.1			6,642.2	
Sample size		67,395			38,970			30,322	

era, the nondemocratic and mixed categories were considerably more conflict-prone than all-democratic dyads. The addition of dyadic time allows us to distinguish different types of interdemocratic relations. Whereas the dotted line represents the steadily falling conflict probability for newly democratic dyads (i.e., those of zero relational age), the two lower bold curves trace the history of two imaginary but typical dyads. The lowest curve, for democratic couples already 50 years old in 1948, reflects a democratic security community with almost no conflict. The curve just above it, for dyads that turned mutually democratic in 1948, reveals a more dramatic decrease in dispute propensity, although the starting point is much higher.[28] This picture is consistent with the difference in "fresh" relations between newly independent states, or between such states and old democracies, on the one hand, and stable interdemocratic relations in the developed world, on the other hand.

To render the three bold democracy curves a bit more concrete, consider the example of Britain and France, which by 1948 had together accumulated 60 years of democratic maturity. Their conflict record is blank since 1898. Approximating the curve labeled "new dyad in 1948" are British-Indian relations, which after India's independence in 1947 display increasing maturity. From 1950, India is coded a democracy and continues to be so until 1992, which yields a maturity of 43 years in the last data year. As would be expected, there are no conflicts between the two countries. Finally, the troubled dyad of Greece and Turkey is close to the "new dyad every year" curve. Because

democracy is unstable in both states, the maturity effect has never stabilized. In 1948 both were democracies, but in 1960 that bond was broken. The relationship bounced back to nondemocratic status during long periods (1967–74 and 1980–83), and democratic maturity never lasted more than nine years. Symptomatically, disputes occurred especially, but not exclusively, in the nondemocratic years.

Several conclusions can be drawn from this picture. First, democratic security communities take time to mature. This result resonates with Maoz and Russett's (1993, 629) statement that "the more democratic both members of [any dyad], the less likely they will become embroiled in a military dispute." The advantage of my maturity-based operationalization is that it provides a more explicitly dynamic way to gauge the deepening of democratic relations.[29] Second, newly democratized relations improve quite quickly after both countries become democratic. My model does not operationalize democratization explicitly, and thus cannot speak to Mansfield and Snyder's (1995) assertion that democratizing states are more conflict-prone than stable democracies, but it does support their claim about democracy's need to settle before pacification can take root. Third, the nonjointly democratic dyads exhibit a trend toward lower dispute propensities. Further analysis is needed to establish whether this is an effect of a general civilizing diffusion of peaceful norms in the international system (Mueller 1989) or is due to geopolitical factors, such as nuclear deterrence and bipolarity (e.g., Gowa 1999; Mearsheimer 1990). The fact that these trends appear to develop smoothly and transgress period boundaries, however, makes it less

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[28] In formal terms, the three curves were constructed as follows. The "just-turned-democratic" function defined for $t = 1948$–92 can be constructed by setting $D = 1$ and $M = 0$: $\Pr(t) = \exp(b_0 + b_1 + b_2 t)$. The corresponding curve for a new democratic dyad in 1948 is $\Pr(t) = \exp\{b_0 + b_1 + b_2 t + b_3(t - 1948)\}$, and that of the dyad that in 1948 is 50 years old is $\Pr(t) = \exp\{b_0 + b_1 + b_2 t + b_3(t - 1948 + 50)\}$.

[29] Gowa (1993, 412) claims that there is no strong evidence of a "deep democracy hypothesis." Although she creates a more demanding criterion for democracy (a score of at least ten), however, her test is static.

FIGURE 3.   Estimated Learning Curves with Democratic Maturity Effect
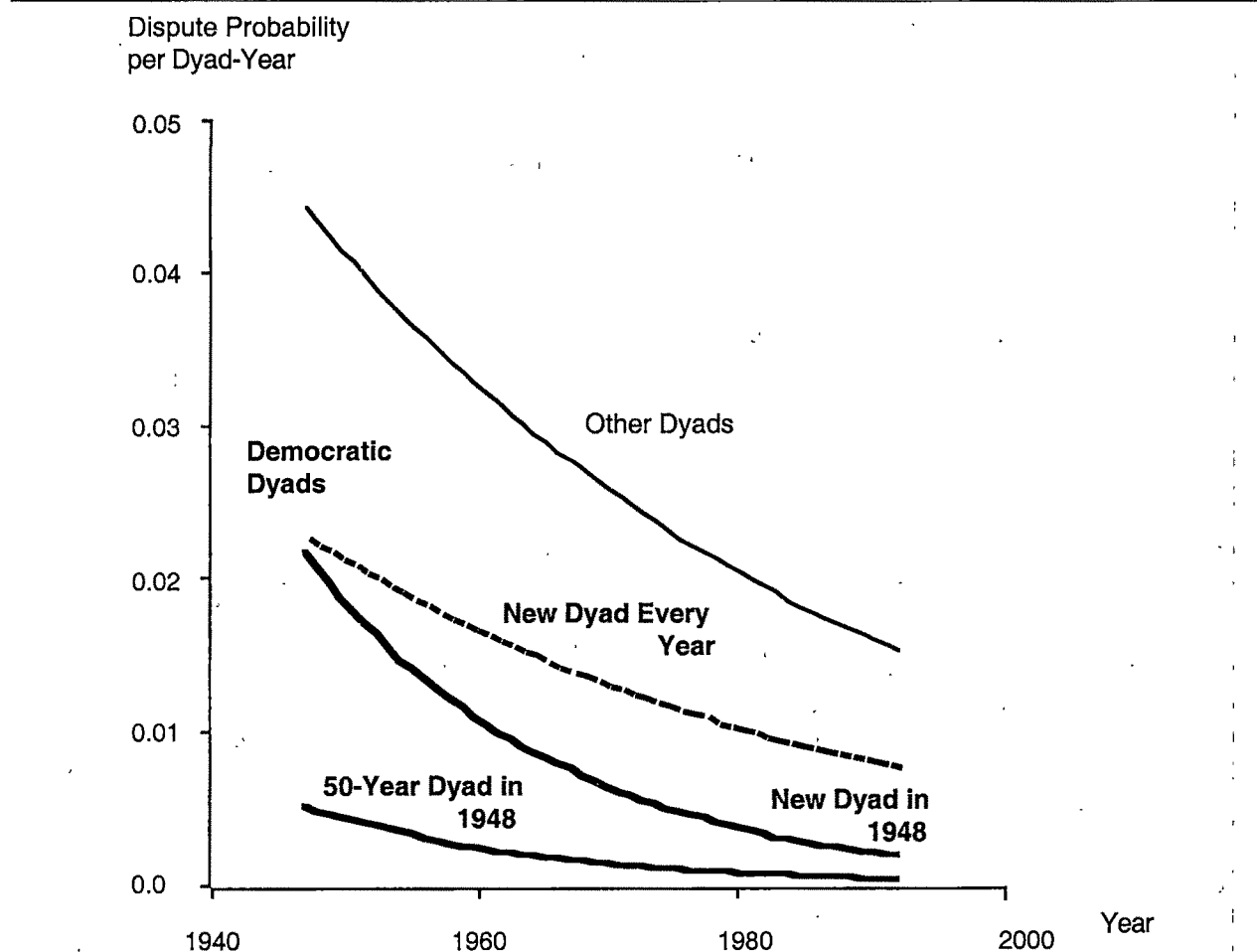
Note: The curves represent the probability of militarized interstate disputes per dyad-year for each category of risky dyads, based on the parameter estimates reported in the middle three columns in Table 4. "New Dyad Every Year" assumes that democracy has zero dyadic age at every point. "New Dyad in 1948" and "50-Year Dyad in 1948" refer to dyads that became mutually democratic in 1948 and 1898, respectively, and have remained so.

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likely that the answer can be found in conventional neorealist analysis.

CONCLUSION

It is premature to conclude that I have corroborated Kant's theory in its entirety, but my empirical investigations show that his conjecture stands up surprisingly well to evidentiary scrutiny. Democracies are indeed faster learners when interacting among themselves, and their relations become more peaceful with common experience of long duration. At the same time, I find no evidence for the strongly Kantian interpretation that pacific progress is restricted exclusively to democratic relations. In fact, the normative evolution appears to spill over into mixed, and to some extent even purely authoritarian, dyads.

In order to reach firmer conclusions, much theoretical, empirical, and methodological work remains to be done. What particular type of collective learning mechanisms operate at different stages of history and in different areas of the globe? Why do the nondemocratic and mixed relations also exhibit a pacifying

trend? What is the relative causal influence of negative and positive learning in various contexts?

Despite some inconclusiveness, this study has important consequences for future exploration of the democratic peace. On the meta-theoretical level, it warns against the dangers of method-driven thinking. Without doubt, regression analysis has improved our knowledge about the influence of regime type on conflict behavior, but this often has happened at the expense of theoretical imagination. By implicitly or explicitly (e.g., Ray 1995) treating the democratic peace hypothesis and its competitors as universal "covering laws," most scholars collect more observations to avoid selection bias rather than theorize about the population's temporal and spatial boundaries (Dessler 1991).

In terms of theory, I argue that international relations researchers still have much to learn from Kant's original statement. This does not mean that theorizing should be limited to merely rearticulating the Kantian thesis. Indeed, the results of this study cast doubt on a simplistic interpretation that treats mutual democracy as a necessary condition for pacific learning. Rather, it is more fruitful to consider Kant's position as a source

of conceptual inspiration. There is no denying that the intervening two centuries have produced promising theoretical insights and improved methods that will be useful in efforts to elaborate theories of dynamic and dialectical processes in the Kantian spirit.

For instance, the explanatory puzzle of democratic learning may find its resolution in recent scholarship. The notion of a security community may be particularly useful for theory-building. Although Karl Deutsch and his colleagues (1957) did not express their definition of security communities in terms of liberal democracy, they and others implicitly approximate the Kantian idea through reference to common values that support peaceful change (cf. Adler and Barnett 1998; Eberwein 1995; Risse-Kappen 1996; Russett 1998). Advances in applied learning theory also are promising in that they link the effectiveness of learning processes to domestic political structures (e.g., Reiter 1995; 1996; cf. Eder 1985). Moreover, methodological advances in evolutionary game theory and computational analysis pave the way for more flexible and context-sensitive modes of modeling (e.g., Axelrod 1984, chap. 8; Bendor and Swistak 1997; Cederman 1999).

One of the most urgent theoretical tasks is to widen the analytical focus as a way to introduce more elaborate causal control. To keep things simple, I chose a minimal set of control variables. In addition to introducing new indicators, it would be desirable to endogenize the functional form, which has been assumed to be constant. A more open-ended assessment of conflict trends stretching farther into the post–Cold War era may yield important findings about the future prospects of a Kantian peace taking root in ever larger areas of the globe.

Indeed, future empirical studies of earlier stages of the world-historical process also could benefit from an explicitly Kantian perspective. Without such refocusing, the debate will continue to treat many nineteenth-century cases as mere refutations of the democratic peace "law," when they actually may corroborate a dynamic reinterpretation. Policy considerations should prompt a shift from the earlier cases to more recent interactions, but the former still deserve scholarly attention.

Seen in this light, the interdemocratic conflicts and crises of the previous century illuminate the dramatic contrasts with today's highly institutionalized relations among democracies. Intertemporal comparisons of this type illustrate how military means have lost virtually all importance within a stable core of today's democratic security community. Mearsheimer's (1990) back-to-the-future scenarios notwithstanding, current power struggles within the democratic zone of peace concern monetary policy rather than clashes of imperial armies. In other words, Frankfurt has replaced Fashoda as the focus of political action.

This does not mean that "backsliding" will cease: To argue otherwise would completely miss Kant's dialectical point. But it would be foolish to treat today's Frankfurt in the same terms as yesterday's Fashoda. Helmut Kohl's statements that European integration is a matter of peace and war illustrate how much this

distinction between the bad old days and the good new days has been internalized by the world's democratic leaders. I have attempted to show that enough evidence now exists for social scientists to take the Kantian peace conjecture seriously. Indeed, if one accepts Kant's ethical reasoning as well, future research into the topic is not just an intellectual challenge; it is our duty.

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Toward a Democratic Civil Peace? Democracy, Political Change, and Civil War, 1816–1992

HÅVARD HEGRE International Peace Research Institute, Oslo, and University of Oslo
TANJA ELLINGSEN University of Oslo
SCOTT GATES International Peace Research Institute, Oslo, and Michigan State University
NILS PETTER GLEDITSCH International Peace Research Institute, Oslo, and Norwegian
University of Science and Technology

Coherent democracies and harshly authoritarian states have few civil wars, and intermediate regimes are the most conflict-prone. Domestic violence also seems to be associated with political change, whether toward greater democracy or greater autocracy. Is the greater violence of intermediate regimes equivalent to the finding that states in political transition experience more violence? If both level of democracy and political change are relevant, to what extent is civil violence related to each? Based on an analysis of the period 1816–1992, we conclude that intermediate regimes are most prone to civil war, even when they have had time to stabilize from a regime change. In the long run, since intermediate regimes are less stable than autocracies, which in turn are less stable than democracies, durable democracy is the most probable end-point of the democratization process. The democratic civil peace is not only more just than the autocratic peace but also more stable.

The "third wave of democratization" (Huntington 1991; Vanhanen 2000) has raised hopes for a more peaceful world. The thesis of the democratic peace suggests that the spread of democracy will promote a decline in interstate warfare (Doyle 1986; Russett 1993), at least once the unsettling effects of the transition period are overcome (Ward and Gleditsch 1998). But does democratization also lead to civil peace?

Considerable research has examined how regime type or the level of democracy relates to domestic conflict. Much of it focuses on the result that semidemocracies (regimes intermediate between a democracy and an autocracy) exhibit a higher propensity for civil conflict than either extreme. Another strand of research focuses on how changes in regime lead to domestic conflict. This has implications for the former finding, since semidemocracies are more prone to regime change. Indeed, is the greater propensity for violence of intermediate regimes equivalent to the finding that states in political transition experience more violence? Are the results relating civil violence to level and change, in fact, one and the same finding? Or,

are both explanations relevant? That is the key issue examined in this article.

We link level of democracy and regime change in an empirical analysis that uses data from 152 countries in the period 1816–1992. We also explore the implications of the direction and magnitude of political change. The statistical model we formulate overcomes some of the problems in research that is based on country-years, such as the fact that these do not constitute independent observations, as well as the possibility that the amount of civil war in the system of states fluctuates over time. Finally, our work adopts a multivariate framework with several control variables, among them socioeconomic and cultural factors, as well as spatial and temporal contagion. A separate analysis, with a more extensive set of control variables, is performed for the post–World War II period.

DEMOCRACY, DEMOCRATIZATION, AND CIVIL WAR

Level of Democracy and Civil War

Harshly authoritarian states and institutionally consistent democracies experience fewer civil wars than intermediate regimes (de Nardo 1985; Francisco 1995; Muller and Weede 1990), which possess inherent contradictions as a result of being neither democratic nor autocratic. Semidemocracies are partly open yet somewhat repressive, a combination that invites protest, rebellion, and other forms of civil violence. Repression leads to grievances that induce groups to take action, and openness allows for them to organize and engage in activities against the regime. Such institutional contradictions imply a level of political incoherence, which is linked to civil conflict.

A number of works support the hypothesis of an inverted U-shaped curve between democracy and domestic violence, but most are based on a small number

of cases or a short period. For instance, Francisco (1995) examines only the former German Democratic Republic, Czechoslovakia, and the Palestinian Intifada. The study by Muller and Weede (1990) used data collected by Taylor and Jodice (1983) for 1973–77.[1]

Ellingsen and Gleditsch (1997) confirmed the inverted U-shaped curve for a longer period, 1973–92. Using two different measures of democracy, they found that open regimes experienced civil war very rarely or, in the case of First World democracies, not at all. Moreover, by far the highest frequency of conflict occurred in semidemocracies, yielding a clearly inverted U-shaped curve across all levels of economic development.

Political Change and Civil War

The road to democracy is complicated and can be marked by internal violence and even collapse of the state (Bratton and van de Walle 1996; Casper and Taylor 1996). Autocratic countries do not become mature consolidated democracies overnight. They usually go through a rocky transition, in which mass politics mixes with authoritarian elite politics in a volatile way. Political change deconsolidates political institutions and heightens the risk of civil war, as discussed by a number of scholars (e.g., Sahin and Linz 1995; Tarrow 1994).

In a classic argument, de Tocqueville ([1856] 1955, 182) points out that "revolutions do not always come when things are going from bad to worse.... Usually the most dangerous time for a bad government is when it attempts to reform itself." Huntington (1991) finds that political violence is frequently coupled with democratization. Such changes are unlikely to occur without serious conflict, especially in countries with different ethnic minorities (Horowitz 1993). Communal groups in liberalizing autocracies have substantial opportunities for mobilization, but such states usually lack the institutional resources to reach the kinds of accommodation typical of established democracy (Gurr 1993, 165). When authoritarianism collapses and is followed by ineffectual efforts to establish democracy, the interim period of relative anarchy is ripe for ethnonational or ideological leaders who want to organize rebellion.

Theoretically, consolidation can occur anywhere on the autocracy-democracy spectrum. Those at either extreme can be consolidated or unconsolidated. Consolidated autocracies exhibit self-enforcing rules and institutions that prevent protest and other activities aimed against the state. Semidemocracies also may become consolidated. If the central idea of an inverted U-curve describes the relationship between regime type and civil war, however, the inconsistent and contradictory nature of these regimes should prevent them from becoming consolidated.

Political institutions also can be deconsolidated.

Political change, whether in the form of democratization or autocratization, can create instability. The loss of legitimacy by the regime induces dissatisfied groups to struggle against it. If the direction of change is toward autocracy, the deconsolidation of political institutions also implies increasing repression (Zanger 2000, 225–6). Repression by a regime without well-developed political institutions is likely to promote civil violence (Lichbach 1987, 1995; Moore 1998).

The initial high level of uncertainty and unrest caused by a regime change will gradually diminish as protesters abandon their aspirations or find ways to obtain part of what they want within the new regime. In the case of democratization, new and more open institutions take root and promote a peaceful resolution of domestic conflict. As time passes, these become more entrenched, and the likelihood of regime failure decreases. The pattern works similarly for autocratization. As repressive institutions strengthen, the effect of the regime change is less destabilizing and therefore less likely to generate political violence.

One Explanation or Two?

Compared to well-established democracies or autocracies, intermediate regimes have a higher hazard of civil war, as do regimes just emerging from a political transition. Are these two findings one and the same? Semidemocracies may be more prone to civil war because, on average, they have more recently undergone a political change. Gurr (1974, 1500) finds that the average persistence of the highly coherent polities—democracies and autocracies—exceeds that of "anocracies," or polities with mixed authority patterns. Below, we corroborate this finding with newer data. The implication is that we cannot readily determine whether a high risk of civil war is due to level of democracy or regime change. The two factors unquestionably overlap. Does the inherent inconsistency of semidemocracies account entirely for their greater frequency of civil war? Or does the youth of semidemocracies fully explain why they are more prone to conflict? Or do both factors affect the risk of civil war?

We cannot satisfactorily answer these questions without including both political change and level of democracy in our analyses. If both factors are relevant, we would expect to see evidence of an inverted U-curve even when controlling for the time since regime change. By controlling for each variable, we can assess whether one, the other, or both are significant.

HYPOTHESES

In an attempt to distinguish between intermediate position or change as the cause of civil conflict, we posit the following hypotheses.

HYPOTHESIS 1. *Semidemocracies are more likely to experience civil war than either democracies or autocracies.*

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[1] Krain and Myers (1997) find that democracies are less prone to civil war than autocracies, but they do not account for semidemocracies and only provide a bivariate analysis.

HYPOTHESIS 2. *Institutionally consistent democracies and stark autocracies are equally unlikely to experience civil war.*

HYPOTHESIS 3. *Countries that have undergone a recent political transition are more likely to experience civil war than countries whose political system has remained stable.*

HYPOTHESIS 4. *The two relationships described in hypotheses 1 and 3 are both valid and reinforce each other. Thus, the likelihood of civil war in semidemocracies remains higher than in other regime types, even a long time after a regime change.*

Hypothesis 1 reflects the inverted U-curve regarding the relationship between level of democracy and domestic violence, and hypothesis 3 states that regime change leads to a heightened risk of civil war in the short run. Both have found support in previous work. Hypothesis 2 states that the inverted U-curve is symmetric, as demonstrated by Muller and Weede (1990) and in contrast to Krain and Myers (1997). Hypothesis 4 accounts for the possibility that hypotheses 1 and 3 are complementary. A rejection of hypothesis 4 would mean that either hypothesis 1 or 3 is a sufficient explanation of the probability of civil war.

RESEARCH DESIGN

The Cox Regression Model

Comparable studies in this field have made use of data sets with country-years as the unit of observation (e.g., Auvinen 1997; Ellingsen 2000; Zanger 2000), but if regime change causes civil war, we expect conflict to follow shortly after regime change. The relevant time frame ranges from a few days to a few years. To model civil war as a consequence of regime change, we have to relate conflict to regime information up to the last day before the civil war breaks out. A country-year approach is unsuitable for modeling swift changes, because it makes sequential events in the same year appear to be simultaneous. Also, the Correlates of War (COW) data on civil war are coded by date, and the Polity IIId data set exactly dates regime changes to the extent possible. Such precision allows us to control for whether a conflict represents diffusion of international war or of civil war in a neighboring country.

As argued in a study of interstate war (Raknerud and Hegre 1997), the country-year structure has disadvantages of a more statistical nature.[2] Country-years do not constitute independent observations. If a civil war continues for some time, war data for the subsequent country-years will be highly dependent on the first year. Removing (censoring) country-years with continuing civil war may ameliorate this problem. Correspondingly, however, consecutive years of peace in a country are just as dependent on the first year of peace. If we censor continuing war, we also should censor continu-

ing peace, but then nothing would remain of the country-year structure. If we do not censor at all, we have a poor basis for estimating the statistical significance of the parameter estimates.

Analytical techniques for country-year data also assume a constant baseline probability of civil war, regardless of other variables. One could imagine that the amount of civil war in the interstate system fluctuates over time, following global political, ideological, and economic variations. If this fluctuation is correlated with trends in the independent variables, there is a potential problem, and to solve it Raknerud and Hegre (1997) formulated an application of the Cox regression model.[3] We modify their model to apply it to civil war. The main idea of Cox regression is the assumption that the hazard of civil war $\lambda_c(t)$ for country c can be factored into a parametric function of (time-dependent) risk factors and a nonparametric function of time itself, the baseline hazard:

$$\lambda_c(t) = \alpha(t) \exp\left[\sum_{k=1}^{p} \beta_k X_k^c(t)\right] \tag{1}$$

$\alpha(t)$, the baseline hazard, is an arbitrary function reflecting unobserved variables at the system level. The baseline hazard will account for any time trend in the data. $X_k^c(t)$ is a (possibly time-dependent) explanatory variable for country c; β_k is the corresponding regression coefficient; and p is the number of explanatory variables. All legitimate variables are known before t; they must be a part of the history up until immediately before point t. Note that t is calendar time, the number of days since a specific date. This differs from the common use of survival models, in which t is time at risk, which in this context would be the number of days since the last civil war or since the country entered the study.

To execute an analysis with this model, we need a data file constructed in the following way. For each t_w, that is, each day a civil war breaks out somewhere, we take a "snapshot" of the international system. We include the values of the explanatory variables on that particular day, t_w, for all countries that are system members and not already at war. When we do not have data on a daily basis, such as for the ethnic heterogeneity and development variables, we enter the value for the year in which the event occurred.[4] The Cox regression model compares the country that erupted in war at t_w to all countries at risk of doing so. Thus, all information for the time between different war outbreaks is ignored (except when estimating the baseline hazard).

Using the Cox regression model, civil war may be modeled as a function of events as recent as the day before the outbreak, unlike the country-year framework. Since all that happens between the outbreaks of

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[2] Raknerud and Hegre (1997) were concerned with the dyad-year structure, but most of their arguments apply equally to country-years.

[3] A good description of the Cox (1972) model can be found in McCullagh and Nelder 1989 and Collett 1994.

[4] Ideally, these variables also should have been coded on a day-to-day basis. This is not a substantial problem, however, since their values usually do not change dramatically over a short time.

war is ignored, dependence between units caused by consecutive years of peace is not a problem. Finally, possible confounding time trends in the probability of civil war are handled by the nonparametric baseline hazard function.

The parameter β_k can be interpreted as a relative probability of civil war. Assume that countries i and j have the same values on all explanatory variables except for $X_k(t)$. Then, from equation 1, the ratio between the hazard of civil war of country i and country j becomes

$$\frac{\lambda_i(t)\Delta t}{\lambda_j(t)\Delta t} = \exp\left[\beta_k\left(X_k^i(t) - X_k^j(t)\right)\right]. \qquad (2)$$

Hence, we have

$$\ln\frac{\lambda_i(t)\Delta t}{\lambda_j(t)\Delta t} = \beta_k\left[X_k^i(t) - X_k^j(t)\right], \qquad (3)$$

where $\lambda(t)\Delta t$ is approximately the probability of a transition (from peace to war) in the "small" time interval $(t, t + \Delta t)$. In the ratios above, the time interval Δt cancels out, and the parameter β_k is the log of the relative probability (or the relative risk) between two countries that differ by one unit on the variable $X_k(t)$ and are otherwise identical.

Time Frame

Our core analysis with all the variables makes use of data for 1946 to 1992. A more limited analysis with fewer variables is carried out for the entire COW period, 1816–1992.

The Dependent Variable

The dependent variable is the outbreak of civil war as recorded in the COW project (Singer and Small 1994). Civil war is defined as an internal war in which: "(a) military action was involved, (b) the national government at the time was actively involved, (c) effective resistance (as measured by the ratio of fatalities of the weaker to the stronger forces) occurred on both sides, and (d) at least 1,000 battle deaths resulted" (Singer and Small 1994, part 3). The COW project does not distinguish between different conflicts within the same country; if a civil war breaks out while another continues in a different region of the country, this is not reflected in the data set.

The criteria for coding the start of a COW civil war are potentially problematic. Coders have dated the start to the year in which the threshold of 1,000 deaths was reached, which means that previous months or even years of some wars would not count.[5] Most wars escalate quickly from the first shots to the peak level of severity, but we cannot exclude the possibility that some commence before the regime change that we code as the most recent. The definition of the start day

is important, because a period of low-level domestic turmoil beforehand may undermine the political system, but this is unlikely to affect a large number of civil wars.

Regime Type and Regime Change

For regime type, we use the Polity IIId data set (McLaughlin et al. 1998), which covers our spatial and temporal domain. The democracy-autocracy index used by Jaggers and Gurr (1995) and others is our measure of *Democracy*: -10 = most autocratic, 10 = most democratic. We add *Democracy Squared*, the square of this variable, to allow modeling of the U-curve relationship between level of democracy and conflict.[6]

To model the relationship between the time since regime change and the risk of civil war, we defined *Proximity of Regime Change* as $x = \exp(-\text{days since regime change}/\alpha)$, where α is some chosen divisor. This exponential function has the value 1 when the regime change is proximate and is close to zero when the change occurred a long time ago. It reflects the assumption that the effect of regime change on the probability of civil war decreases at a constant rate.

A weakness of the Polity data set is that an on-going civil war or other political violence in the country may be reflected in the coding of regime characteristics, notably in the indicators that characterize regulation and competitiveness of participation. A country with extensive political violence is unlikely to be coded as a full democracy or a full autocracy. Because there may be an overrepresentation of political violence in the regimes coded intermediate, our results may be confounded somewhat, since a civil war as defined in the COW project (1,000 deaths) may have begun earlier with a lower level of violence. We return to this question in the analysis section.

The Polity III data set (Gurr, Jaggers, and Moore 1989; Jaggers and Gurr 1995) codes regime change only annually. This makes it difficult to pinpoint a change relative to the outbreak of civil war. In Polity IIId the regime change is recorded to the exact day whenever possible, so if one occurred in the same year as the outbreak of a civil war, the newer data set enables us to code the regime score for the day before the outbreak. With these data we could also count the number of days since the last regime change (if any had occurred) for all countries for each time-point with outbreak of civil war (t_w). A regime change is defined as an alteration in an existing state greater than or equal to 2 in the democracy-autocracy index, or as the creation of a new state.[7] Because the range is 20 points,

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[5] We have not found precise coding criteria for war starting dates. Our source here is a personal communication from Melvin Small, June 16, 1998.

[6] A square term is the simplest model of a curvilinear pattern. We also fitted models with democracy as a 7-category and 21-category variable, as well as a model with a cubic term. All these suggested very similar relationships between the level of democracy and the risk of civil war. According to likelihood ratio tests, however, none performed better than the model reported in Table 2.

[7] If the country just entered or left a period of transition (coded in Polity IIId as -66, -77, or -88), the event is *not* coded as a regime change, regardless of what kind of regime the country had before the transition.

this definition is very inclusive, and it captures all political changes that might be related to civil war.

Control Variables

Our model does not attempt to present an inclusive theory of civil war, but level of democracy and political change do not provide a complete explanation. Therefore, we identify a number of control variables—*Development, Ethnic Heterogeneity, Proximity of Independence,* and *International War in Country*—whose omission might bias the results for the regime change variable. The remaining control variables—*Proximity of Civil War* and *Neighboring Civil War*—are included to model how the hazard of civil war depends on earlier events in the country and the neighborhood. With these controls, we may assume that the units of observations are conditionally independent (cf. Raknerud and Hegre 1997, 386–8).

Civil war occurs more frequently in poor, underdeveloped countries (Hauge and Ellingsen 1998). Referring to modernization theory, Hibbs (1973, 21–3) relates the decline in internal violence to the reduced class conflict in all affluent societies, which renders negotiated outcomes and conciliation more acceptable to all groups. Yet, since class conflicts increase in the early stage of industrialization in traditionally agrarian societies, the relationship between level of development and political violence may be curvilinear. For the poorest countries, development may actually stimulate violence. Hibbs does find evidence for a moderate curvilinear pattern, as do Collier and Hoeffler (1998).

To control for the level of development, we use *Energy Consumption per Capita* (measured as coal-ton equivalents). The variable is log-transformed, since we expect the effect of a unit increase to be larger for a country with a low level on the variable than for a country with a high level. Log-transforming also reduces the skewness of the variable. In addition, we enter the squared term of this variable to capture the curvilinear pattern found by Hibbs (1973). We expect negative estimates in both cases, which would indicate that the risk of civil war grows with development in the poorest countries and decreases in the more developed ones. The data were taken from the COW National Capabilities data set (Singer and Small 1993).

Civil war seems to occur more frequently in countries with a substantial population of one or more ethnic, linguistic, or religious groups (Ellingsen 2000; Vanhanen 1999). We measure *Heterogeneity* by $(1 - s^2)$, where s is the share of the population in the country that belongs to the largest group. We created independent variables for *Linguistic Heterogeneity, Religious Heterogeneity,* and *Ethnic Heterogeneity* based on the data set assembled by Ellingsen (2000).

The probability of civil war also depends on the country's conflict history. Hibbs (1973, 163) found internal war (but not collective protest) to be strongly influenced by earlier internal war. We expect, however, that time heals all wounds and construct a variable along the lines of the proximity to regime change variable: Proximity to civil war = exp(− time in days

since the last civil war ended/α). For a country that has never had a civil war, the variable is assigned a 0.[8]

War against another state may engender war within. An international war may provide an opportunity for dissident groups to attack a weakened regime, or another country's government may incite a revolt. Yet, international war may reduce the probability of civil war because the population unites against a common enemy. We remain neutral as to the direction of this relationship. To test it, we include a dichotomous variable, *International War in Country,* which is coded 1 if the country was involved in an interstate war (as defined in the COW Interstate War data set) the day before the day of observation.

Likewise, civil war may spread from one country to nearby areas. The variable *Civil War in Neighboring Country* has the value 1 if there was a civil war in a neighboring country the day before the day of observation.

Finally, we have added the variable *Proximity of Independence,* which equals exp(− time in days since day of independence/α). Political institutions in new states are assumed to be poorly consolidated, which may have implications for their regime type as well as for their modes of conflict resolution. Since the declaration of national independence is coded as all countries' first regime change, this variable is correlated with proximity of regime change, but not very highly (see Appendix A for the correlation matrix for the independent variables). A newly independent country may have many changes of government that are not regime changes in the sense defined here. Another aspect of the unsettled character of new nations is that their borders (e.g., if they are inherited colonial boundaries) may be in dispute and out of alignment with ethnic or religious groupings. This could lead to interstate war or to a war of secession, which would be classified as a civil war in the COW data set. We include this variable to distinguish between these effects and the effects of regime change.

We allowed the proximity of independence, civil war, and regime change variables to have independent values for α. We ran the basic model for all possible combinations of a range of values in this interval.[9] The combination of a half-life of one year for proximity of independence, sixteen years for proximity of civil war, and one year for proximity of regime change maximized the likelihood function for the period 1946–92. The corresponding values for 1816–1992 were half a year and

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[8] A half-life of 16 years means a reduction of the initial effect to 1/8 after 48 years and to 0.015 after 100 years. We have no COW data for conflict history before 1816. This is potentially problematic, since a civil war in 1815 could have a considerable effect on the risk of another in that country for most of the nineteenth century. Without data for the preceding years, the country is assigned a 0 until we know that it has experienced a civil war. The result is a systematic underestimation of the variable as a whole, such that the temporal dependence is not fully accounted for in the first 50 years of our analysis. This problem is negligible after the 1860s.

[9] To be interpretable as dynamic effects, the half-life times were restricted to values between 0.5 and 16 years. These values were α = 263.5 (0.5 years), α = 526.9 (1 year), α = 1053.9 (2 years), α = 2107.8 (4 years), α = 4215.6 (8 years), and α = 8431.1 (16 years).

sixteen years.[10] To ensure comparability, we employed the set of half-life parameters that optimized the shorter period and applied them to the longer period as well. A half-life of one year implies that the contribution to the hazard function is halved in one year, is 1/32 (or 0.03) in five years, and is 0.001 in ten years. Coding the proximity variables for a given date requires that we know the history of the country for some previous years. The Polity data set goes back to 1800 and allows us to know at least sixteen years of regime history before 1816. Since the half-life parameter assumes that the influence of any regime change is reduced to a minuscule fraction of its original effect after sixteen years, the proximity of regime change variable is adequately coded.

ANALYSIS

Level of Democracy and Political Change

We first tested whether intermediate regimes have a shorter expected duration than democracies and autocracies. We computed the Kaplan-Meier estimate of median survival time for polities belonging to the three regime types. The survival time is defined as the period from one regime change to another. As noted earlier, regime change is defined as an alteration greater than or equal to 2 in the democracy-autocracy index, or as the creation of a new state. Table 1 shows that semidemocracies have a significantly shorter median survival time than the other two types. On average, less time has passed since the last regime change in the average semidemocracy than in the other categories. Semidemocracies are the least stable type of regime, which corroborates the point made by Gurr (1974).

Autocracies are estimated to have a shorter median life than democracies, but the difference is not statistically significant. Gates et al. (2000) provide a much more elaborate and extensive investigation of the duration of different regime types. They find democracies to be significantly more durable than autocracies, and both more stable than semidemocracies. This also holds when controlling for development, the political composition of the neighborhood, and changes in regime transition rate over time.

We then tested hypotheses 1–4. The results are presented in Table 2. We conducted parallel analyses of the COW civil war data for 1946–92 with all explanatory variables, and for 1816–1992 without the ethnic heterogeneity and development variables. We only included the days with an outbreak of civil war when we had data for all variables for the country that experienced the outbreak.[11] The number of countries and the number of outbreaks that contribute to the different analyses are reported in the bottom lines of the tables. These figures vary with the availability of data for the different variables.

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[10] For the long period, we obtained even higher likelihood values when trying half-life times shorter than 0.5 years and longer than 16 years. This was not the case for the short period.

[11] A complete list of the civil wars is given in Appendix B. Additional information is available at our website (http://www.prio.no/cwp/datasets.asp).

TABLE 1. Kaplan-Meier Estimate of the Median Life for Different Regime Categories, 1800–1992

Regime Type	Median Life (years)	95% Confidence Interval	N
Autocracies	7.9	(6.7; 9.0)	445
Semidemocracies	5.8	(4.9; 6.6)	452
Democracies	10.0	(7.9; 12.1)	232

Note: An autocracy is a polity with a score in the range −6 to −10 on the democracy-autocracy index. A democracy is a polity within the 6–10 interval. A semidemocracy has a score in the range −5 to 5.

The negative estimate for democracy squared reflects an inverted U-shaped relationship between democracy and civil war. The estimate for the democracy variable is virtually 0. In other words, the estimated inverted U is symmetrical, with the apex at 0 (the intermediate regime), and regimes at the very low end of the democracy-autocracy scale are estimated to be as unlikely to experience civil war as regimes at the very high end. This supports hypothesis 2: Institutionally consistent democracies and stark autocracies are equally unlikely to experience civil war. An intermediate regime is estimated to be four times more prone to civil war than a consistent democracy. The estimates for the proximity of regime change variable are positive, large, and clearly significant. For both periods, the estimates show clearly that the risk of civil war is high after a regime change. Translated into relative risk terms, the partial effect of regime change on the hazard of civil war for the 1946–92 period was estimated at 3.55 times the baseline the day after the regime change, at 1.89 times the baseline after one year, and at 1.02 times the baseline after six years.[12]

Both democracy squared and proximity of regime change are statistically significant, which supports hypothesis 4: Both level of democracy (hypothesis 1) and regime change (hypothesis 3) are necessary to provide a full model of the relationship between regime type and the risk of civil war. In Figure 1, the estimated risk of civil war relative to the baseline is plotted (along the vertical axis) as a function of the level of democracy (the horizontal axis) and the time passed since the latest regime change. The figure shows that the maximum effect of intermediacy and political change are roughly equal when regarded separately. A regime change implies both a deconsolidation and a change in level of democracy. The combined effect can be seen in the figure. For instance, a shift from an old autocracy to a new semidemocracy (a move, say, from −10 to 0 on democratization and from 15 to 0 on years since

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[12] To obtain this estimated relative hazard one year after the regime change, we first computed the value for proximity to regime change: exp (−365 days/527) = exp (−0.692) = 0.50. This value was multiplied by $\hat{\beta}$: 0.50 × 1.27 = 0.62, which is this variable's contribution to the linear expression. The exponential of this is exp (0.62) = 1.89, which gives the hazard relative to countries that have not experienced a regime change in a long time but are equal in all other respects.

TABLE 2. Risk of Civil War by Level of Democracy and Proximity of Regime Change				
Explanatory Variables	β	s.e.	p-value	Exp(β)
A. 1946–92				
Proximity of regime change	1.27	0.47	0.004	3.55
Democracy	−0.002	0.021	0.92	1.00
Democracy squared	−0.012	0.0051	0.009	0.99
Proximity of civil war	1.16	0.97	0.078	3.19
Proximity of independence	1.51	0.97	0.060	4.55
International war in country	0.86	0.59	0.075	2.36
Neighboring civil war	0.097	0.33	0.38	1.10
Development	−0.48	0.16	0.001	0.62
Development squared	−0.066	0.036	0.031	0.94
Ethnic heterogeneity	0.80	0.39	0.019	2.22
Log-likelihood$_{\text{null model}}$	−292.17			
Log-likelihood$_{\text{model}}$	−254.76			
Likelihood ratio index	0.13			
Number of countries	152			
Number of events	63			
B. 1816–1992				
Proximity of regime change	0.98	0.37	0.004	2.68
Democracy	−0.010	0.019	0.29	0.99
Democracy squared	−0.013	0.0027	<0.0005	0.99
Proximity of civil war	1.66	0.25	<0.0005	5.27
Proximity of independence	1.86	0.68	0.003	6.41
International war in country	0.24	0.42	0.28	1.28
Neighboring civil war	0.27	0.27	0.16	1.31
Log-likelihood$_{\text{null model}}$	−535.69			
Log-likelihood$_{\text{model}}$	−484.82			
Likelihood ratio index	0.095			
Number of countries	169			
Number of events	129			

Note: The exponential of the parameter estimate, exp(β), is the estimated risk of civil war relative to the baseline hazard if all other explanatory variables are zero (cf. equation 3 above). If some of the variables are nonzero, exp(β) is the hazard relative to other countries with similar values for all the other risk factors. The log-likelihood ratio index is computed as $1 - (LL_{\text{model}}/LL_{\text{null model}})$ (Greene 1997, 891). For the democracy variable, the p-value refers to a two-tailed test; $\beta \neq 0$. For democracy squared, development, and development squared, $\beta < 0$ is tested. For the rest of the variables, $\beta > 0$ is tested. All estimates are in the expected direction.

change) increases the risk of civil war almost nine times (from 0.30 to 2.68) relative to the risk before the regime change.

In a Cox regression, all parameter estimates are interpreted relative to the baseline. The baseline hazard $\lambda(t)$ is the nonstationary probability of civil war within a short interval in countries for which all covariates equal zero, that is, countries with a democracy score of 0 that have had no regime changes or civil wars for the last forty years, are not involved in international wars and have no neighbors with civil wars, and have development = 0. In Figure 1, the baseline case is found at the forward end of the figure, at the point marked "baseline case."

In Figure 2, the estimated baseline hazard—the probability of an outbreak of civil war during one year for the baseline case—is plotted for the period 1816–

1992 (Table 2B).[13] In contrast to the common use of survival analysis, the time variable (the x-axis in the figure) is calendar time. This provides a rough picture of trends in the probability of civil war after accounting for the explanatory variables. The increase in the baseline hazard after World War II demonstrates that assuming a constant baseline probability of civil war is not tenable. As discussed by Raknerud and Hegre (1997, 388–9), statistical models that require the assumption of a constant baseline probability (e.g., logistic regression) are problematic when there are trends both in the explanatory variables (as evident in the level of democracy variable) and in the baseline probability. In some cases, the problem may lead to spuri-

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[13] The baseline was estimated using the procedure described in Collett 1994, 95ff.

FIGURE 1. Relative Risk of Civil War as a Function of Democracy and Time since Most Recent Regime Change, 1816–1992
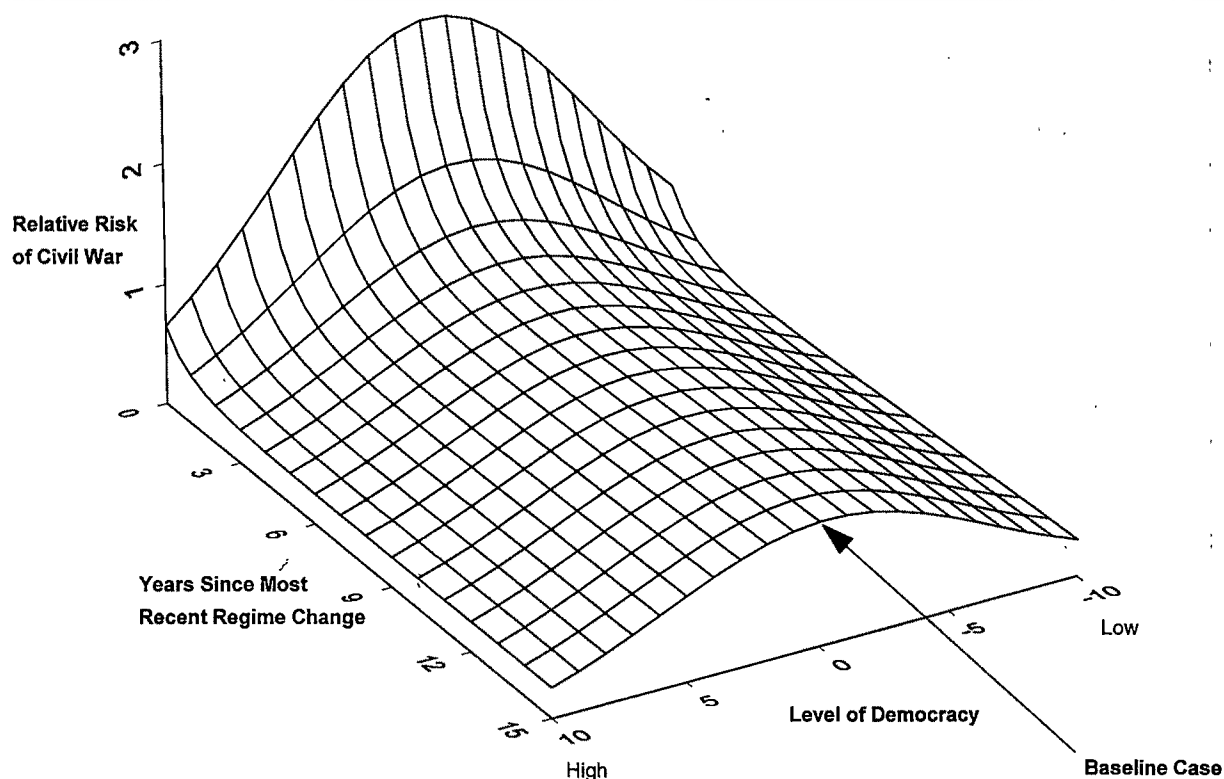
Note: The figure is based on the parameter estimates in Table 2B. The baseline case is an observation with democracy = 0 and proximity of regime change = 0 (15 years since regime change). All risks are plotted relative to this case. For instance, an observation with democracy = 0 and years since most recent regime change = 0 is estimated to have a risk of $\exp(\hat{\beta}_1 \exp(-0/527) + 0\hat{\beta}_2) = \exp(\hat{\beta}_1) = 2.68$ relative to the baseline (cf. note 12).

ous results. The Cox regression model employed here avoids these problems.

Using the more precisely dated Polity IIId data helps address a question raised earlier regarding the sequence of events. Even with Polity IIId, however, there is a danger that the events may be reversed, so that the civil war precedes rather than follows regime change. To test to what extent the estimates for proximity of regime change are influenced by such individual observations, we ran the model reported in Table 2A using only outbreaks that occurred more than 60 days after a new regime. The variable was still significant, with a p-value of .035 (one-tailed test). A drop in significance is to be expected, since we removed the five outbreaks with the highest value for proximity of regime change. Consequently, we think our results are quite robust to the problem of a reversed sequence of events.

A reversed sequence creates another potential difficulty. In such cases, the values we use for the level of democracy at the time of the war will be incorrect. To make sure that the analysis is not sensitive to this, we ran the model in Table 2A for all outbreaks that occurred less than one year after a new regime and used the democracy score before the change. Although only 18 civil wars remained in this analysis, the estimate for democracy squared was close to significance ($p = .065$).

The Effect of the Control Variables

Some of the control variables contribute significantly to the model. For 1946–92, the development variable and its squared term have highly significant effects on the probability of civil war. For that period, values under −5 (7 kgs coal-equivalent) are rare. Bhutan in 1946 is the definitively least developed country, with a score of −6.7. The estimated relative risk of civil war increases with development up to somewhere above −4 (e.g., Paraguay or Thailand in 1950, or Mali and Uganda in 1990). When the level of development passes −3 (Bhutan or Haiti around 1990), the relative risk starts decreasing, and it is halved at −0.5 (e.g., Costa Rica in 1990). The industrialized countries in Europe and North America have values around 2 (7.4 coal-ton equivalents) on our development variable. For such values, the estimated relative risk is one-eighth that of the most conflict-prone level of development. The curvilinear relationship is consistent with the findings of Hibbs (1973).

The proximity of independence variable is significant for the longer period, from 1816–1992. For 1946–92, the estimated p-value is .060. Ethnic heterogeneity does increase the probability of civil war; it is roughly twice as high in countries where the largest ethnic

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FIGURE 2. Estimated Baseline Hazard of Civil War, 1820–1992
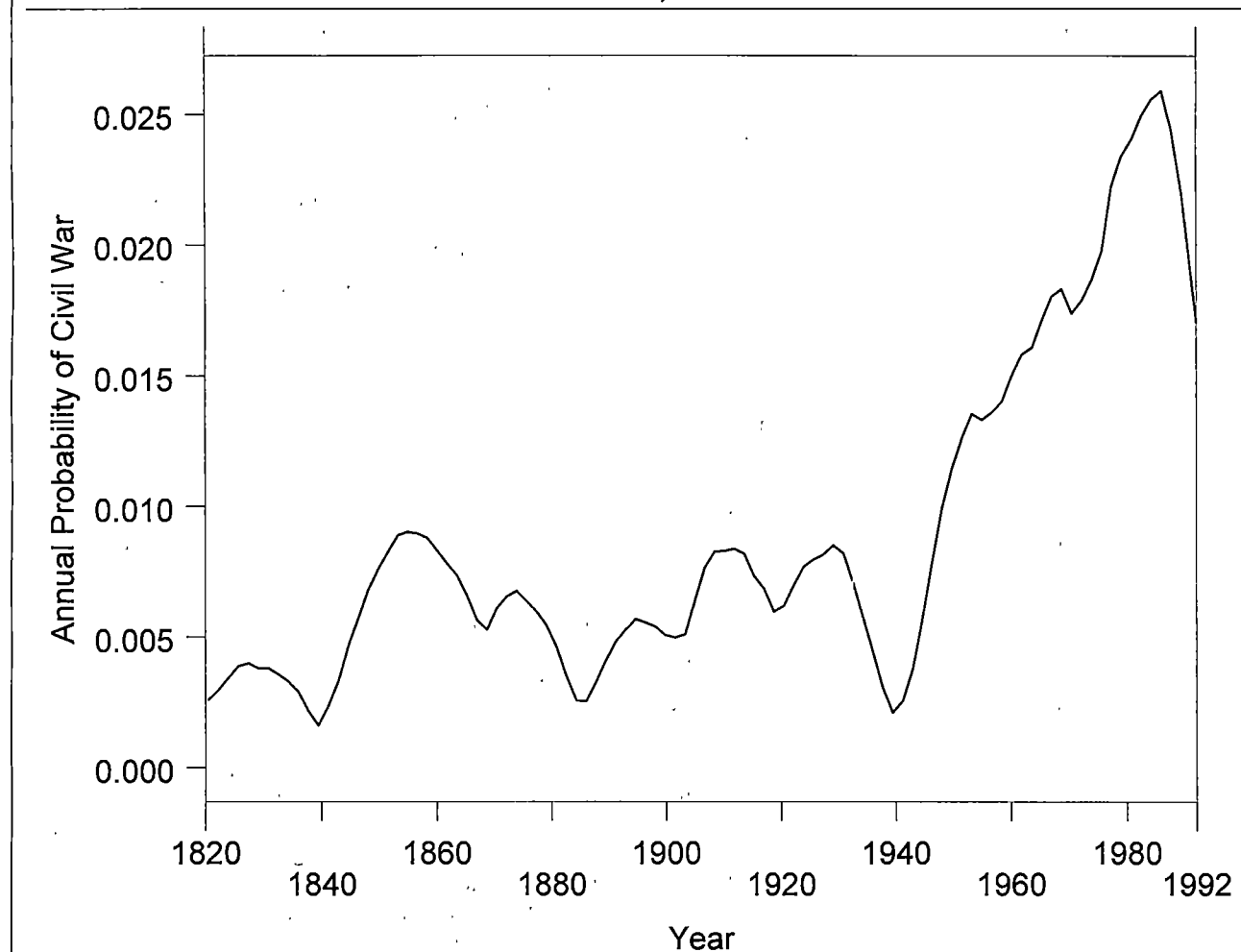
Note: The figure plots the hazard derived during the estimation in Table 2B for the baseline case. The hazard is partly a function of the frequency of civil wars relative to number of countries that are system members, and partly of the distribution of values for the explanatory variables. The low hazard around 1940 reflects the relatively low number of civil wars in that period (see Appendix B). The high hazard in the 1980s reflects a fairly high frequency of civil wars despite relatively low average values for explanatory variables that are estimated to increase the risk of civil war. The baseline is only defined from the first outbreak of civil war in our data set (i.e., from 1820)

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group constitutes half the population as in countries where it accounts for 95% of the population.[14] This result is significant at the .05 level. For both periods, countries with a recent civil war have a propensity for renewed violence. For instance, exactly one year after the previous conflict, the country is estimated to have a hazard of civil war 1.8 times higher than the baseline for the 1946–92 period. The parameter estimate is clearly significant, even more so in the longer than the shorter period.

We found only weak support for the idea that countries involved in an international war have a higher probability of civil war ($p = .090$ for 1946–92 was the strongest estimate). This weak result reflects the ambiguous findings of the literature on the internal-external conflict nexus (Heldt 1997; Levy 1989). An

international war may be an opportunity for dissenting groups to rebel, but it is also a means for the government to unite the country against an external enemy. The neighboring civil war variable is even less important. We find no clear evidence for the hypothesized diffusion of nearby conflict. Civil war occurs more frequently in some parts of the world than in others, but this is due to the clustering of other factors in the model, mainly development and regime type.

Direction and Magnitude of Regime Change

We have established that the relative risk of civil war is altered as the result of a regime change, but which type of shift has most effect, that is, toward democratization or autocratization? And is a large change more dangerous than a small one? The implications of the earlier results are not straightforward for the issue of direction and magnitude of regime change. Table 2 demonstrates that a new regime increases the risk of

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[14] This figure is calculated by computing the heterogeneity index for the cases: $1 - 0.5^2 = 0.0975$, and $1 - 0.95^2 = 0.75$. The risk of the first relative to the second is the exponential of the difference between the two: $\exp(0.79 \times (0.75 - 0.0975)) = 1.92$.

civil war when controlling for the level of democracy, but the model assumes that all types of regime change have the same effect on the probability of conflict. This is not necessarily the case.

To explore this issue, we divided proximity of regime change into five proximity variables: *Small/Large Democratization, Small/Large Autocratization*, and *Other Regime Change*. A large change is defined as an increase or decrease of six units or more, and a small change as two to five units. For instance, if the most recent shift was toward a much lower level of democracy (i.e., 6 or more units on the scale), the *Proximity of Large Autocratization* variable was set to exp(− days since regime change/α), and the others were set to 0. Throughout, we assume that the parameter α in the formula for the five proximity of regime change variables is independent of the initial risk, such that the half-life of the effect on the risk of civil war is the same for all regime change types. We also assume that the influence of a specific type of regime change is independent of the level of democracy before the change.[15] A small degree of democratization in an autocracy is assumed to have the same effect as a small degree of democratization in an intermediate regime, after we control for the effect of changing from one level to another.

Table 3 reports the results of replacing the simple regime change variable in Table 2 with the subdivided variable. In Table 3B, proximity of other regime change is the only variable that is not positive and significantly larger than zero. This is not surprising, since most of those "other" changes are minor or are accounted for by proximity of independence. In Table 3A, proximity of large democratization is also not significant. Of the remaining variables, large autocratization seems to be associated with the largest change in risk of civil war, but in neither period are the four parameter estimates for proximity of change toward either democracy or autocracy significantly different from one another. Thus, when controlling for the regime type toward which the change leads, there is no significant difference between the effects of democratization and autocratization.[16] As before, the contribution of regime change to the hazard function is greater for the shorter than the longer period. A comparison with Table 2 shows that the estimates for democracy and democracy squared remain virtually unchanged.

Because gaining independence is coded as "other regime change," there is a high correlation between that variable and proximity of independence (cf. Appendix A). The estimates for the latter are substantially higher in Table 3 than in Table 2. This is a result of the separation between the different categories of regime

change. The parameter estimates for the other control variables are unchanged, as is the estimate for democracy squared. Distinguishing the different directions or magnitudes of regime change adds very little information to the overall model.

Figure 3 portrays the combined effect of a regime change and an altered level of democracy on the risk of civil war. The relative risk is plotted as a function of the democracy scores before and exactly two years after the regime change. The darker the shade, the higher is the estimated risk. In the "valley" along the main diagonal, from the lower left to upper right corner of the figure, are countries that have had no regime changes. For them, the inverted U-curve at the forward end of Figure 1 describes the relative risk of civil war. Just to the right of this valley are polities that have experienced small democratizations. Farther right are those with large democratizations. The figure demonstrates our estimate that the risk of civil war is increased the most by changes that lead to a semidemocracy, in particular if the shift is a large autocratization (the darkest area, to the left of the valley).[17]

The example of South Korea illustrates how to interpret the figure. Until March 1981, Polity IIId reports South Korea as an autocracy, with a democracy-autocracy score of −8. Apart from a couple of minor alterations, the regime had existed for more than eight years. For our purposes, we treat a polity of this age as equal to one that has existed for an infinite number of years. We indicate that location in the figure as South Korea 1981. The estimated risk of civil war was then .47 relative to the baseline. On March 4, 1981, a small democratization from −8 to −6 took South Korea to the location labeled South Korea 1983, with an estimated risk of civil war of .87 relative to the baseline two years after the change. In 1985, democratization from −6 to −2 moved the location to South Korea 1987. The relative risk of civil war two years later is estimated to be 1.3. Finally, for February 26, 1988, Polity IIId reports South Korea changed from −2 to +10. This large democratization moved the country to the location labeled South Korea 1990, with the risk of civil war reduced to .45 relative to the baseline.

A DEMOCRATIC CIVIL PEACE?

Our analysis clearly confirms that the U-curve defines the relationship between democracy and civil war (hypothesis 1). Regimes that score in the middle range on the democracy-autocracy index have a significantly higher probability of civil war than either democracies or autocracies. As expected, we found no significant difference between the risk of civil war in harsh autocracies and in strong democracies (hypothesis 2). We have also shown that regime change clearly and strongly increases the probability of civil war in the short run (hypothesis 3), using the same control variables for the longer and the shorter period. Yet, regime change alone does not explain the higher level of civil

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[15] This is consistent with our interest in assessing the relative effects of intermediate position and political change.

[16] We also estimated a model with three regime change categories: proximity of democratization, proximity of autocratization, and proximity of other regime change. The estimates for 1946–92 were 1.84 for democratization and 1.49 for autocratization. The corresponding figures for 1816–1992 were 1.14 and 1.52. The merging of categories reduced the estimated standard errors but not sufficiently to assert that autocratization is more conducive to civil war than democratization.

[17] The difference between parameter estimates for autocratizations and democratizations were not statistically significant, however.

TABLE 3. Risk of Civil War by Level of Democracy and Subdivided Proximity of Regime Change Variable				
Explanatory Variables	$\hat{\beta}$	s.e.	p-value	Exp($\hat{\beta}$)
A. 1946–92				
Proximity of small democratization	1.54	0.67	0.011	4.66
Proximity of large democratization	1.22	0.95	0.10	3.39
Proximity of small autocratization	1.22	0.73	0.048	3.39
Proximity of large autocratization	2.63	0.75	<0.0005	13.9
Proximity of other regime change	0.29	0.62	0.32	1.33
Democracy	0.0016	0.024	0.47	1.002
Democracy squared	−0.012	0.0051	0.011	0.99
Proximity of civil war	1.14	0.34	0.001	3.13
Proximity of independence	2.52	1.06	0.009	12.4
International war in country	0.85	0.53	0.11	2.35
Neighboring civil war	0.16	0.33	0.31	1.18
Development	−0.48	0.16	0.001	0.62
Development squared	−0.066	0.036	0.032	0.94
Ethnic heterogeneity	0.80	0.40	0.022	2.23
Log-likelihood$_{null\ model}$	−292.17			
Log-likelihood$_{model}$	−252.00.			
Likelihood ratio index	0.14			
Number of countries	152			
Number of events	63			
B. 1816–1992				
Proximity of small democratization	1.04	0.61	0.044	2.84
Proximity of large democratization	1.37	0.71	0.028	3.93
Proximity of small autocratization	1.44	0.57	0.006	4.21
Proximity of large autocratization	1.91	0.84	0.012	6.73
Proximity of other regime change	0.12	0.46	0.40	1.13
Democracy	−0.010	0.020	0.29	0.99'
Democracy squared	−0.013	0.0027	<0.0005	0.99
Proximity of civil war	1.61	0.25	<0.0005	5.00
Proximity of independence	2.52	0.56	<0.0005	12.4
International war in country	0.25	0.41	0.27	1.28
Neighboring civil war	0.30	0.27	0.14	1.35
Log-likelihood$_{null\ model}$	−535.67			
Log-likelihood$_{model}$	−482.00			
Likelihood ratio index	0.10			
Number of countries	169			
Number of events	129			
Note: See note to Table 2.				

war in intermediate regimes. The two factors are partly overlapping, yet complementary. The democracy squared variable, which models the U-curve relationship between level of democracy and the risk of civil war, is clearly significant, even when controlling for the time elapsed since the most recent regime change (which supports hypothesis 4).

The hypotheses were tested with long-term data from the COW project (1816–1992), controlling for proximity of independence, civil war, and international war, as well as civil war in a neighboring country. They

were supported by those data and for the post–World War II period (1946–92) with additional controls for economic development and ethnic heterogeneity. The relationships were tested using a more appropriate statistical model than in previous studies, with more reliable estimates for statistical significance.

The direction of change has no discernible influence on the probability of civil war. This is not to say that democratization is as conducive to conflict as autocratization. The short-term effects are the same, but the long-term effects are different. As shown above and

FIGURE 3. Relative Risk of Civil War as Function of Democracy Index before or after Regime Change, 1816–1992

Relative Risk of Civil War

High

Democracy Index Before Regime Change

High

Democracy Index After Regime Change

South Korea 1990

South Korea 1987

South Korea 1983

South Korea 1981

Low

Low
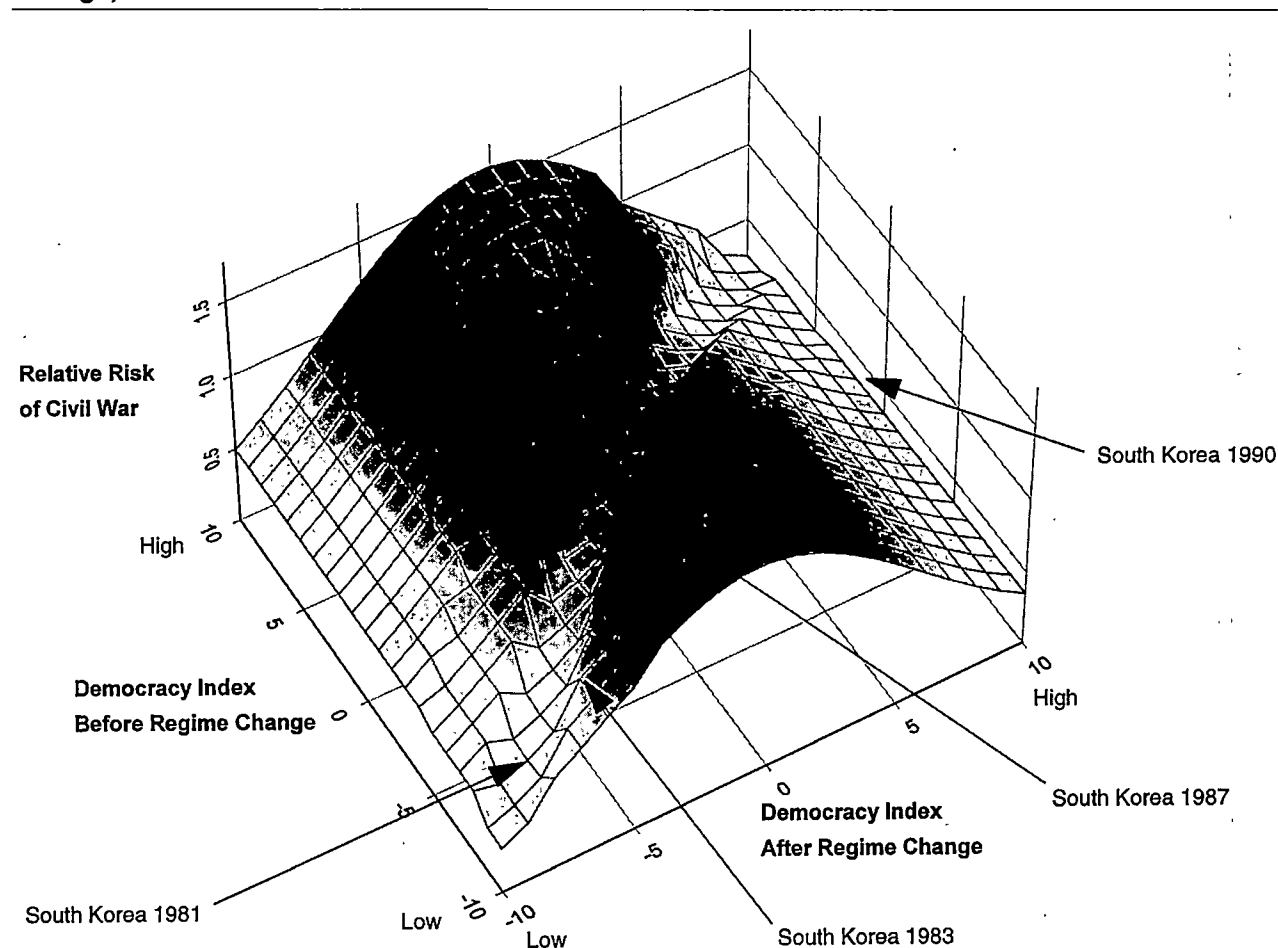
Note: The figure is based on the parameter estimates in Table 3B. The relative risks are calculated for a time two years after the regime change. The various shadings correspond to different ranges of values along the vertical axis. The darker the shading, the higher is the estimated risk of civil war.

noted by Gurr (1974), countries that have moved toward the middle category are the most likely to experience further regime change. Table 1 demonstrates that political stability increases as the democracy-autocracy scores approach both ends of the scale. Among countries that have moved toward the ends, the analysis in Gates et al. (2000) indicates that autocracies are somewhat less stable than democracies and are more likely to experience further change, which exposes them to the more risky middle position. The most reliable path to stable domestic peace in the long run is to democratize as much as possible. A change in that direction ensures the strongest ratchet effect in terms of consolidating political institutions and makes it less likely that the country will slide back into a state in which it is more prone to civil war.

Eventually, then, countries are more likely to end up at the democratic end of the scale. The conflict-generating effect of democratization when moving from autocracy to intermediacy produces violence in the short run only. In the long run these states, too, will attain civil peace, but if semidemocracies experience a succession of transitions in and around the middle

zone, it will take a long time before there is a net decrease in violence. A full assessment of the long-term effect of democratization requires a study of whether sequences of regime change and civil war form certain patterns.

Does the third wave of democracy reduce the specter of violent domestic conflict? The effect of political change depends heavily on the point of departure. In the short run, a democratizing country will have to live through an unsettling period of change. But if we focus on countries that are at least half-way toward complete democracy, the prospects for domestic peace are promising. There *is* a democratic civil peace, and it may be achieved in the short run in some countries. In the long run most states, possibly all, may reach this condition, especially if we take into account the higher survival rate of open societies, which are less likely to move once again through the doubly dangerous zone of intermediate democracy and political change. While totalitarian states may achieve a domestic peace of sorts, which may be characterized as the peace of a zoo, a democratic civil peace is likely not only to be more just but also more durable.

APPENDIX A. Correlation Matrix for Explanatory Variables, Pearson's r, 1946–92 Data

	Democracy	Democracy squared	Proximity of regime change	Proximity of small democratization	Proximity of large democratization	Proximity of small autocratization	Proximity of large autocratization	Proximity of other regime change	Proximity of civil war	Proximity of independence	International war in country	Neighboring civil war	Development	Development squared
Democracy squared	0.37													
Proximity of regime change	−0.05	−0.26												
Proximity of small democratization	−0.03	−0.18	0.34											
Proximity of large democratization	0.16	−0.08	0.33	−0.03										
Proximity of small autocratization	−0.08	−0.12	0.29	−0.03	−0.03									
Proximity of large autocratization	−0.11	−0.06	0.23	−0.03	−0.03	−0.02								
Proximity of other regime change	−0.06	−0.11	0.68	−0.07	−0.07	−0.07	−0.05							
Proximity of civil war	−0.14	−0.23	0.08	0.04	0.02	0.12	0.04	−0.01						
Proximity of independence	0.03	−0.09	0.34	−0.03	−0.03	0.00	−0.01	0.48	−0.07					
International war in country	0.00	−0.04	0.01	−0.02	−0.01	0.05	0.02	−0.01	0.09	−0.01				
Neighboring civil war	−0.10	−0.15	0.14	0.06	0.05	0.03	−0.03	0.11	0.07	0.05	0.06			
Development	0.45	0.39	−0.21	−0.07	−0.05	−0.12	−0.08	−0.12	−0.22	−0.12	0.01	−0.14		
Development squared	−0.23	−0.09	0.08	0.04	0.01	0.10	0.01	0.02	0.06	0.10	−0.01	0.03	−0.67	
Ethnic heterogeneity	−0.09	−0.22	0.08	0.05	0.02	0.02	0.01	0.05	0.04	0.03	−0.05	0.09	−0.19	0.15

Note: The correlation matrix refers to all countries observed once for each outbreak of civil war (n = 8,262).

APPENDIX B.   List of Civil Wars from the Correlates of War Data

COW Number	Country	Event Date	Democracy Index	Days Since Regime Change	Proximity of Regime Change	Development	Ethnic Heterogeneity
329	Two Sicilies	07.02.1820	−10	1,644	0.04	(Available only after	
325	Sardinia	03.10.1821	−10	1,895	0.03	World War II)	
230	Spain	12.01.1821	−4	699	0.27		
640	Ottoman Empire	06.14.1826	−10	3,817	0.00		
235	Portugal	07.01.1829	−3	2,217	0.01		
220	France	07.25.1830	−1	69	0.88		
70	Mexico	01.02.1832	−1	366	0.50		
230	Spain	07.15.1834	−6	3,970	0.00		
100	Colombia	07.15.1840	2	3,059	0.00		
230	Spain	05.15.1847	−2	720	0.26		
329	Two Sicilies	01.12.1848	−10	9,790	0.00		
220	France	02.22.1848	−1	6,490	0.00		
300	Austria-Hungary	03.13.1848	−6	9	0.98		
220	France	01.01.1851	6	1,042	0.14		
155	Chile	09.15.1851	−5	4,640	0.00		
135	Peru	12.21.1853	−1	5,241	0.00		
100	Colombia	04.17.1854	2	8,083	0.00		
135	Peru	10.31.1856	−1	6,286	0.00		
70	Mexico	02.15.1858	−3	3,665	0.00		
101	Venezuela	02.01.1859	−5	6,605	0.00		
710	China	01.01.1860	−6	21,914	0.00		
100	Colombia	05.15.1860	2	10,303	0.00		
2	United States	04.10.1861	8	2,506	0.01		
160	Argentina	04.02.1863	−3	3,622	0.00		
160	Argentina	12.15.1866	−3	4,975	0.00		
101	Venezuela	01.11.1868	−5	9,871	0.00		
160	Argentina	05.20.1870	−3	6,227	0.00		
230	Spain	04.20.1872	1	401	0.47		
160	Argentina	09.01.1874	−3	7,792	0.00		
2	United States	02.01.1876	10	1,676	0.04		
100	Colombia	11.15.1876	8	3,463	0.00		
740	Japan	01.29.1877	1	3,313	0.00		
160	Argentina	06.15.1880	−3	9,906	0.00		
100	Colombia	11.15.1884	8	6,385	0.00		
155	Chile	01.07.1891	5	920	0.17		
140	Brazil	02.02.1893	−3	1,174	0.11		
140	Brazil	09.06.1893	−3	1,390	0.07		
135	Peru	10.15.1894	2	3,059	0.00		
140	Brazil	10.01.1896	−3	823	0.21		
100	Colombia	09.01.1899	−3	4,775	0.00		
165	Uruguay	01.01.1904	−3	8,034	0.00		
365	Russia	01.22.1905	−10	32,528	0.00		
360	Rumania	03.15.1907	−6	2,448	0.01		
600	Morocco	08.01.1907	−6	22,126	0.00		
70	Mexico	11.20.1910	−9	10,945	0.00		
600	Morocco	01.15.1911	−6	23,389	0.00		
150	Paraguay	07.15.1911	−3	2,570	0.01		
710	China	10.11.1911	−6	17,998	0.00		
710	China	07.12.1913	2	488	0.40		
365	Russia/USSR	12.09.1917	−1	44	0.92		
375	Finland	01.28.1918	8	53	0.90		
310	Hungary	03.25.1919	−7	3	0.99		
91	Honduras	02.09.1924	5	1,318	0.08		
700	Afghanistan	03.15.1924	−6	1,680	0.04		
710	China	07.01.1926	−5	4,443	0.00		
70	Mexico	08.31.1926	−3	791	0.22		
700	Afghanistan	11.10.1928	−6	3,381	0.00		
710	China	03.01.1929	−5	5,417	0.00		
710	China	11.15.1930	−5	6,041	0.00		
92	El Salvador	01.22.1932	−9	50	0.91		
230	Spain	10.04.1934	7	1,029	0.14		
230	Spain	07.18.1936	7	1,682	0.04		
710	China	02.28.1947	−5	64	0.89		
150	Paraguay	03.07.1947	−9	2,577	0.01	−4.08	0.14
94	Costa Rica	03.12.1948	10	10,320	0.00	−1.56	0.04
775	Burma	09.15.1948	8	255	0.62	−4.28	0.48
100	Colombia	09.15.1949	−5	524	0.37	−1.58	0.71
850	Indonesia	05.31.1950	3	155	0.75	−2.71	0.82

APPENDIX B. (Continued)

COW Number	Country	Event Date	Democracy Index	Days Since Regime Change	Proximity of Regime Change	Development	Ethnic Heterogeneity
840	Philippines	09.01.1950	5	62	0.89	−2.29	0.04
145	Bolivia	04.09.1952	−5	5,805	0.00	−2.09	0.86
850	Indonesia	09.20.1953	0	1,131	0.12	−2.36	0.82
90	Guatemala	06.08.1954	2	1,275	0.09	−1.62	0.69
160	Argentina	06.15.1955	−9	2,382	0.01	0.03	0.15
850	Indonesia	12.15.1956	0	2,313	0.01	−1.93	0.82
660	Lebanon	05.09.1958	2	4,268	0.00	−0.62	0.14
40	Cuba	06.15.1958	−9	1,095	0.13	−0.23	0.47
645	Iraq	03.06.1959	−5	234	0.64	−0.70	0.45
817	Vietnam, Rep. of	01.01.1960	−3	1,528	0.06	−2.94	0.24
812	Laos	10.15.1960	−1	288	0.58	−4.02	0.66
615	Algeria	07.28.1962	−8	25	0.95	−1.44	0.00
678	Yemen Arab Rep.	11.15.1962	0	56	0.90	−4.79	0.19
625	Sudan	10.01.1963	−7	1,778	0.03	−2.76	0.76
517	Rwanda	11.15.1963	−5	501	0.39	−4.29	0.19
42	Dominican Republic	04.25.1965	−3	480	0.40	−1.58	0.47
90	Guatemala	10.01.1966	3	208	0.67	−1.57	0.68
710	China	01.15.1967	−9	259	0.61	−1.49	0.12
475	Nigeria	07.06.1967	−7	536	0.36	−2.61	0.65
775	Burma	01.01.1968	−7	1,645	0.04	−2.84	0.48
663	Jordan	09.17.1970	−9	4,935	0.00	−1.08	0.04
90	Guatemala	11.15.1970	1	258	0.61	−1.36	0.68
770	Pakistan	03.25.1971	3	2,272	0.01	−1.85	
780	Sri Lanka	04.06.1971	8	313	0.55	−2.13	0.50
516	Burundi	04.30.1972	−7	1,978	0.02	−4.79	0.29
840	Philippines	10.01.1972	−9	7	0.99	−1.27	0.04
552	Zimbabwe	12.28.1972	4	2,553	0.01	−0.32	0.10
770	Pakistan	01.23.1973	3	2,942	0.00	−1.81	
660	Lebanon	04.13.1975	5	1,699	0.04	−0.02	0.14
90	Guatemala	03.12.1978	−5	6	0.99	−1.46	0.68
700	Afghanistan	06.01.1978	−7	1,779	0.03	−2.79	0.75
630	Iran	09.03.1978	−10	8,582	0.00	0.24	0.71
93	Nicaragua	10.01.1978	−8	15,460	0.00	−0.84	0.51
811	Kampuchea	01.08.1979	−7	1,013	0.15	−6.06	0.15
92	El Salvador	07.01.1979	−6	860	0.20	−1.34	0.19
541	Mozambique	10.21.1979	−8	1,579	0.05	−2.27	0.31
475	Nigeria	12.18.1980	7	443	0.43	−1.98	0.63
630	Iran	06.06.1981	−6	855	0.20	−0.06	0.71
135	Peru	03.04.1982	7	583	0.33	−0.49	0.75
93	Nicaragua	03.18.1982	−5	378	0.49	−1.28	0.52
520	Somalia	04.21.1982	−7	4,564	0.00	−2.27	0.19
775	Burma	02.01.1983	−8	945	0.17	−2.76	0.48
780	Sri Lanka	07.25.1983	3	214	0.67	−1.95	0.45
625	Sudan	11.17.1983	−7	4,418	0.00	−2.49	0.74
475	Nigeria	02.02.1984	−7	32	0.94	−1.52	0.63
100	Colombia	03.15.1984	8	3,507	0.00	−0.20	0.71
750	India	01.01.1985	8	2,741	0.01	−1.38	0.48
645	Iraq	01.01.1985	−9	1,995	0.02	−0.41	0.45
680	Yemen Peoples Rep.	01.13.1986	−8	2,573	0.01		0.26
780	Sri Lanka	09.01.1987	3	1,713	0.04	−2.08	0.45
516	Burundi	08.18.1988	−7	7,932	0.00	−4.02	0.29
450	Liberia	12.01.1989	−6	1,976	0.02	−2.00	0.10
360	Rumania	12.21.1989	−8	4,712	0.00	1.50	0.23
517	Rwanda	09.30.1990	−7	6,295	0.00	−3.51	0.19
365	USSR	04.30.1991	0	321	0.54	1.91	0.73
345	Yugoslavia/Serbia	05.01.1991	−1	465	0.41	0.55	0.87
640	Turkey	07.10.1991	10	607	0.32	0.05	0.26
516	Burundi	11.23.1991	−4	68	0.88	−3.82	0.29
372	Georgia	12.25.1991	2	260	0.61	0.00	0.51
702	Tajikistan	05.01.1992	3	235	0.64	0.00	0.58
540	Angola	10.28.1992	−6	602	0.32	−2.40	0.86

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Analyzing Incomplete Political Science Data: An Alternative Algorithm for Multiple Imputation

GARY KING Harvard University
JAMES HONAKER Harvard University
ANNE JOSEPH Harvard University
KENNETH SCHEVE Yale University

*W*e propose a remedy for the discrepancy between the way political scientists analyze data with missing values and the recommendations of the statistics community. Methodologists and statisticians agree that "multiple imputation" is a superior approach to the problem of missing data scattered through one's explanatory and dependent variables than the methods currently used in applied data analysis. The discrepancy occurs because the computational algorithms used to apply the best multiple imputation models have been slow, difficult to implement, impossible to run with existing commercial statistical packages, and have demanded considerable expertise. We adapt an algorithm and use it to implement a general-purpose, multiple imputation model for missing data. This algorithm is considerably faster and easier to use than the leading method recommended in the statistics literature. We also quantify the risks of current missing data practices, illustrate how to use the new procedure, and evaluate this alternative through simulated data as well as actual empirical examples. Finally, we offer easy-to-use software that implements all methods discussed.

On average, about half the respondents to surveys do not answer one or more questions analyzed in the average survey-based political science article. Almost all analysts contaminate their data at least partially by filling in educated guesses for some of these items (such as coding "don't know" on party identification questions as "independent"). Our review of a large part of the recent literature suggests that approximately 94% use listwise deletion to eliminate entire observations (losing about one-third of their data, on average) when any one variable remains missing after filling in guesses for some.[1] Of course,

similar problems with missing data occur in nonsurvey research as well.

This article addresses the discrepancy between the treatment of missing data by political scientists and the well-developed body of statistical theory that recommends against the procedures we routinely follow.[2] Even if the missing answers we guess for nonrespondents are right on average, the procedure overestimates the certainty with which we know those answers. Consequently, standard errors will be too small. Listwise deletion discards one-third of cases on average, which deletes both the few nonresponses and the many responses in those cases. The result is a loss of valuable information at best and severe selection bias at worst.

[1] These data come from our content analysis of five years (1993–97)

of the *American Political Science Review*, the *American Journal of Political Science*, and the *British Journal of Political Science*. Among these articles, 203—24% of the total and about half the quantitative articles—used some form of survey analysis, and 176 of these were mass rather than elite surveys. In only 19% of the articles were authors explicit about how they dealt with missing values. By also asking investigators, looking up codebooks, checking computer programs, or estimating based on partial information provided, we were able to gather sufficient information regarding treatment of missing values for a total of 77% of the articles. Because the situation is probably not better in the other 23% of the articles without adequate reporting, both missing data practices and reporting problems need to be addressed. Our more casual examinations of other journals in political science and other social sciences suggest similar conclusions.

[2] This article is about item nonresponse, that is, respondents answer some questions but not others (or, in general, scattered cells in a data matrix are missing). A related issue is unit nonresponse: Some of the chosen sample cannot be located or refuse to be interviewed. Brehm (1993) and Bartels (1998) demonstrate that, with some interesting exceptions, the types of unit nonresponse common in political science data sets do not introduce much bias into analyses. Globetti (1997) and Sherman (2000) show that item nonresponse is a comparatively more serious issue in our field. The many other types of missing data can often be seen as a combination of item and unit nonresponse. Some examples include entire variables missing from one of a series of cross-sectional surveys (Franklin 1989; Gelman, King, and Liu 1998), matrix sampling (Raghunathan and Grizzle 1995), and panel attrition.

Some researchers avoid the problems missing data can cause by using sophisticated statistical models optimized for their particular applications (such as censoring or truncation models; see Appendix A). When possible, it is best to adapt one's statistical model specially to deal with missing data in this way. Unfortunately, doing so may put heavy burdens on the investigator, since optimal models for missing data differ with each application, are not programmed in currently available standard statistical software, and do not exist for many applications (especially when missingness is scattered throughout a data matrix).

Our complementary approach is to find a better choice in the class of widely applicable and easy-to-use methods for missing data. Instead of the default method for coping with the issue—guessing answers in combination with listwise deletion—we favor a procedure based on the concept of "multiple imputation" that is nearly as easy to use but avoids the problems of current practices (Rubin 1977).[3] Multiple imputation methods have been around for about two decades and are now the choice of most statisticians in principle, but they have not made it into the toolbox of more than a few applied statisticians or social scientists. In fact, aside from the experts, "the method has remained largely unknown and unused" (Schafer and Olsen 1998). The problem is only in part a lack of information and training. A bigger issue is that although this method is easy to use in theory, in practice it requires computational algorithms that can take many hours or days to run and cannot be fully automated. Because these algorithms rely on concepts of stochastic (rather than deterministic) convergence, knowing when the iterations are complete and the program should be stopped requires much expert judgment, but unfortunately, there is little consensus about this even among the experts.[4] In part for these reasons, no commercial software includes a correct implementation of multiple imputation.[5]

We begin with a review of three types of assumptions one can make about missing data. Then we demonstrate analytically the disadvantages of listwise deletion. Next, we introduce multiple imputation and our alternative algorithm. We discuss what can go wrong

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[3] The most useful modern work on the subject related to our approach is Schafer (1997), which we rely on frequently. Schafer provides a detailed guide to the analysis of incomplete multivariate data in a Bayesian framework. He presents a thorough explanation of the use of the IP algorithm. Little and Rubin (1987), Rubin (1987a), and Rubin (1996) provide the theoretical foundations for multiple imputation approaches to missing data problems.

[4] Although software exists to check convergences, there is significant debate on the adequacy of these methods (see Cowles and Carlin 1996; Kass et al. 1998).

[5] The public domain software that accompanies Schafer's (1997) superb book implements monotone data augmentation by the IP algorithm, the best currently available approach (Liu, Wong, and Kong 1994; Rubin and Schafer 1990). The commercial programs Solas and SPlus have promised implementations. SPSS has released a missing data module, but the program only produces sufficient statistics under a multivariate normality model (means, variances, and covariates), so data analysis methods that require raw data cannot be used. Furthermore, it adds no uncertainty component, which produces standard errors biased toward zero.

TABLE 1. Three Missingness Assumptions

Assumption	Acronym	You Can Predict M with:
Missing completely at random	MCAR	—
Missing at random	MAR	D_{obs}
Nonignorable	NI	D_{obs} and D_{mis}

and provide Monte Carlo evidence that shows how our method compares with existing practice and how it is equivalent to the standard approach recommended in the statistics literature, except that it runs much faster. We then present two examples of applied research to illustrate how assumptions about and methods for missing data can affect our conclusions about government and politics.

ASSUMPTIONS ABOUT MISSINGNESS

We now introduce three assumptions about the process by which data become missing. Briefly in the conclusion to this section and more extensively in subsequent sections, we will discuss how the various methods crucially depend upon them (Little 1992).

First, let D denote the data matrix, which includes the dependent Y and explanatory X variables: $D = \{Y, X\}$. If D were fully observed, a standard statistical method could be used to analyze it, but in practice, some elements of D are missing. Define M as a missingness indicator matrix with the same dimensions as D, but there is a 1 in each entry for which the corresponding entry in D is observed, or a 0 when missing. Elements of D for which the corresponding entry in M is 0 are unobserved but do "exist" in a specific metaphysical sense. For example, everyone has a (positive or negative) income, even if some prefer not to reveal it in an interview. In some cases, however, "I don't know" given in response to questions about the national helium reserve or the job performance of the Secretary of Interior probably does not mean the respondent is hiding something, and it should be treated as a legitimate answer to be modeled rather than a missing value to be imputed. We focus on missing data for which actual data exist but are unobserved, although imputing values that the respondent really does not know can be of interest in specific applications, such as predicting how people would vote if they were more informed (Bartels 1996). Finally, let D_{obs} and D_{mis} denote observed and missing portions of D, respectively, so $D = \{D_{obs}, D_{mis}\}$.

Standard terminology describing possible missingness assumptions is unintuitive (for historical reasons). In Table 1 we try to clarify the assumptions according to our ability to predict the values of M (i.e., which values of D will be missing) (Rubin 1976). For example, missing values in processes that are *missing completely at random* (MCAR) cannot be predicted any better with information in D, observed or not. More formally, M is independent of D: $P(M|D) = P(M)$. An example of an MCAR process is one in which respondents

decide whether to answer survey questions on the basis of coin flips. Of course, the MCAR assumption rarely applies: If independents are more likely to decline to answer a vote preference or partisan identification question, then the data are not MCAR.

For *missing at random* (MAR) processes, the probability that a cell value is missing may depend on D_{obs} but (after controlling for D_{obs}) must be independent of D_{mis}. Formally, M is independent of D_{mis}: $P(M|D) = P(M|D_{obs})$. For example, if Democratic Party identifiers are more likely to refuse to answer the vote choice question, then the process is MAR so long as party identification is a question to which at least some people respond. Similarly, if those planning to vote for Democrats do not answer the vote choice question as frequently as those planning to vote for Republicans, the process is not MCAR, but it would be MAR if this difference can be predicted with any other variables in the data set (such as ideology, issue positions, income, and education). The prediction required is not causal; for example, the vote data could be used whether or not the vote causes or is caused by party identification. To an extent then, the analyst, rather than the world that generates the data, controls the degree to which the MAR assumption fits. It can be made to fit the data by including more variables in the imputation process to predict the pattern of missingness.

Finally, if the probability that a cell is missing depends on the unobserved value of the missing response, the process is *nonignorable* (NI). Formally, M is not independent of D: $P(M|D)$ does not simplify. An example occurs when high-income people are more likely to refuse to answer survey questions about income *and* when other variables in the data set cannot predict which respondents have high income.[6]

The performance of different methods of analyzing incomplete data under MCAR, MAR, or NI depends upon the ultimate goals of the analysis. We consider various situations in some detail in subsequent sections, but a few general statements are possible at this stage. First, inferences from analyses using listwise deletion are relatively inefficient, no matter which assumption characterizes the missingness, and they are also biased unless MCAR holds. Inferences based on multiple imputation are more efficient than listwise deletion (since no observed data are discarded), and they are not biased under MCAR or MAR (Little and Rubin 1989; Little and Schenker 1995). Both listwise deletion and basic multiple imputation approaches can be biased under NI, in which case additional steps must be taken, or different models must be chosen, to ensure valid inferences. Thus, multiple imputation will normally be better than, and almost always not worse than, listwise deletion. We discuss below the unusual configuration of assumptions, methods, and analysis models

for which listwise deletion can outperform multiple imputation.

In many situations, MCAR can be rejected empirically in favor of MAR. By definition, however, the presence or absence of NI can never be demonstrated using only the observed data. Thus, in most circumstances, it is possible to verify whether multiple imputation will outperform listwise deletion, but it is not possible to verify absolutely the validity of any multiple imputation model (or, of course, any statistical model). In sum, these methods, like all others, depend on assumptions that, if wrong, can lead the analyst astray, so careful thought should always go into the application of these assumptions.

DISADVANTAGES OF LISTWISE DELETION

Whenever it is possible to predict the probability that a cell in a data matrix is missing (using D_{obs} or D_{mis}), the MCAR assumption is violated, and listwise deletion may generate biased parameter estimates. For example, listwise deletion can bias conclusions if those who think of themselves as independents are less likely to respond to a party identification question, or if better educated people tend to answer issue opinion questions, or if less knowledgeable voters are less likely to reveal their voting preferences. These patterns might each be MAR or NI, but they are not MCAR. Listwise deletion can result in different magnitudes or signs of causal or descriptive inferences (Anderson, Basilevsky, and Hum 1983). It does not always have such harmful effects; sometimes the fraction of missing observations is small or the assumptions hold sufficiently well so that the bias is not large.

In this section, we quantify the efficiency loss due to listwise deletion under the optimistic MCAR assumption, so that no bias exists. We consider estimating the causal effect of X_1 on Y, which we label β_1, and for simplicity suppose that neither variable has any missing data. One approach might be to regress Y on X_1, but most scholars would control for a list of potential confounding influences, variables we label X_2. As critics we use omitted variables as the first line of attack, and as authors we know that controlling for more variables helps protect us from potential criticism; from this perspective, the more variables in X_2 the better.

The goal is to estimate β_1 in the regression $E(Y) = X_1\beta_1 + X_2\beta_2$. If X_2 contains no missing data, then even if X_2 meets the rules for causing omitted variable bias (i.e., if the variables in X_2 are correlated with and causally prior to X_1 and affect Y), omitting it is still sometimes best. That is, controlling will reduce bias but may increase the variance of β_1 (since estimating additional parameters puts more demands on the data). Thus, the mean square error (a combination of bias and variance) may in some cases increase by including a control variable (Goldberger 1991, 256). Fortunately, since we typically have a large number of observations, adding an extra variable does not do much harm so long as it does not introduce substantial colinearity, and we often include X_2.

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[6] Missingness can also be NI if the parameters of the process that generate D are not distinct from those that generate M, even if it is otherwise MAR. In the text, for expository simplicity, we assume that if a data set meets the MAR assumption, it also meets the distinctness condition and is therefore ignorable.

The tradeoff between bias and variance looms larger when data are missing. Missing data will normally be present in Y, X_1, and X_2, but suppose for simplicity there is MCAR item nonresponse only in λ fraction of the n observations in X_2. Ideally, we would observe all of X_2 (i.e., $\lambda = 0$) and estimate β_1 with the complete data:

Infeasible Estimator: Regress Y on X_1 and a fully observed X_2, and use the coefficient on X_1, which we denote b_1^I.

In contrast, when data are missing ($0 < \lambda < 1$), most analysts consider only two estimators:

Omitted Variable Estimator: Omit X_2 and estimate β_1 by regressing Y on X_1, which we denote b_1^O.

Listwise Deletion Estimator: Perform listwise deletion on Y, X_1, and X_2, and then estimate the vector β_1 as the coefficient on X_1 when regressing Y on X_1 and X_2, which we denote b_1^L.

The omitted variable estimator (b_1^O) risks bias, and the listwise deletion estimator (b_1^L) risks inefficiency (and bias except in the "best" case in which MCAR holds). Presumably because the risks of omitted variable bias are better known than the risks of listwise deletion, when confronted with this choice most scholars opt for listwise deletion. We quantify these risks with a formal proof in Appendix B and discuss the results here. If MSE(a) is the mean square error for estimator a, then the difference MSE(b_1^L) − MSE(b_1^O) is how we assess which method is better. When this difference is positive, b_1^O has lower mean square error and is therefore better than b_1^L; when it is negative, b_1^L is better. The problem is that this difference is often positive and large.

We need to understand when this mean square error difference will take on varying signs and magnitudes. The actual difference is a somewhat complicated expression that turns out to have a very intuitive meaning:

$$\text{MSE}(b_1^L) - \text{MSE}(b_1^O) = \frac{\lambda}{1-\lambda} V(b_1^I)$$
$$+ F[V(b_2^I) - \beta_2\beta_2']F'. \quad (1)$$

The second term on the right side of equation 1 is the well-known tradeoff between bias and variance when no data are missing (where F are regression coefficients of X_2 on X_1, and b_2^I is the coefficient on X_2 in the infeasible estimator). The key here is the first term, which is the extra mean square error due to listwise deletion. Because this first term is always positive, it causes the comparison between the two estimators to tilt farther away from listwise deletion as the fraction of missing data (λ) grows.

To better understand equation 1, we estimate the average λ value in political science articles. Because of the bias-variance tradeoff, those who try to fend off more possible alternative explanations have more control variables and thus larger fractions of observations lost. Although, on average, slightly less than one-third of observations are lost when listwise deletion is used,[7]

the proportion can be much higher. In the papers and posters presented at the 1997 annual meeting of the Society for Political Methodology, for example, the figure exceeded 50% on average and in some cases was more than 90%.[8] Because scholars usually drop some variables to avoid extreme cases of missingness, the "right" value of λ for our purposes is larger than the observed fraction. We thus study the consequences of setting $\lambda = 1/2$, which means the first term in equation 1 reduces to $V(b_1^I)$. The MSE also depends on the second term, which can be positive or negative depending on the application. For simplicity, consider the case in which this second term is zero (such as when $V(b_2^I) = \beta_2\beta_2'$, or X_1 and X_2 are uncorrelated). Finally, we take the square root of the MSE difference to put it in the interpretable units of the average degree of error. The result is that the average error difference is SE(b_1^I), the standard error of b_1^I.

If these assumptions are reasonable, then the point estimate in the average political science article is about one standard error farther away from the truth because of listwise deletion (as compared to omitting X_2 entirely). This is half the distance from no effect to what usually is termed "statistically significant" (i.e., two standard errors from zero).[9] Of course, this is the average absolute error: Point estimates in some articles will be too high, in others too low. In addition, we are using the standard error here as a metric to abstract across applications with different meanings, but in any one application the meaning of the expression depends on how large the standard error is relative to changes in the variables. This relative size in large part depends on the original sample size and cases lost to listwise deletion. Omitted variable bias, in contrast, does not diminish as the sample size increases.

Although social scientists rarely choose it, omitted variable bias is often preferable, if only it and listwise deletion are the options. One cannot avoid missing value problems since they usually affect all variables rather than only potential control variables. Moreover, because this result relies on the optimistic MCAR assumption, the degree of error will often be more than one standard error, and its direction will vary as a function of the application, pattern of missingness, and model estimated (Globetti 1997; Sherman 2000). Fortunately, better methods make this forced choice between suboptimal procedures unnecessary.

A METHOD FOR ANALYZING INCOMPLETE DATA

We now describe a general definition of multiple imputation, a specific model for generating the impu-

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[7] This estimate is based on our content analysis of five years of the

[8] This estimate is based on 13 presented papers and more than 20 posters.

[9] This is one of the infeasible estimator's standard errors, which is 71% of the listwise deletion estimator's standard error (or, in general, $\sqrt{\lambda} \times$ SE(b_1^L)). Calculated standard errors are correct under MCAR but larger than those for better estimators given the same data, and they are wrong if MCAR does not hold.

tations, and the existing computational algorithms and our alternative. We also make several theoretical clarifications and consider potential problems.

Definition of Multiple Imputation

Multiple imputation involves imputing m values for each missing item and creating m completed data sets. Across these completed data sets, the observed values are the same, but the missing values are filled in with different imputations to reflect uncertainty levels. That is, for missing cells the model predicts well, variation across the imputations is small; for other cases, the variation may be larger, or asymmetric, to reflect whatever knowledge and level of certainty is available about the missing information. Analysts can then conveniently apply the statistical method they would have used if there were no missing values to each of the m data sets, and use a simple procedure that we now describe to combine the m results. As we explain below, m can be as small as 5 or 10.

First estimate some Quantity of interest, Q, such as a univariate mean, regression coefficient, predicted probability, or first difference in each data set j ($j = 1, \ldots, m$). The overall point estimate $\bar{q}$ of Q is the average of the m separate estimates, q_j:

$$\bar{q} = \frac{1}{m} \sum_{j=1}^{m} q_j. \tag{2}$$

Let $SE(q_j)$ denote the estimated standard error of q_j from data set j, and let $S_q^2 = \sum_{j=1}^{m} (q_j - \bar{q})^2/(m - 1)$ be the sample variance across the m point estimates. Then, as shown by Rubin (1987a), the variance of the multiple imputation point estimate is the average of the estimated variances from *within* each completed data set, plus the sample variance in the point estimates *across* the data sets (multiplied by a factor that corrects for bias because $m < \infty$):

$$SE(q)^2 = \frac{1}{m} \sum_{j=1}^{m} SE(q_j)^2 + S_q^2(1 + 1/m). \tag{3}$$

If, instead of point estimates and standard errors, simulations of q are desired, we create $1/m$th the needed number from each completed data set (following the usual procedures; see King, Tomz, and Wittenberg 2000) and combine them into one set of simulations.

An Imputation Model

Implementing multiple imputation requires a statistical model from which to compute the m imputations for each missing value in a data set. Our approach assumes that the data are MAR, conditional on the imputation model. The literature on multiple imputation suggests that in practice most data sets include sufficient information so that the additional outside information in an application-specific NI model (see Appendix A) will not add much and may be outweighed by the costs of

nonrobustness and difficulty of use (Rubin 1996; Schafer 1997). Although this is surely not true in every application, the advantages make this approach an attractive option for a wide range of potential uses. The MAR assumption can also be made more realistic by including more informative variables and information in the imputation process, about which more below. Finally, note that the purpose of an imputation model is to create predictions for the distribution of each of the missing values, not causal explanation or parameter interpretation.

One model that has proven useful for missing data problems in a surprisingly wide variety of situations assumes that the variables are jointly multivariate normal. This model obviously is an approximation, as few data sets have variables that are all continuous and unbounded, much less multivariate normal. Yet, many researchers have found that it works as well as more complicated alternatives specially designed for categorical or mixed data (Ezzati-Rice et al. 1995; Graham and Schafer 1999; Rubin and Schenker 1986; Schafer 1997; Schafer and Olsen 1998). Transformations and other procedures can be used to improve the fit of the model.[10] For our purposes, if there exists information in the observed data that can be used to predict the missing data, then multiple imputations from this normal model will almost always dominate current practice. Therefore, we discuss only this model, although the algorithms we discuss might also work for some of the more specialized models as well.

For observation i ($i = 1, \ldots, n$), let D_i denote the vector of values of the p (dependent Y_i and explanatory X_i) variables, which if all observed would be distributed normally, with mean vector μ and variance matrix Σ. The off-diagonal elements of Σ allow variables within D to depend on one another. The likelihood function for complete data is:

$$L(\mu, \Sigma|D) \propto \prod_{i=1}^{n} N(D_i|\mu, \Sigma). \tag{4}$$

By assuming the data are MAR, we form the observed data likelihood. The procedure is exactly as for application-specific methods (equations 12–13 in Appendix A, where with the addition of a prior this likelihood is proportional to P ($D_{obs}|\theta$)). We denote $D_{i,obs}$ as the observed elements of row i of D, and $\mu_{i,obs}$ and $\Sigma_{i,obs}$ as the corresponding subvector and submatrix of μ and Σ (which do not vary over i), respectively. Then, because the marginal densities are normal, the observed data likelihood is

$$L(\mu, \Sigma|D_{obs}) \propto \prod_{i=1}^{n} N(D_{i,obs}|\mu_{i,obs}, \Sigma_{i,obs}). \tag{5}$$

The changing compositions of $D_{i,obs}$, $\mu_{i,obs}$, and $\Sigma_{i,obs}$ over i make this a complicated expression to evaluate,

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[10] Most variables in political science surveys are ordinal variables with four to seven values, which are reasonably well approximated by the normal model, at least for the purpose of making imputations.

although for clarity of presentation we have omitted several computational conveniences that can help (see Schafer 1997, 16).[11]

The multivariate normal specification implies that the missing values are imputed linearly. Thus, we create an imputed value the way we would usually simulate from a regression. For example, let $\tilde{D}_{ij}$ denote a simulated value for observation i and variable j, and let $D_{i,-j}$ denote the vector of values of all observed variables in row i, except variable j. The coefficient β from a regression of D_j on the variables in D_{-j} can be calculated directly from elements of μ and Σ, since they contain all available information in the data under this model. Then we use this equation to create an imputation:

$$\tilde{D}_{ij} = D_{i,-j}\tilde{\beta} + \tilde{\epsilon}_i, \qquad (6)$$

where $\sim$ indicates a random draw from the appropriate posterior. Thus, random draws of $\tilde{D}_{ij}$ are linear functions of the other variables whenever they are observed $D_{i,-j}$, of estimation uncertainty due to not knowing β (i.e., μ and Σ) exactly, and of fundamental uncertainty $\tilde{\epsilon}_i$ (i.e., since Σ is not a matrix of zeros). If we had an infinite sample, $\tilde{\beta}$ could be replaced with the fixed β, but there would still be uncertainty generated by the world, ϵ_i. The computational difficulty is taking random draws from the posterior of μ and Σ.

Equation 6 can be used to generate imputations for categorical variables by rounding off to the nearest valid integer (as recommended by Schafer 1997). A slightly better procedure draws from a multinominal or other appropriate discrete distribution with mean equal to the normal imputation. For example, to impute a 0/1 variable, take a Bernoulli draw with mean equal to the imputation (truncated to [0,1] if necessary). That is, we impute a 1 with probability equal to the continuous imputation, 0 otherwise.

Computational Algorithms

Computing the observed data likelihood in equation 5, and taking random draws from it, is computationally infeasible with classical methods. Even maximizing the function takes inordinately long with standard optimization routines. In response to such difficulties, the _Imputation-Posterior_ (IP) and _Expectation-Maximization_ (EM) algorithms were devised and subsequently applied to this problem.[12] From the perspective of statisticians, IP is now the gold standard of algorithms for multivariate normal multiple imputations, in large part because it can be adapted to numerous specialized models. Unfortunately, from the perspective of users, it is slow and hard to use. Because IP is based on Markov Chain Monte Carlo (MCMC) methods, considerable expertise is needed to judge convergence, and there is

no agreement among experts about this except for special cases. IP has the additional problem of giving dependent draws, so we need adaptations because multiple imputation requires that draws be independent. In contrast, EM is a fast deterministic algorithm for finding the maximum of the likelihood function, but it does not yield the rest of the distribution. We outline these algorithms and refer the reader to Schafer (1997) for a clear presentation of the computational details and historical development.

We also will discuss two additional algorithms, which we call EMs (EM with _s_ampling) and EMis (EM with importance _r_esampling), respectively. Our recommended procedure, EMis, is quite practical: It gives draws from the same posterior distribution as IP but is considerably faster, and, for this model, there appear to be no convergence or independence difficulties. Both EMs and EMis are made up of standard parts and have been applied to many problems outside the missing data context. For missing data problems, EMs has been used, and versions of EMis have been used for specialized applications (e.g., Clogg et al. 1991). EMis also may have been used for problems with general patterns of missingness, although we have not yet located any (and it is not mentioned in the most recent exposition of practical computational algorithms, Schafer 1997). In any event, we believe this procedure has widespread potential (see Appendix C for information about software we have developed).

IP. A version of the data augmentation algorithm of Tanner and Wong (1987), IP enables us to draw random simulations from the multivariate normal observed data posterior $P(D_{mis}|D_{obs})$ (see Schafer 1997, 72ff). The basic idea is that drawing directly from this distribution is difficult, but "augmenting" it by conditioning on additional information makes the problem easier. Because this additional information must be estimated, the procedure has two steps that are carried out iteratively. First, imputations, $\tilde{D}_{mis}$, are drawn from the conditional predictive distribution of the missing data in what is called the imputation step:

$$\tilde{D}_{mis} \sim P(D_{mis}|D_{obs}, \tilde{\mu}, \tilde{\Sigma}). \qquad (7)$$

On the first application of equation 7, guesses are used for the additional information, $\tilde{\mu}$ and $\tilde{\Sigma}$. Then, new values of the parameters μ and Σ are drawn from their posterior distribution, which depends on the observed data and the present imputed values for the missing data. This is called the posterior step:

$$\tilde{\mu}, \tilde{\Sigma} \sim P(\mu, \Sigma|D_{obs}, \tilde{D}_{mis}). \qquad (8)$$

This procedure is iterated, so that over time draws of $\tilde{D}_{mis}$, and $\tilde{\mu}$ and $\tilde{\Sigma}$, come increasingly from their actual distributions independent of the starting values.

The advantage of IP is that the distributions are exact, but convergence to these distributions is known to occur only after an infinite number of iterations. The belief is that after a suitably long "burn-in period" (a number of iterations that are performed and discarded before continuing), perhaps recognizable by various

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[11] Since the number of parameters $p(p + 3)/2$ increases rapidly with the number of variables p, priors help avoid overfitting and numerical instability in all the algorithms discussed here.

[12] Gelman et al. (1995), Jackman (2000), McLachlan and Krishan (1997), and Tanner (1996) provide excellent introductions to the literature on these algorithms and on Bayesian methods more generally.

diagnostics, convergence will have occurred, after which additional draws will come from the posterior. Unfortunately, experts disagree about how to assess convergence of this and other MCMC methods (Cowles and Carlin 1996; Kass et al. 1998).

In order to use the relatively simple equations 2 and 3 in combining the separate multiply imputed analyses, imputations must be statistically independent, but this is not a characteristic of successive draws from Markov chain methods such as IP. Some scholars reduce dependence by using every rth random draw from IP (where r is determined by examining the autocorrelation function of each of the parameters), but Schafer (1997), following Gelman and Rubin (1992), recommends addressing both problems by creating one independent chain for each of the m desired imputations, with starting values drawn randomly from an overdispersed approximation distribution. The difficulty with taking every rth draw from one chain is the interpretation of autocorrelation functions (which requires analysts of cross-sectional data to be familiar with time-series methods). The difficulty of running separate chains is that the increase in run time, due to the need to burn in iterations to ensure convergence for each chain, is typically greater than the m times r iterations saved by not needing multiple draws from any one chain.

EM. The EM algorithm (Dempster, Laird, and Rubin 1977; McLachlan and Krishnan 1996; Orchard and Woodbury 1972) works like IP except that random draws from the entire posterior are replaced with deterministic calculations of posterior means. The draw of $\tilde{D}_{mis}$ in equation 7 is replaced with each missing cell's predicted value. The random draw of $\tilde{\mu}$ and $\tilde{\Sigma}$ in equation 8 is replaced with the maximum posterior estimate. In simple cases, this involves running regressions to estimate β, imputing the missing values with a predicted value, reestimating β, and iterating until convergence. The result is that both the imputations and the parameters computed are the single (maximum posterior) values, rather than a whole distribution.

The advantages of EM are that it is fast (relative to other options), it converges deterministically, and the objective function increases with every iteration. Like every numerical optimization algorithm, EM can sometimes settle on a local maximum, and for some problems convergence is slow, although these do not seem to be insurmountable barriers in most political science data. The more serious disadvantage of EM is that it yields only maximum values, rather than the entire density. It is possible to use EM to produce multiple imputations by treating point estimates of μ and Σ as if they were known with certainty. This means that estimation uncertainty is ignored, but the fundamental variability is included in the imputations. EM for multiple imputation works reasonably well in some instances, but ignoring estimation uncertainty means its standard errors are generally biased downward, and point estimates for some quantities will be biased.

EMs. Our strategy is to begin with EM and to add back in estimation uncertainty so we get draws from the correct posterior distribution of D_{mis}. The problem is that it is difficult to draw from the posterior of μ and Σ. We approach this problem in two different ways. In this section, we use the asymptotic approximation (e.g., Tanner 1996, 54–9), which we find works as expected— well in large data sets and poorly in small ones.

To create imputations with this method, which we denote EMs, we first run EM to find the maximum posterior estimates of the parameters, $\hat{\theta} = \text{vec}(\hat{\mu}, \hat{\Sigma})$ (where the vec(·) operator stacks the unique elements). Then we compute the variance matrix, $V(\hat{\theta})$.[13] Next we draw a simulated θ from a normal with mean $\hat{\theta}$ and variance $V(\hat{\theta})$. From this, we compute $\tilde{\beta}$ deterministically, simulate $\tilde{\epsilon}$ from the normal, and substitute these values into equation 6 to generate an imputation. The entire procedure after the EM step and variance computation is repeated m times for the necessary imputations.

EMs is very fast, produces independent imputations, converges nonstochastically, and works well in large samples. For small samples, for data with many variables relative to the number of observations, or for highly skewed categorical data, EMs can be misleading in the shape or variance of the distribution. As a result, the standard errors of the multiple imputations, and ultimately of the quantities of interest, may be biased.

EMis. EM finds the mode well, and EMs works well for creating fast and independent imputations in large samples, but it performs poorly with small samples or many parameters. We can improve EMs with a round of importance resampling (or "sampling importance/ resampling"), an iterative simulation technique not based on Markov chains, to enhance small sample performance (Gelfand and Smith 1990; Gelman et al. 1995; Rubin 1987a, 192–4, 1987b; Tanner 1996; Wei and Tanner 1990).

EMis follows the same steps as EMs except that draws of θ from its asymptotic distribution are treated only as first approximations to the true (finite sample) posterior. We also put the parameters on unbounded scales, using the log for the standard deviations and Fisher's z for the correlations, to make the normal approximation work better with smaller sample sizes. We then use an acceptance-rejection algorithm by keeping draws of θ with probability proportional to the "importance ratio"—the ratio of the actual posterior to the asymptotic normal approximation, both evaluated at θ—and discarding the rest. Without priors, the importance ratio is

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[13] To compute the variance matrix, we generally use the outer product gradient because of its speed. Other options are the inverse of the negative Hessian, which is asymptotically the same and supposedly somewhat more robust in real problems; "supplemented EM," which is somewhat more numerically stable but not faster; and White's estimator, which is more robust but slower. We have also developed an iterative simulation-based method that seems advantageous in speed and numerical stability when p is large.

$$IR = \frac{L(\hat{\theta}|D_{obs})}{N(\bar{\theta}|\hat{\theta}, V(\hat{\theta}))}. \qquad (9)$$

We find that the normal approximation is usually good enough even in small, nonnormal samples so that the algorithm operates quickly.[14] In the final step, these draws of $\hat{\theta}$ are used with equation 6 to produce the desired m imputations.

EMis has all the advantages of IP, since it produces multiple imputations from the exact, finite sample posterior distribution. It is fast, does not rely on Markov chains, and produces the required fully independent imputations. Importance resampling, on which EMis is based, does not work well for all likelihood functions, especially when the normal density is not a good first approximation; for the present likelihood, however, our extensive experimentation with a wide variety of data types has not revealed any systematic differences when compared to runs of IP with immense numbers of iterations (so that judging MCMC convergence of IP is not as much of an issue). Our software includes the full range of standard diagnostics in case a problem arises that we have not foreseen. It also includes other approaches (IP, EM, EMs, and others), since our suggestion for improving methodological practice in political science is *not* to rely exclusively on EMis. Rather, we argue that any appropriately applied multiple imputation algorithm will generally outperform current incomplete data analysis practices.

Theoretical Clarifications and Common Misconceptions

It has been shown that multiple imputation inferences are statistically valid from both Bayesian and frequentist perspectives (Brownstone 1991; Meng 1994a; Rubin 1987a, 1996; Schafer 1997; Schenker and Welsh 1988). Since there is some controversy over the strength and applicability of the assumptions involved from a frequentist perspective, we focus on the far simpler Bayesian version. This version also encompasses the likelihood framework, which covers the vast majority of social science statistical models.

The fundamental result, for some chosen quantity Q to be estimated, involves approximating the correct posterior $P(Q|D_{obs})$. We would get this from an optimal application-specific method, with an approach based on the "completed" data $P(Q|D_{obs}, \bar{D}_{mis})$, that is filled in with imputations $\bar{D}_{mis}$ drawn from the conditional predictive density of the missing data $P(D_{mis}|D_{obs})$. Under MAR, we know that averaging $P(Q|D_{obs}, \bar{D}_{mis})$ over $\bar{D}_{mis}$ gives exactly $P(Q|D_{obs})$:

$$P(Q|D_{obs}) = \int P(Q|D_{obs}, D_{mis})P(D_{mis}|D_{obs})dD_{mis}. \qquad (10)$$

This integral can be approximated with simulation. To draw a random value of $\tilde{Q}$ from $P(Q|D_{obs})$, draw independent random imputations of $\bar{D}_{mis}$ from $P(D_{mis}|D_{obs})$, and then draw Q conveniently from $P(Q|D_{obs}, \bar{D}_{mis})$, given the imputed $\bar{D}_{mis}$. We can approximate $P(Q|D_{obs})$ or any point estimate based on it to any degree of accuracy with a large enough number of simulations. This shows that if the complete-data estimator is consistent and produces accurate confidence interval coverage, then multiple imputation based on $m = \infty$ is consistent, and its confidence intervals are accurate.

Multiple imputation is feasible because the efficiency of estimators based on the procedure increases rapidly with m (see Rubin 1987a and the citations in Meng 1994a; and especially Wang and Robins 1998). Indeed, the relative efficiency of estimators with m as low as 5 or 10 is nearly the same as with $m = \infty$, unless missingness is exceptionally high.

Multiple imputation is made widely applicable by Meng's (1994a) results regarding an imputation model that differs from the analysis model used. He finds that so long as the imputation model includes all the variables (and information) in the analysis model, no bias is introduced; nominal confidence interval coverage will be at least as great as actual coverage and equal when the two models coincide (Fay 1992). Robins and Wang (2000) indicate, however, that multiple imputation confidence intervals are not always conservative when there is misspecification of either both the imputation and analysis model or just the latter. (The next section considers in greater depth what can go wrong with analyses using multiple imputation.)[15]

In summary, even with a very small m and an imputation model that differs from the analysis model, this convenient procedure gives a good approximation to the optimal posterior distribution, $P(Q|D_{obs})$. This result alone guarantees valid inferences in theory from multiple imputation. Indeed, deviating from it to focus on partial calculations sometimes leads to misconceptions on the part of researchers. For example, no assumptions about causal ordering are required in making imputations: The use of variables that may be designated "dependent" in the analysis phase to impute missing values in variables to be designated "explanatory" generates no endogeneity, since the imputations do not change the joint distribution. Simi-

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[14] For difficult cases, our software allows the user to substitute the heavier tailed t for the approximating density. The normal or t with a larger variance matrix, scaled up by some additional factor (1.1–1.5 to work well), can also help.

[15] When the information content is greater in the imputation than analysis model, multiple imputation is more efficient than even the "optimal" application-specific method. This is the so-called super-efficiency property (Rubin 1996). For example, suppose we want to run 20 cross-sectional regressions with the same variables measured in different years, and we discover an additional control variable for each that strongly predicts the dependent variable but on average across the set correlates at zero with the key causal indicator. Excluding this control variable will only bias the causal estimate, on average, if it is a consequence of the causal variable, whereas including it will substantially increase the statistical efficiency of all the regressions. Unfortunately, an application-specific approach would need to exclude such a variable if it were a consequence of the key causal variable to avoid bias and would thus give up the potential efficiency gains. A multiple imputation analysis could include this variable no matter what its causal status, so statistical efficiency would increase beyond an application-specific approach.

larly, randomness in the missing values in the explanatory variable from the multiple imputations do not cause coefficients to be attenuated (as when induced by random measurement error) because the imputations are being drawn from their posterior; again, the joint distribution is unchanged. Since the multiple imputation procedure taken as a whole approximates $P(Q|D_{obs})$, these "intuitions" based on parts of the procedure are invalid (see Schafer 1997, 105ff).[16]

WHAT CAN GO WRONG?

We first discuss common fixable stumbling blocks in the application of EMis and multiple imputation. We then consider the one situation in which listwise deletion would be preferable to multiple imputation, as well as situations in which application-specific approaches would sufficiently outperform multiple imputation to be preferable.

Practical Suggestions

As with any statistical approach, if the model-based estimates of EMis are wrong, then there are circumstances in which the procedure will lead one astray. At the most basic level, the point of inference is to learn something about facts we do not observe by using facts we do observe; if the latter have nothing to do with the former, then we can be misled with any statistical method that assumes otherwise. In the present context, our method assumes that the observed data can be used to predict the missing data. For an extreme counterexample, consider an issue scale with integer responses 1–7, and what you think is a missing value code of -9. If, unbeknownst to you, the -9 is actually an extreme point on the same scale, then imputing values for it based on the observed data and rounding to 1–7 will obviously be biased.[17] Of course, in this case

listwise deletion will be at least as bad, since it generally discards more observed information than EMis has to impute, and it is biased unless strong assumptions about the missing data apply.

An advantage of our approach over application-specific methods (see Appendix A) is that it is often robust to errors in the imputation model, since (as with the otherwise inferior single imputation models; see Appendix A) separating the imputation and analysis stages means that errors in the missingness model can have no effect on observed parts of the data set, because they are the same for all m imputations. If a very large fraction of missingness exists in a data set, then multiple imputation will be less robust, but listwise deletion and other methods will normally be worse.

Beyond these general concerns, a key point for practice is that the imputation model should contain at least as much information as the analysis model. The primary way to go wrong with EMis is to include information in the analysis model and omit it from the imputation model. For example, if a variable is excluded from the imputation model but used in the analysis, estimates of the relationship between this variable and others will be biased toward zero. As a general rule, researchers should include in the imputation model all the variables from the analysis model. For greater efficiency, add any other variables in the data set that would help predict the missing values.[18]

The ability to include extra variables in the imputation model that are not in the analysis model is a special advantage of this approach over listwise deletion. For example, suppose the chosen analysis model is a regression of Y on X, but the missingness in X depends on variables Z that also affect Y (even after controlling for X). In this case, listwise deletion regression is inconsistent. Including Z in the regression would make the estimates consistent in the very narrow sense of correctly estimating the corresponding population parameters, but these would be the wrong population parameters because in effect we were forced to control for Z. For example, suppose the purpose of the analysis model is to estimate the causal effect of partisan identification X on the vote Y. We certainly would not want to control for voting intention five minutes before walking into the voting booth Z, since it is a consequence of party identification and so would incorrectly drive that variable's estimated effect to zero. Yet, Z would be a powerful predictor of the missing value of the vote variable, and the ability to include it in the imputation stage of a multiple imputation model and

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[16] Because the imputation and analysis stages are separate, proponents of multiple imputation argue that imputations for public use data sets could be created by a central organization, such as the data provider, so that analysts could ignore the missingness problem altogether. This strategy would be convenient for analysts and can be especially advantageous if the data provider can use confidential information in making the imputations that otherwise would not be available. The strategy is also convenient for those able to hire consultants to make the imputations for them. Others are not enthusiastic about this idea (even if they have the funds) because it can obscure data problems that overlap the two stages and can provide a comforting but false illusion to analysts that missingness problems were "solved" by the imputer (in ways to which analysts may not even have access). The approach also is not feasible for large data sets, such as the National Election Studies, because existing computational algorithms cannot reliably handle so many variables, even in theory. Our alternative but complementary approach is to make the tools of imputation very easy to use and available directly to researchers to make their own decisions and control their own analyses.

[17] In this sense, the problem of missing data is theoretically more difficult than ecological inference, for example, since both involve filling in missing cells, but in missing data problems deterministic bounds on the unknown quantities cannot be computed. In practice, dealing with the missing data problem may be relatively easier since its assumption (that observed data will not drastically mislead in predicting the missing data) is very plausible in most applications.

[18] If the data are generated using a complex or multistage survey design, then information about the design should be included in the imputation model. For example, to account for stratified sampling, the imputation model should include the strata coded as dummy variables. Our software allows one to include these directly or to condition on them. The former requires no special programming. The latter, which we do by letting μ be a linear function of the dummy variables, is easy to implement because the dummies are fully observed, and many fewer parameters need to be estimated. Other possibilities for dealing with complex sampling designs include hierarchical Bayesian models, the general location model, and other fixed effects designs.

also omit it from the analysis model is a great advantage.

In fact, in many applications scholars apply several analysis models to the same data (such as estimating the effect of party identification while excluding voting intentions, and estimating the effect of voting intentions while including party ID). Despite these different theoretical goals, using different missingness models for the same variables, as listwise deletion effectively requires, is rarely justified. For another example, scholars often choose for an analysis model only one of several very similar issue preference variables from a data set to measure ideology. This is fine for the analysis model, but for the imputation model the entire set of issue preference questions should be included, because an observed value in one can be especially useful for predicting a missing value in another.

A similar information discrepancy occurs if the analysis model specifies a nonlinear relationship, since the imputation model is linear (see equation 6). There is little problem with the set of nonlinear functional forms typically used in the social sciences (logit, probit, exponential, and so on), because a linear approximation to these forms has been shown to perform very well during imputation, even if not for the analysis model. Yet, more severe nonlinearity, such as quadratic terms that are the central question being researched, can cause problems if ignored. A quadratic form is estimated in an analysis model by including an explanatory variable and its square as separate terms. Omitting the squared term from the imputation model causes the same problems as omitting any other important variable. The solution is easy: Include the squared term in the imputation model. The same problem and solution apply to interaction terms (although the imputation procedure will be less efficient if one variable has much more missingness than another).

Researchers also should try to meet the distributional assumptions of the imputation model. For the imputation stage, variables should be transformed to be unbounded and relatively symmetric. For example, budget figures, which are positive and often positively skewed, can be logged. Event counts can be made closer to normal by taking the square root, which stabilizes the variance and makes them approximately symmetric. The logistic transformation can be used to make proportions unbounded and symmetric.

Ordinal variables should be coded to be as close to an interval scaling as information indicates. For example, if categories of a variable measuring the degree of intensity of international conflicts are diplomatic dispute, economic sanctions, military skirmish, and all out war, a coding of 1, 2, 3, and 4 is not approximately interval. Perhaps 1, 2, 20, and 200 might be closer. Of course, including transformations to fit distributional assumptions, and making ordinal codings more reasonable like this, are called for in any linear model, even without missing data.[19]

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[19] Researchers with especially difficult combinations of nominal and continuous variables may want to consider implementing the general location imputation model (Schafer 1997).

Finally, NI missingness is always a serious concern because, by definition, it cannot be verified in the observed data. We discuss this issue in different ways in the sections to follow.

When Listwise Deletion Is Preferable

For listwise deletion to be preferable to EMis, all four of the following (sufficient) conditions must hold. (1) The analysis model is conditional on X (such as a regression model), and the functional form is known to be correctly specified (so that listwise deletion is consistent, and the characteristic robustness of regression is not lost when applied to data with measurement error, endogeneity, nonlinearity, and so on). (2) There is NI missingness in X, so that EMis can give incorrect answers, and no Z variables are available that could be used in an imputation stage to fix the problem. (3) Missingness in X is not a function of Y, and unobserved omitted variables that affect Y do not exist. This ensures that the normally substantial advantages of our approach in this instance do not apply. (4) The number of observations left after listwise deletion should be so large that the efficiency loss from listwise deletion does not counterbalance (e.g., in a mean square error sense) the biases induced by the other conditions. This last condition does not hold in most political science applications except perhaps for exit polls and some nonsurvey data.

In other words, in order to prefer listwise deletion, we must have enough information about problems with our variables so that we do not trust them to impute the missing values in the X's—or we worry more about using available information to impute the X's than the possibility of selection on X as a function of Y in (3), which our approach would correct. Despite this, to use listwise deletion we must still trust the data enough to use them in an analysis model. That is, we somehow know the same variables cannot be used to predict D_{mis} but can be used to estimate quantities based on D_{obs}. Furthermore, we must have no extra variables Z to predict X or Y, and many observations must be left after listwise deletion.

If all of these conditions hold, listwise deletion can outperform EMis, and researchers should consider whether these might hold in their data. However, we feel this situation—using more information is worse—is likely to be rare. It is indeed difficult to think of a real research project that fits these conditions sufficiently so that listwise deletion would be knowingly preferable to EMis. Probably the best case that can be made for listwise deletion is convenience, although our software should help close the gap.

When Application-Specific Approaches Are Worth the Trouble

Although proponents of application-specific methods and of multiple imputation frequently debate the right approach to analyzing data with missing values, if a good application-specific approach is feasible, we believe it should be adopted. Such an approach not only

is better statistically but also by definition allows inclusion of more of the normally substantial qualitative knowledge available to social scientists but not recorded in the numerical data. It encourages researchers to explore features of their data suggested by this qualitative knowledge or revealed by preliminary data analyses, and more information is extracted. Unfortunately, these methods do not exist for all applications, are especially rare for missingness scattered throughout X and Y, can be technically demanding to create, and often are not robust when the chosen model does not fit the data well. The rich variety of methods now available should be studied by social scientists, and the literature should be followed for the many advances likely to come. But if no such method is available, when is a social scientist's effort best devoted to developing a new application-specific method? We identify four situations.

First, as discussed above, imputing values that do not exist makes little sense. Answers to survey questions that are "inconvenient" for the analyst, as when "no opinion" means that the respondent really has no opinion rather than prefers not to share information with the interviewer, should be treated seriously and modeled directly, like any other survey response. In this situation, virtually any general-purpose imputation method would bias the analysis model, and listwise deletion would be no better. An application-specific approach is necessary to model the specific process that generated the survey responses.

Second, when missingness is a function of $Y|X$ (even after controlling for extra variables in the imputation stage), the data are NI. For example, researchers should be suspicious that MAR might not hold in measures of the duration of parliamentary cabinets that are censored due to governments that are still in office at the time of data collection. If these missing values can be predicted from the remaining variables, then the data are still MAR, but this fact is unverifiable, and researchers should tread especially carefully in these circumstances. When NI is a strong possibility, substantial gains can sometimes be had with an application-specific approach. Even if the selection mechanism is not so severe, but is central to the research question, then development of an application-specific approach may be worth considering.

Third, whenever key information in the analysis model cannot be approximated within the imputation model, it may be desirable to develop an alternative. For example, if the analysis model contains severe nonlinearity or very complex interactions that cannot be incorporated into our linear imputation model, then it may be worth developing an application-specific approach. Neural network models provide one such example that cannot be handled easily within the EMis imputation stage (Bishop 1995).

Finally, extreme distributional divergences from multivariate normal can be a good reason to consider an alternative approach. Ordinal and dichotomous variables will often do well under EMis, but variables that are highly skewed (even after transformation) or a variable of primary interest that is mixed continuous

and discrete may make it worth the trouble to develop an alternative.

MONTE CARLO EVIDENCE

In this section, we provide analyses based on simulated data: a timing test that reveals EMis is much faster than IP under different conditions; an illustration of how EMis corrects the problems in EMs and EM in order to match IP's (correct) posterior distribution; and more extensive Monte Carlo evidence demonstrating that IP and EMis give the same answers, and these results are only slightly worse than if no data were missing and normally are far better than listwise deletion. (We have run many other Monte Carlo experiments to verify that the reported standard errors and confidence intervals, as well as estimates for other quantities of interest and different analysis models, are correct, but we omit these here.)

First, we compare the time it takes to run IP and EMis. Since imputation models are generally run once, followed by numerous analysis runs, imputation methods that take time are still useful. Runs of many hours, however, make productive analysis much less likely, especially if several data sets must be analyzed.

We made numerous IP and EMis runs, but it is not obvious how IP should be timed because there are no clear rules for judging convergence. We made educated guesses, ran experiments in which we knew the distribution to which IP was converging, studied profile plots of the likelihood function, and otherwise used Schafer's (1997) recommended defaults. On the basis of this experience, we chose max(1000, 100p) iterations to generate the timing numbers below, where p is the number of variables. For the EMis algorithm we chose a very conservative 1/50 ratio of draws to imputations. With each algorithm we created ten imputed data sets. We used a computer with average speed for 1999 (450Mhz with 128M of RAM). We then created a data set with 1,000 observations, of which 50 of these and one variable were fully observed. Every remaining cell was missing with 5% probability, which is not unusual for most social science survey data.

For 5 variables, IP takes 4.8 minutes, whereas EMis finishes in 3 seconds. For 10 variables, IP takes 28 minutes, and EMis runs for 14 seconds. With 20 variables, IP takes 6.2 hours, and EMis takes 2 minutes. With 40 variables, IP takes 3.5 days, whereas EMis runs for 36 minutes. Overall, EMis ranges from 96 to 185 times faster. Counting the analyst's time that is necessary to evaluate convergence plots would make these comparisons more dramatic.[20] Running one IP chain would be 2–3 times as fast as the recommended approach of separate chains, but that would require evaluating an additional $p(p + 3)/2$ autocorrelation

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[20] Since convergence is determined by the worst converging parameter, one typically needs to monitor $p(p + 3)/2$ convergence plots. For applications in which the posterior is nearly normal, evaluating the worst linear function of the parameters can sometimes reduce the number of plots monitored. We also did not include the time it would take to create an overdispersed set of starting values for the IP chains.

FIGURE 1.   Comparison of Posterior Distributions
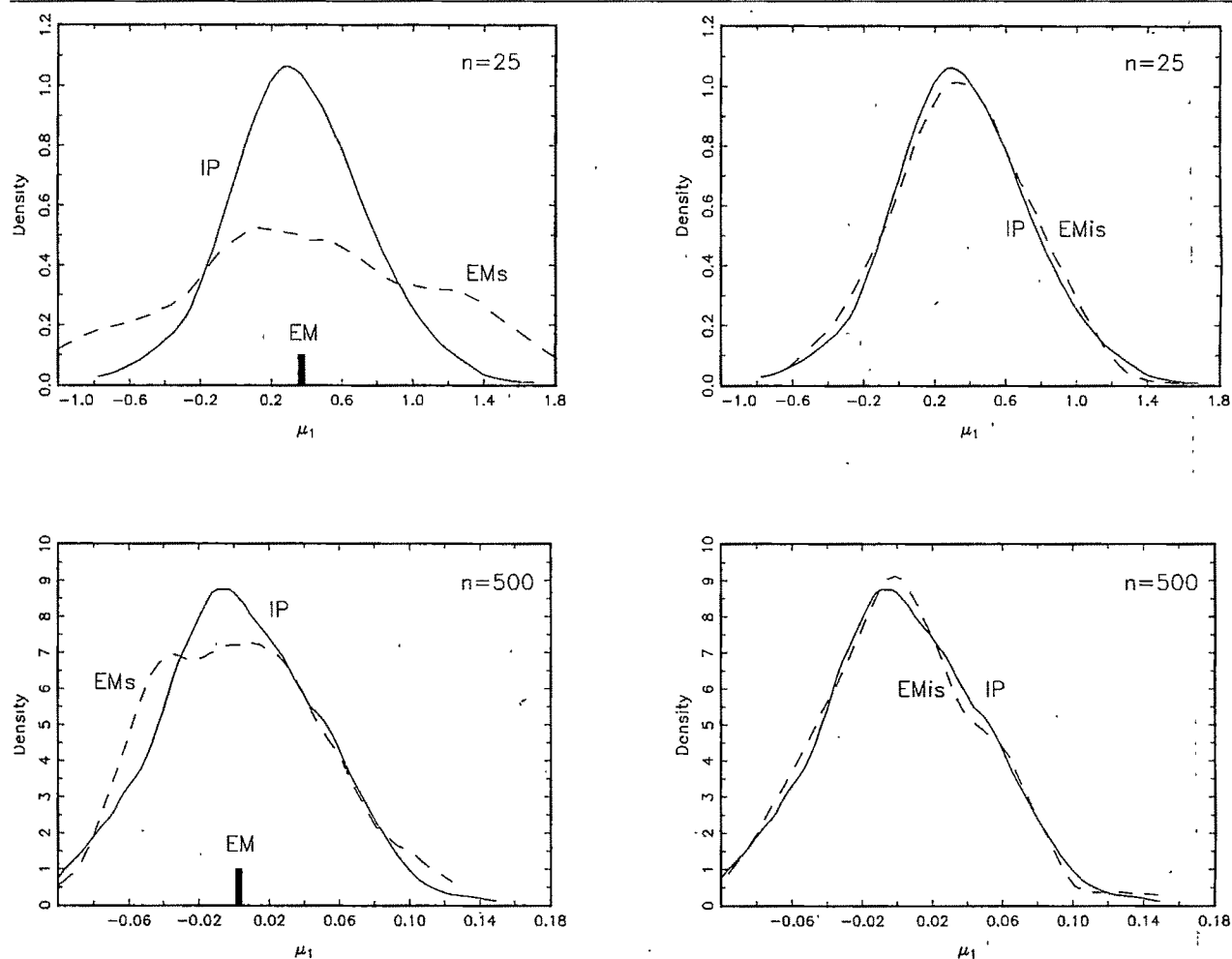
Note: These graphs show, for one mean parameter, how the correct posterior (marked IP) is approximated poorly by EM, which only matches the mode, and EMs when *n* is small (top left). IP is approximated well by EMs for a larger *n* (bottom left) and by EMIs for both sample sizes (right top and bottom).

function plots to avoid creating dependent imputations.[21]

Second, we plot smooth histograms (density estimates of 200 simulations) of one mean parameter from a Monte Carlo run to illustrate how EM, EMs, and EMIs approximate the posterior computed by IP and known to be correct (see Figure 1). The first row of

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[21] We programmed both IP and EMIs in the same language (GAUSS), which keeps them comparable to a degree. Our algorithm is more suited to the strengths of the GAUSS language. Additional vectorization will speed up both algorithms, but not necessarily in the same ratio. For example, Schafer's (1997) FORTRAN implementation of IP (which should be approximately as fast as vectorized code in a modern vectorized language) is about 40 times as fast as our GAUSS implementation of IP following Schafer's pseudocode. Schafer's FORTRAN implementation of EM is about 25 times as fast as the EM portion of EMIs. Similarly, the speed of our variance calculation could be substantially improved with complete vectorization. We use a FORTRAN implementation, as part of our GAUSS code, for calculating the likelihood in the importance sampling portion of the EMIs algorithm, making the calculation of the likelihood fully vectorized. We do this because it is a calculation not well suited to GAUSS. Without this, our algorithm in GAUSS runs for 5 seconds, 52 seconds, 25 minutes, and 25 hours, respectively, or from 4 to 58 times faster than IP.

graphs is for $n = 25$, and the second row is for $n = 500$. The first column compares EMs and EM to IP, and the second compares EMIs to IP. In all four graphs, the correct posterior, computed by IP, is a solid line. Clearly, the maximum likelihood point estimate found by EM (and marked by a small vertical bar on the left graphs) is not an adequate approximation to the entire posterior. By ignoring estimation variability, EM underestimates standard errors and confidence intervals.

The figure also enables us to evaluate EMs and EMIs. For example, the dashed line in the top left graph shows how, with a small sample, EMs produces a poor approximation to the true IP posterior. The bottom left graph shows how EMs improves with a larger sample, courtesy of the central limit theorem. In this example, more than 500 observations are apparently required to have a close match between the two, but EMs does not perform badly with $n = 500$. In contrast, EMIs closely approximates the true IP posterior when the sample is as small as 25 (in the top right) and is not noticeably different when $n = 500$. (The

FIGURE 2. Root Mean Square Error Comparisons
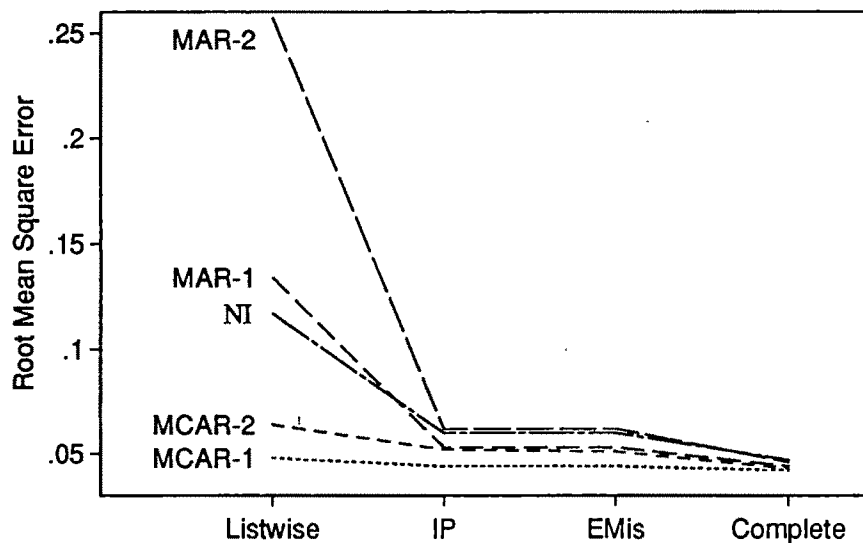
Note: This figure plots the average root mean square error for four missing data procedures—listwise deletion, multiple imputation with IP and EMis, and the true complete data—and the five data-generation processes described in the text. Each point in the graph represents the root mean square error averaged over two regression coefficients in each of 100 simulated data sets. Note that IP and EMis have the same root mean square error, which is lower than listwise deletion and higher than the complete data.

small differences remaining between the lines in the two right graphs are attributable to approximation error in drawing the graphs based on only 200 simulations.)

Finally, we generate data sets with different missingness characteristics and compare the mean square errors of the estimators. The Monte Carlo experiments we analyze here were representative of the many others we tried and are consistent with others in the literature. We generate 100 data sets randomly from each of five data generation processes, each with five variables, Y, $X_1, \ldots, X_4$.

MCAR-1: Y, X_1, X_2, and X_4 are MCAR; X_3 is completely observed. About 83% of the observations in the regression are fully observed.

MCAR-2: The same as MCAR-1, with about 50% of rows fully observed.

MAR-1: Y and X_4 are MCAR: X_1 and X_2 are MAR, with missingness a function of X_3, which is completely observed. About 78% of rows are fully observed.

MAR-2: The same as MAR-1, with about 50% of rows fully observed.

NI: Missing values in Y and X_2 depend on their observed and unobserved values; X_1 depends on the observed and unobserved values of X_3; and X_3 and X_4 are generated as MCAR. About 50% of rows are fully observed.[22]

The quantities of interest are β_1 and β_2 in the regression $E(Y) = \beta_0 + \beta_1 X_1 + \beta_2 X_2$.[23] The Σ matrix is set so that β_1 and β_2 are each about 0.1. For each of the 100 data sets and five data-generation processes, we estimate these regression coefficients with imputation models based on listwise deletion, IP, and EMis as well as with the true complete data set. For each application of IP and EMis, we multiply imputed ten data sets and averaged the results as described above. We then computed the average root mean square error for the two coefficients in each run and averaged these over the 100 simulations for each data type and statistical procedure.

The vertical axis in Figure 2 is this averaged root mean square error. Each line connects the four different estimations for a single data-generation process. The graph helps demonstrate three points. First, the root mean square error of EMis is virtually identical to that of IP for each data-generation process. This confirms again the equivalence of the two approaches.

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[22] We drew $n = 500$ observations from a multivariate normal with means 0, variances 1, and correlation matrix {1 $-.12$ $-.1$ $.5$ $.1$, $-.12$ 1 $.1$ $-.6$ $.1$, $-.1$ $.1$ 1 $-.5$ $.1$, $.5$ $-.6$ $-.5$ 1 $.1$, $.1$ $.1$ $.1$ $.1$ 1}, where commas separate rows. For each missingness process, we created M as follows. Let row i and column j of M be denoted M_{ij}, and let u be a uniform random number. Recall that columns of M correspond to columns of $D = \{Y, X_1, \ldots, X_4\}$. For MCAR-1, if $u < 0.6$, then $M_{ij} = 1$, 0 otherwise. For MCAR-2, if $u < 0.19$, then $M_{ij} = 1$, 0

otherwise. For MAR-1, M_{i1} and M_{i5} were created as in MCAR-1; $M_{i4} = 0 \forall i$; and if $X_{i3} < -1$ and $u < 0.9$, then $M_{i2} = 1$ and (with a separate value of u) $M_{i3} = 1$, 0 otherwise. For MAR-2, M_{i1} and M_{i5} equal 0 if $u < 0.12$, 1 otherwise; $M_{i4} = 0 \forall i$; and if $X_{i3} < -0.4$ and $u < 0.9$, then $M_{i2} = 1$ and (with a separate value of u) $M_{i3} = 1$. For NI, $M_{i1} = 1$ if $Y_i < -0.95$; $M_{i2} = 1$ if $X_{i3} < -0.52$; $M_{i3} = 1$ if $X_{i2} > 0.48$; and M_{i4} and M_{i5} were created as in MCAR-1. In other runs, not reported, we changed every parameter, the generating density, and the analysis model, and our conclusions were very similar.

[23] We chose regression as our analysis model for these experiments because it is probably still the most commonly used statistical method in the social sciences. Obviously, any other analysis model could have been chosen, but much research has demonstrated that multiple imputation works in diverse situations. For our testing, we did extensive runs with logit, linear probability, and several univariate statistics, as well as more limited testing with other more complicated models.

FIGURE 3.   Monte Carlo Comparison of *t* Statistics
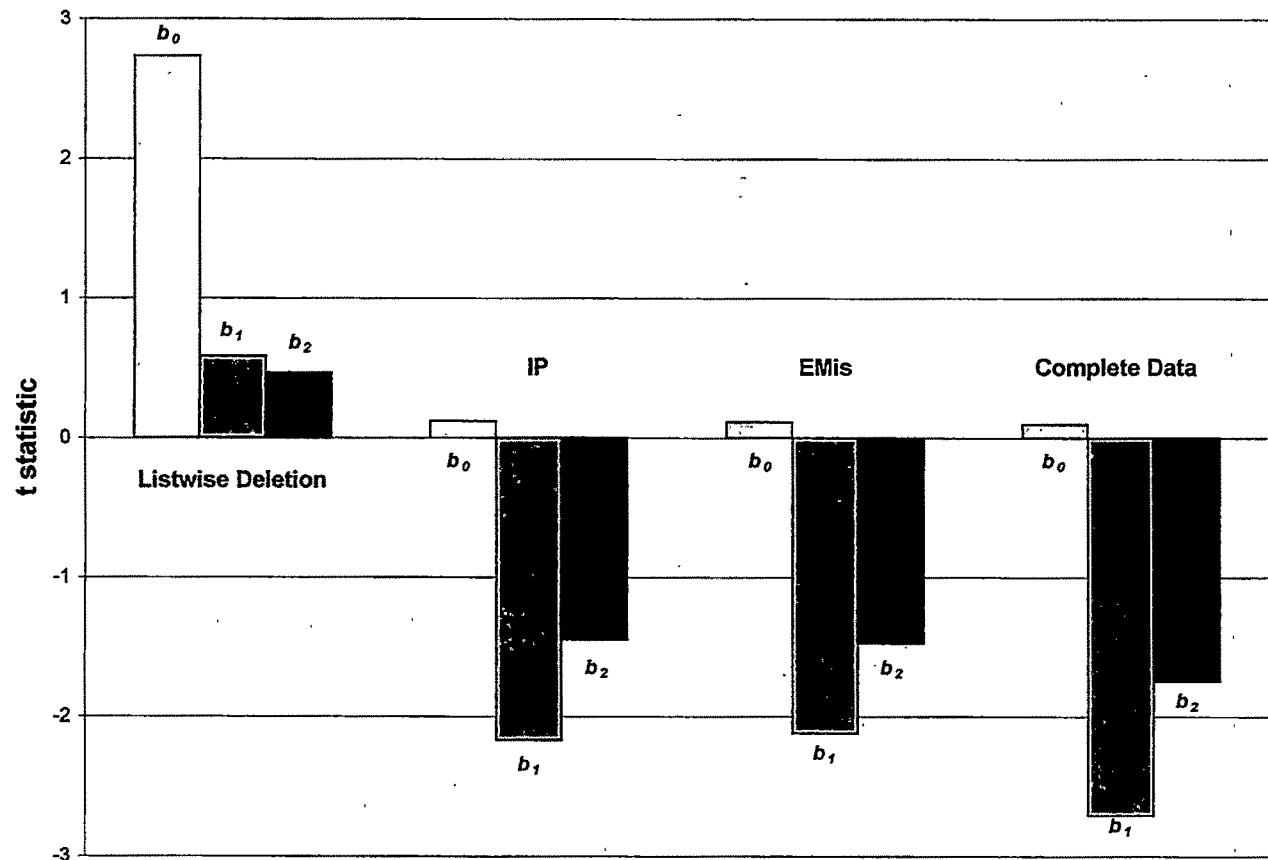
Note: T statistics are given for the constant (b_0) and the two regression coefficients (b_1, b_2) for the MAR-1 run in Figure 2. Listwise deletion gives the wrong results, whereas EMis and IP recover the relationships accurately.

Second, the error for EMis and IP is not much higher than the complete (usually unobserved) data set, despite high levels of missingness. Finally, listwise deletion ranges from slightly inferior to the two multiple imputation methods (in the MCAR cases when the assumptions of listwise deletion hold) to a disaster (in the MAR and NI cases). Since the true value of the coefficients being estimated is about 0.1, root mean square errors this large can bias results by flipping signs or greatly changing magnitude. An open question is which articles in political science have large mean square errors like that for MAR-2 due to listwise deletion.

A further illustration of the results of our Monte Carlo study is provided in Figure 3, which gives a different view of the MAR-1 run in Figure 2. For MAR-1, the case of low missingness, the root mean square error for listwise deletion was higher than for the other methods but not as high as for MAR-2. Figure 3 graphs the *t* statistic for the constant term and each of the two regression coefficients, averaged over the 100 runs for each of the four imputation procedures. For the two regression coefficients, the sign is negative (and "significant" for b_1) when estimated by the true complete data, IP, and EMis, but the opposite is the case for listwise deletion. In the listwise deletion

run, both coefficients have point estimates that are positive but statistically indistinguishable from zero. Most of the action in the listwise case is generated in the substantively uninteresting constant term.

Figure 3 is a clear example of the dangers political scientists face in continuing to use listwise deletion. Only 22% of the observations were lost in this case, yet the key substantive conclusions are reversed by choosing an inferior method. It is easy to generate hypothetical data with larger effects, but this instance is probably closer to the risks we face.

EXAMPLES

We present two examples that demonstrate how switching from listwise deletion can markedly change substantive conclusions.

Voting Behavior in Russian Elections

The first example is vote choice in Russia's 1995 parliamentary election. Analyses of elections in Russia and emerging democracies generally present conflicting descriptions of individual voting behavior. In one view, electoral choice in these elections is thought to be chaotic at worst and personalistic at best. The alterna-

TABLE 2. First-Difference Effects on Voting in Russia		
	Listwise Deletion	Multiple Imputation
Satisfaction with democracy	−.06 (.06)	−.10 (.04)
Opposition to the market economy	.08 (.08)	.12 (.05)
Trust in the Russian government	−.06 (.08)	−.12 (.04)

Source: Authors' reanalysis of data from Colton 2000.
Note: Entries are changes in the probability of voting for the Communist Party in the 1995 parliamentary election as a function of changes in the explanatory variable (listed on the left), with standard errors in parentheses.

tive perspective is that voting decisions are based in predictable ways on measurable social, demographic, attitudinal, and economic variables (not unlike voters in more established democracies). Our analysis illustrates how inferences can be substantially improved by implementing the EMis algorithm.

We present only a simplified voting model, but detailed accounts of behavior in recent Russian elections are available (Brader and Tucker 2001; Colton 2000; Fish 1995; Miller, Reisinger, and Hesli 1998; White, Rose, and McAllister 1997; Whitefield and Evans 1996).[24] Using data from the Russian Election Study (Colton n.d.), we estimate a logit model with the dependent variable defined as 1 if the voter casts a ballot for the Communist Party of the Russian Federation (KPRF), 0 otherwise. With more than 22% of the popular vote, the KPRF was the plurality winner in the 1995 parliamentary elections, which makes understanding this vote essential to a correct interpretation of the election. The explanatory variables for our simple model vary according to the stage of the voter's decision-making process being tested, in order to avoid controlling for the consequences of key causal variables. Listwise deletion loses 36%, 56%, and 58% of the observations, respectively, in the three stages from which we use data.

Table 2 presents estimates of three first differences derived from our logit regressions for listwise deletion and EMis. First, we estimate the effect of a voter's satisfaction with democracy on the probability of supporting the KPRF. This is one measure of voters' assessments of current economic and political conditions in Russia. Voters more satisfied with democracy may be less likely to support the KPRF than those who are dissatisfied. The quantity of interest is the difference between the predicted probability for a voter who is completely dissatisfied with how democracy is developing in Russia and the predicted probability for a voter who is completely satisfied, holding all other values of the explanatory variables constant at their

[24] We were alerted to the potential importance of missing data problems in this literature by Timothy Colton as he experimented with alternative strategies for his study, *Transitional Citizens: Voters and What Influences Them in the New Russia* (2000).

means. The listwise deletion estimate is −0.06 with a relatively large standard error of 0.06, which for all practical purposes is no finding. In contrast, the EMis estimate is −0.10 with a standard error of 0.04. The unbiased and more efficient EMis estimate is nearly twice as large and is estimated much more precisely. As such, we can be relatively confident that voters highly satisfied with Russian democracy were about 10% less likely to support the KPRF, a finding not ascertainable with existing methods.

Issue opinions are another likely determinant of vote choice. In particular, are voters who oppose the transition to a market economy more likely than others to support the Communist Party? The answer seems obvious, but listwise deletion reveals little support for this hypothesis; again, the first-difference estimate is in the hypothesized direction but is only as large as its standard error (and thus not "significant" by any relevant standard). In contrast, the EMis estimate suggests that voters opposed to the transition were about 12% more likely to vote for the KPRF, with a very small standard error.

The final comparison that we report is the voting effect of trust in the Russian government. Positive evaluations should have had a negative influence on KPRF support in the 1995 Duma election. Again, listwise deletion detects no effect, but multiple imputation finds a precisely estimated twelve percentage point difference.

Table 2 presents only these three of the forty-six effects we estimated. Overall, we found substantively important changes in fully one-third of the estimates. Ten changed in importance as judged by traditional standards (from "statistically significant" to not, or the reverse, plus some substantively meaningful difference), and roughly five others increased or decreased sufficiently to alter the substantive interpretation of their effects.

Public Opinion about Racial Policies

The second example replicates the analysis by Alvarez and Brehm (1997) of the factors that explain Americans' racial policy preferences and the variance in those preferences. They use a heteroskedastic probit to model citizens preferences about racial policies in fair-housing laws, government set asides, taxes to benefit minority educational opportunities, and affirmative action in university admissions. Their explanatory variables are scales constructed to measure individual's core values or beliefs, such as individualism, authoritarianism, egalitarianism, and ideology. They also include scales measuring antiblack stereotypes, generic out-group dislike (proxied by anti-Semitism), and modern racism. The latter term is a subject of debate in the literature (Kinder 1986; Kinder and Sears 1981; McConahay 1986); proponents argue that there is "a form of racism that has replaced overt expressions of racial superiority" (Alvarez and Brehm 1997, 347), and it defines attitudes to racial policies and questions. This "symbolic or modern racism denotes a conjunction of antiblack affect with traditional American values, tak-

ing form in the sense that blacks are receiving more attention from government or other advantages than they deserve" (p. 350).[25]

Alvarez and Brehm employ a statistical model that explains with these variables not only the racial policy preferences of individuals but also the individual variability in responses. When variability is explained by the respondent's lack of political information, then it is considered to be caused by uncertainty, whereas if variability is explained by a conflict between "competing core values" or "incommensurable choices," then it is caused by ambivalence. They find that these preferences are not motivated by core values such as individualism, and so on, but are solely determined by a person's level of modern racism. The authors are more interested substantively in understanding what causes variability in response. They find that the "individual variability in attitudes toward racial policy stems from uncertainty" (Alvarez and Brehm 1997, 369) derived from a "lack of political information" (p. 370), not from a conflict of core values, such as individualism with egalitarianism. The same model shows variability in abortion policy preferences to be due to a conflict of core values (Alvarez and Brehm 1995), but variability in response on racial policy is due to a lack of political information. Therefore, better informed individuals might change their responses, which offers encouragement to advocates of education and debate about racial policy.

To tap core values, Alvarez and Brehm constructed "core belief scales" from responses to related feeling thermometers and agree/disagree measures. A missing value in any of the individual scale items caused the entire scale value for that observation to be treated as missing. This problem was severe, since listwise deletion would have eliminated more than half the observations.

For one of the scales—ideology—Alvarez and Brehm dealt with the missingness problem by replacing the scale (based on a question using the terms "liberal-conservative") with an alternate question if respondents refused to answer or did not know their ideology in the terms of the original question. The alternate question pressed the respondent to choose liberal or conservative, which Alvarez and Brehm coded as a neutral with a weak leaning to the side finally chosen. This is a clear case of unobserved data and the use of a reasonable but ad hoc imputation method.[26] If the question concerned party identification, a valid response might be "none," and this might not be a missing value, merely an awkward response for the analyst. Yet, although "ideological self-placement" may be legitimately missing, the self-placement question is considered to be at fault. The individual pre-

sumably has some ideological stance, no matter how uncertain, but is not willing or able to communicate it in the terminology of the survey question. Nevertheless, to press the respondent to choose and then guess how to code these values on the same scale as the original question risks attenuating the estimated relationships.[27]

Fortunately, use of the forcing question is unnecessary, since items on homelessness, poverty, taxes, and abortion can easily be used to predict the technical placement without shifting the responsibility to the respondent who does not understand, or has not thought about, our academic terminology. Indeed, bias seems to be a problem here, since in the Alvarez and Brehm analysis, ideology rarely has an effect. When we impute missing ideology scores from the other items, however, instead of using the alternate question, ideology becomes significant just over half the time, and the coefficients all increase in both the choice and the variance models (for all the dependent variables they used).

We apply EMis for the missing components of the scales to counter the problem of nonresponse with greater efficiency and less bias. We present first-difference results in the style of Alvarez and Brehm in Table 3. The first differences represent the change in probability of supporting an increase in taxation to provide educational opportunities to minorities when a particular variable is moved from its mean to its mean plus two standard deviations, as in Alvarez and Brehm.[28]

The main substantive finding, that variance in policy choice between respondents is driven by a lack of information rather than a conflict between the core values, still holds. In contrast, the secondary finding, which explains individual preferences and which contributes to the more mainstream and developed policy argument, is now reversed. Most important, individual racial policy choice now appears to be a broad function of many competing values, not just modern racism. An individual's level of authoritarianism, anti-Semitism, and egalitarianism as well as ideological position all strongly affect the probability that a person will support increased taxes for minority educational opportunities.

Finally, and quite important, the chi-square test reported at the bottom of Table 3 is insignificant under Alvarez and Brehm's original specification but is now

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[25] Alvarez and Brehm measured modern racism with three questions relating to the amount of attention minorities are paid by government, anger that minorities are given special advantages in jobs and education, and anger about minority spokespersons complaining about discrimination.
[26] This procedure was made known to us, and other portions of the replication were made possible, when the authors provided us code from their original analysis, for which we are grateful.

[27] Consistent with the literature (e.g., Hinich and Munger 1994), we assume that ideology measures an individual's underlying policy preferences. If one assumes that people have at least some policy views, then they have an ideology, even if they are unwilling or unable to place themselves on an ideological scale. Alternative treatments, especially in the European context, view ideology as an exogenous orientation toward politics. Missingness in ideology in that framework might be treated very much like partisan identification.
[28] These results mirror those presented by Alvarez and Brehm (1997, 367) in their Table 3, column 3, rows 1–7. Similar effects are found in all the other rows and columns of their tables 3 and 4. Our replication using their methods on the original data does not match their results exactly, including the N, but the substantive findings of our replication of their methods and their results are almost entirely the same throughout tables 1–4 of the original work. We also include standard errors in the reporting of first differences in our presentation (King, Tomz, and Wittenberg 2000).

TABLE 3. Estimated First Differences of Core Beliefs

	Listwise Deletion	Multiple Imputation
Modern racism	−.495*	−.248*
	(.047)	(.046)
Individualism	.041	.005
	(.045)	(.047)
Antiblack	−.026	−.011
	(.047)	(.042)
Authoritarianism	.050	.068*
	(.045)	(.035)
Anti-Semitism	−.097	−.115*
	(.047)	(.045)
Egalitarianism	.201*	.236*
	(.049)	(.053)
Ideology	−.076	−.133*
	(.054)	(.063)
N	1,575	2,009
χ^2	8.46	11.21*
$p(\chi^2)$	.08	.02

Note: The dependent variable is support for an increase in taxation to support educational opportunities for minorities. The first column reports our calculation of first difference effects and standard errors for the substantive variables in the mean function, using the same data set (the 1991 Race and Politics Survey, collected by the Survey Research Center, University of California, Berkeley) used by Alvarez and Brehm (1997). (For details on the survey and availability information, see their note 1.) Although we followed the coding rules and other procedures given in their article as closely as possible, our analysis did not yield the same values reported by Alvarez and Brehm for the first difference effects. Even so, our listwise deletion results confirm the substantive conclusions they arrived at using this method of dealing with missing data. The second column is our reanalysis using EMis. Asterisks indicate $p < 0.05$, as in the original article. The χ^2 test indicates whether the heteroskedastic probit model is distinguishable from the simpler probit model.

significant.[29] This test measures whether their sophisticated analysis model is superior to a simple probit model, and thus whether the terms in the variance model warrant our attention. Under their treatment of missing values, the variance component of the model does not explain the between-respondent variances, which implies that their methodological complications were superfluous. Our approach, however, rejects the simpler probit in favor of the more sophisticated model and explanation.[30]

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[29] See Meng (1994b) and Meng and Rubin (1992) for procedures and theory for p values in multiply imputed data sets. We ran the entire multiple imputation analysis of $m = 10$ data sets 100 times, and this value never exceeded 0.038.

[30] Sometimes, of course, our approach will strengthen rather than reverse existing results. For example, we also reanalyzed Domínguez and McCann's (1996) study of Mexican elections and found that their main argument (voters focus primarily on the potential of the ruling party and viability of the opposition rather than specific issues) came through stronger under multiple imputation. We also found that several of the results on issue positions that Domínguez and McCann were forced to justify ignoring or attempting to explain away turned out to be artifacts of listwise deletion.

We also replicated Dalton, Beck, and Huckfeldt's (1998) analysis of partisan cues from newspaper editorials, which examined a merged data set of editorial content analyses and survey responses.

CONCLUSION

For political scientists, almost any disciplined statistical model of multiple imputation would serve better than current practices. The threats to the validity of inferences from listwise deletion are of roughly the same magnitude as those from the much better known problems of omitted variable bias. We have emphasized the use of EMis for missing data problems in a survey context, but it is no less appropriate and needed in fields that are not survey based, such as international relations. Our method is much faster and far easier to use than existing multiple imputation methods, and it allows the usage of about 50% more information than is currently possible. Political scientists also can jettison the nearly universal but biased practice of making up the answers for some missing values. Although any statistical method can be fooled, including this one, and although we generally prefer application-specific methods when available, EMis normally will outperform current practices. Multiple imputation was designed to make statistical analysis easier for applied researchers, but the methods are so difficult to use that in the twenty years since the idea was put forward it has been applied by only a few of the most sophisticated statistical researchers. We hope EMis will bring this powerful idea to those who can put it to best use.

APPENDIX A. CURRENT APPROACHES

Available methods for analyzing data sets with item nonresponse can be divided into two approaches: application specific (statistically optimal but hard to use) and general purpose (easy to use and more widely applicable but statistically inadequate).

Application-Specific Approaches

Application-specific approaches usually assume MAR or NI. The most common examples are models for selection bias, such as truncation or censoring (Achen 1986; Amemiya 1985, chap. 10; Brehm 1993; Heckman 1976; King 1989, chap. 7; Winship and Mare 1992). Such models have the advantage of including all information in the estimation, but almost all allow missingness only in or related to Y rather than scattered throughout D.

When the assumptions hold, application-specific approaches are consistent and maximally efficient. In some cases, however, inferences from these models tend to be sensitive to small changes in specification (Stolzenberg and Relles 1990). Moreover, different models must be used for each type of application. As a result, with new types of data, application-specific approaches are most likely to be used by

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Most missing data resulted from the authors' inability to content analyze the numerous newspapers that respondents reported reading. Because the survey variables contained little information useful for predicting content analyses that were not completed, an MCAR missingness mechanism could not be rejected, and the point estimates did not substantially change under EMis, although confidence intervals and standard errors were reduced. Since Dalton, Beck, and Huckfeldt's analysis was at the county level, it would be possible to gather additional variables from census data and add them to the imputation stage, which likely would substantially improve the analysis.

those willing to devote more time to methodological matters.[31]

More formally, these approaches model D and M jointly and then factor the joint density into the marginal and conditional. One way to do this produces selection models, $P(D, M|\theta, \gamma) = P(D|\theta)P(M|D, \gamma)$, where $P(D|\theta)$ is the likelihood function when no data are missing (a function of θ, the parameter of interest), and $P(M|D, \gamma)$ is the process by which some data become missing (a function of γ, which is not normally of interest). Once both distributions are specified, as they must be for these models, averaging over the missing data yields the following likelihood:

$$P(D_{obs}, M|\theta, \gamma) = \int P(D|\theta)P(M|D, \gamma)dD_{mis}, \qquad (11)$$

where the integral is over elements of D_{mis} and is summation when discrete. If MAR is appropriate (i.e., D and M are stochastically independent), then equation 11 simplifies:

$$P(D_{obs}, M|\theta, \gamma) = P(D_{obs}|\theta)P(M|D_{obs}, \gamma). \qquad (12)$$

If, in addition, θ and γ are parametrically independent, the model is ignorable, in which case the likelihood factors and only $P(D_{obs}|\theta)$ need be computed.

Unlike multiple imputation models, application-specific approaches require specifying $P(M|D, \gamma)$, about which scholars often have no special interest or knowledge. Evaluating the integral in equation 11 can be difficult or impossible. Even with MAR and ignorability assumptions, maximizing $P(D_{obs}|\theta)$ can be computationally demanding, given its nonrectangular structure. When these problems are overcome, application-specific models are theoretically optimal, even though they can make data analyses difficult in practice. (Software that makes this easier includes Amos and Mx, but only for linear models and only assuming MAR.)

General Purpose Methods

General purpose approaches are easier to use. The basic idea is to impute ("fill in") or delete the missing values and then analyze the resulting data set with any standard treatment that assumes the absence of missing data. General purpose methods other than listwise deletion include mean substitution (imputing the univariate mean of the observed observations), best guess imputation (common in political science), imputing a zero and then adding a dummy variable to control for the imputed value, pairwise deletion (which really only applies to covariance-based models), and hot deck imputation (imputing from a complete observation that is similar in as many observed ways as possible to the observation that has a missing value). Under MAR (or NI), all these techniques are biased or inefficient, except in special cases. Most of those which impute give standard errors that are too small because they essentially "lie" to the computer program, telling it that we know the imputed values with as much certainty as we do the observed values. It is worth noting that listwise deletion, despite the problems discussed above, does generate valid standard errors, which makes it preferable in an important way to approaches such as mean substitution and best guess imputation.

When only one variable has missing data, one possibility is to run a regression (with listwise deletion) to estimate the relationship among the variables and then use the predicted

values to impute the missing values. A more sophisticated version of this procedure can be used iteratively to fill in data sets with many variables missing. This procedure is not biased for certain quantities of interest, even assuming MAR, since it conditions on the observed data. Since the missing data are imputed on the regression line as if there were no error, however, the method produces standard errors that are too small and generates biased estimates of quantities of interest that require more than the conditional mean (such as $\Pr(Y > 7)$). To assume that a statistical relationship is imperfect when observed but perfect when unobserved is optimistic, to say the least.

Finally, one general purpose approach developed recently is an imputation method that combines elements of the multiple imputation procedures presented in this article and the application-specific methods discussed above. Analysts generate one or more imputed data sets in the first step and then calculate estimates of the relevant quantity of interest and its variance using alternative formulas to equations 2 and 3 (Robins and Wang 2000; Wang and Robins 1998). Like application-specific methods, this approach is theoretically preferred to multiple imputation but requires different adjustments for each analysis model, and it is not currently available in commercial software packages. Since this approach can be more efficient than multiple imputation, and the computed variances are correct under several forms of misspecification, there is much to recommend it.

APPENDIX B. PROOF OF MEAN SQUARE ERROR COMPARISONS

Model

Let $E(Y) = X\beta = X_1\beta_1 + X_2\beta_2$ and $V(Y) = \sigma^2 I$, where $X = (X'_1, X'_2)'$, $\beta = (\beta'_1, \beta'_2)'$, and λ is the fraction of rows of X_2 missing completely at random (other rows of X_2 and all of Y and X_1 are observed). The ultimate goal is to find the best estimator for β_1; the specific goal is to derive equation 1. We evaluate the three estimators of β_1 by comparing their mean square errors (MSE). MSE is a measure of how close the distribution of the estimator $\hat{\theta}$ is concentrated around θ. More formally, MSE $(\hat{\theta}, \theta) = E[(\hat{\theta} - \theta)^2] = V(\hat{\theta}) + E(\hat{\theta} - \theta)E(\hat{\theta} - \theta)' = \text{variance} + \text{bias}^2$.

Estimators

Let $b^I = AY = (b_1^{I'}, b_2^{I'})'$, where $A = (X'X)^{-1}X'$. Then b_1^I is the Infeasible estimator of β_1. Let $b_1^O = A_1Y$ be the Omitted variable bias estimator of β_1, where $A_1(X'_1X_1)^{-1}X'_1$. Finally, let $b^L = A^L Y^L = (b_1^{L'}, b_2^{L'})'$, where $A^L = (X^{L'}X^L)^{-1}X^{L'}$, and where the superscript L denotes listwise deletion applied to X and Y. So b_1^L is the Listwise deletion estimator of β_1.

Bias

The infeasible estimator is unbiased—$E(b^I) = E(AY) = AX\beta = \beta$—and thus bias$(b_1^I) = 0$. The omitted variable estimator is biased, as per the usual calculation, $E(b_1^O) = E(b_1^I + Fb_2^I) = \beta_1 + F\beta_2$, where each column of F is a factor of coefficients from a regression of a column of X_2 on X_1 so bias $(b_1^O) = F\beta_2$. If MCAR holds, then listwise deletion is also unbiased, $E(b^L) = E(A^L Y^L) = A^L X^L \beta = \beta$, and thus bias$(b_1^L) = 0$.

[31] For application-specific methods in political science, see Achen 1986; Berinsky 1997; Brehm 1993; Herron 1998; Katz and King 1999; King et al. 1990; Skalaban 1992; and Timpone 1998.

Variance

The variance of the infeasible estimator is $V(b^I) = V(AY) = A\sigma^2 IA' = \sigma^2(X'X)^{-1}$. Since $V(b_1^I) = V(b_1^I - Fb_2^I) = V(b_1^O) - FV(b_2^I)F'$, the omitted variable bias variance is $V(b_1^O) = V(b_1^I) - FV(b_2^I)F'$. Because $V(b^L) = V(A^L Y^L) = A^L \sigma^2 IA^{L'} = \sigma^2(X^{L'} X^L)^{-1}$, the variance of the listwise deletion estimator is $V(b_1^L) = \sigma^2(Q^L)^{11}$, where $(Q^L)^{11}$ is the upper left portion of the $(X^L X^L)^{-1}$ matrix corresponding to X_1^L.

MSE

Putting together the (squared) bias and variance results gives MSE computations: $MSE(b_1^O) = V(b_1^I) + F[\beta_2\beta_2' - V(b_2^I)]F'$, and $MSE(b_1^L) = \sigma^2(Q^L)^{11}$.

Comparison

In order to evaluate when listwise deletion outperforms the omitted variable bias estimator, we compute the *difference d* in MSE:

$$d = MSE(b_1^L) - MSE(b_1^O) = [V(b_1^L) - V(b_1^I)]$$
$$+ F[V(b_2^I) - \beta_2\beta_2']F'. \quad (13)$$

Listwise deletion is better than omitted variable bias when $d < 0$, worse when $d > 0$, and no different when $d = 0$. The second term in equation 13 is the usual bias-variance tradeoff, so our primary concern is with the first term. $V(b^I)[V(b^L)]^{-1} = \sigma^2(X^{L'} X^L + X'_{mis}X_{mis})^{-1}1/\sigma^2(X^{L'} X^L) = I - (X^{L'} X^L + X'_{mis}X_{mis})^{-1}(X'_{mis}X_{mis})$, where X_{mis} includes the rows of X deleted by listwise deletion (so that $X = \{X^L, X_{mis}\}$). Since exchangability among rows of X is implied by the MCAR assumption (or, equivalently, takes the expected value over sampling permutations), we write $(X^{L'} X^L + X'_{mis}X_{mis})^{-1}(X'_{mis}X_{mis}) = \lambda$, which implies $V(b_1^L) = V(b^I)/(1 - \lambda)$. This, by substitution into equation 13, yields and thus completes the proof of equation 1.

APPENDIX C. SOFTWARE

To implement our approach, we have written easy-to-use software, *Amelia: A Program for Missing Data* (Honaker et al. 1999). It has many features that extend the methods discussed here, such as special modules for high levels of missingness, small *n*'s, high correlations, discrete variables, data sets with some fully observed covariates, compositional data (such as for multiparty voting), time-series data, time-series cross-sectional data, *t* distributed data (such as data with many outliers), and data with logical constraints. We intend to add other modules, and the code is open so that others can add modules themselves.

The program comes in two versions: for Windows and for GAUSS. Both implement the same key procedures. The Windows version requires a Windows-based operating system and no other commercial software, is menu oriented and thus has few startup costs, and includes some data input procedures not in the GAUSS version. The GAUSS version requires the commercial program (GAUSS for Unix 3.2.39 or later, or GAUSS for Windows NT/95 3.2.33 or later), runs on any computer hardware and operating system that runs the most recent version of GAUSS, is command oriented, and has some statistical options not in the Windows version. The software and detailed documentation are freely available at http://GKing.Harvard.Edu.

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Nobility and Necessity: The Problem of Courage in Aristotle's *Nicomachean Ethics*

LEE WARD *Kenyon College*

In the current debate over the status of moral virtue in ethical and political theory, Aristotle is an imposing and controversial figure. Both champions and critics of the ancient conception of virtue identify Aristotle as its most important proponent, but commentators often obscure the complexity of his treatment of moral virtue. His account of courage reveals this complexity. Aristotle believes that courage, and indeed virtue generally, must be understood as both an end in itself and a means to a more comprehensive good. In this way Aristotle's political science offers a middle course that corrects and embraces the claims of nobility and necessity in political life. Honor is central to this political science. It acts as a bridge between the desires of the individual and the needs of the political community and reduces the dangers posed by the excessive pursuit of nobility and the complete acquiescence to necessity.

For centuries, Aristotle's *Nicomachean Ethics* has been widely recognized as the definitive expression of the classical understanding of virtue and ethical training. Alisdair MacIntyre's (1984) influential *After Virtue* proposes the possibility of restoring the Aristotelian conception of virtue as a corrective to the regnant, but in his view increasingly discredited, utilitarian and subjectivist theories of ethics.[1] For MacIntyre, the classical view, with its emphasis on the nobility and inherent goodness of moral action, is superior to the crude self-interest that marks contemporary ethical theory. The understanding of virtue and ethics that MacIntyre opposes originates in Machiavelli's root-and-branch rejection of classical moral virtue in the dawn of modern political theory. The critique of the Aristotelian understanding of virtue in *The Prince* redefined the very possibility of virtue and nobility in light of a conception of human nature that reflects the primacy of the passions and necessity over reason and nobility (Machiavelli [1532] 1985, esp. chap. 15, and [1531] 1996, I:23.2, II:20). For Machiavelli, the classical theory of virtue exemplified in Aristotle was too idealistic and too directed to the moral horizon of the gentleman to be a practical guide for those concerned with the harsh but effectual truths of political power. To the modern view of ethics, so profoundly influenced by Machiavelli (Hobbes [1651] 1994, 100; Locke [1700] 1975, 68–70), the standard of political morality must be what individuals actually do, not what they ought to do.

I shall argue that both MacIntyre and Machiavelli exhibit a partial understanding of Aristotle's complex teaching on moral virtue. My investigation of the treatment of courage in the *Nicomachean Ethics* will show that both the simple acceptance or rejection of

Aristotle's conception of nobility tends to distort his deeper moral teaching. His presentation of the complexity of political life and moral virtue challenges the sharp distinction between ethical and political theory maintained by MacIntyre and Machiavelli. Aristotle attempts to demonstrate both how courage can be understood as a moral virtue and how an appreciation of moral virtue can be accommodated within political life by means of honor. His account of courage, and of virtue generally, reveals the inconsistencies in and ultimate incoherence of the morally serious individual's view of the self-sufficiency of virtuous action, but he preserves and justifies that individual's concern for nobility. In this way Aristotle also rejects the complete politicization or functionalization of the virtues. His treatment of courage offers a defense of moral virtue that can provide guidance to lawgivers concerned with distributing honors for virtue as well as to the morally serious individual in practically any regime. I also shall argue that his treatment of moral virtue encourages the possibility of noble action but at the same time resists the idealization of the noble and offers a far from naïve assessment of the harsh necessities of political life.

Courage, the first moral virtue Aristotle discusses, in several ways serves as a pattern for his treatment of moral virtue as a whole. First, Aristotle argues that courage, like moral virtue generally, must be performed for the sake of nobility, but he also states that courageous acts are subject to praise and blame, and thus subject to the political community, which confers praise and blame through the distribution of public honors. Second, although courage may be more emphatically political than many of the other virtues he considers, Aristotle's account of courage explores important questions relating to moral responsibility, the saliency of pleasure and pain, and the difficulty in distinguishing between the external appearance of an action and the disposition of character that generates it. These issues are central to Aristotle's treatment of all the particular virtues. Third, Aristotle indicates that moral virtue is inseparable from the human capacity for the deliberation and choice required for moral responsibility (Aristotle 1934, 1113b5–8; hereafter

Lee Ward is Visiting Professor of Political Science, Kenyon College, Gambier, OH 43022 (wardl@kenyon.edu).

[1] See especially chapters 5–6 and 10–2. MacIntyre (1984, 159–60) has reservations about Aristotle's defense of slavery and his assumptions about class and the political capacity of barbarians, but he maintains that "these limitations do not necessarily injure [Aristotle's] general scheme for understanding the place of the virtues in human life." The appeal to Aristotelian virtue is also a theme of MacIntyre 1988.

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NE).[2] Inasmuch as courage involves confidence and fear in dangerous situations, in which we have only imperfect control over external forces, it serves as a test case for the very possibility of understanding virtuous action as the product of a morally responsible agent.

Aristotle's treatment of courage reflects an ambiguity in the notion of the moral virtues themselves, which, for Aristotle, must be understood as both ends in themselves and as means to other ends. Far from merely urging noble action for its own sake, as the *Ethics* is typically understood to do (Nussbaum 1986, 295–6; Sherman 1989, 167; Urmson 1988, 62), Aristotle explores ways of life and facets of human nature that threaten decent political life.[3] In light of the problematic character of the noble as well as its attraction, Aristotle urges a middle course that gives its due to both nobility and necessity, to the perspective of MacIntyre's morally serious individual and to Machiavelli's assessment of the harsh realities of political life. That middle ground involves honor and shame. By containing an element of dependence on the judgment of others, honor and shame can bridge the tension between the private interests and concerns of the individual and the political needs of the community.

MORAL RESPONSIBILITY AND CHOICE

Aristotle's detailed discussion of moral responsibility in Book III serves as a preface to his treatment of the particular virtues. He immediately raises the issue of human control over one's actions as the essential precondition for the possibility of encouraging virtue and discouraging vice. Human control is complicated by the need to understand virtue in terms of both emotions (*pathe*), which literally are things done to or that befall a person, and actions (*praxeis*), which are generated by the individual. The attribution of moral responsibility is gauged with respect to the individual's relation to the external world, both as the agent and the recipient of actions. The dynamic and reactive dimensions of virtue make moral responsibility a multifaceted phenomenon. Aristotle suggests this complexity when he distinguishes voluntary actions, which are either praised or blamed, and involuntary actions, which "are condoned, and sometimes even pitied" (*NE* 1109b30–2). From the outset of this discussion Aristotle presents himself as a teacher of statesmen, as he argues that clarifying the difference between voluntary and involuntary actions is necessary for "those examining virtue" and "will also be of service to the legislator in assigning honors and punishments" (*NE* 1109b34–5).[4] Even at

this early point in the discussion Aristotle adumbrates the prominent role honor will assume in his treatment of moral virtue. Yet, his presentation of the issue of moral responsibility also exposes the tension between an individual's act for the sake of the noble and the rewards and punishments offered for his or her action by the political community. Action for the sake of the noble may require a kind of forgetting of the legal and social encouragements for virtue. Aristotle's look at moral responsibility from the perspectives of both individual and community reminds us that the problem of the political character of the moral virtues underlies this entire discussion.

Aristotle begins his account of moral responsibility with a treatment of involuntary actions. These are inappropriate for praise and blame and the legal and social devices of reward and punishment. Although Aristotle argues that these actions can only be condoned or pitied, he nevertheless considers them in his exploration of the ground of virtuous action. The two causes of involuntary action are compulsion and ignorance. Compulsion is evinced when the action originates from outside the individual, making the recipient passive with respect to the action. Actions done through "fear of a worse alternative" are not involuntary. The example Aristotle offers is that of a tyrant who forces someone to perform a base act by threatening his or her family (*NE* 1110a6–7). The individual is forced to make a difficult choice, but the choice is, nonetheless, the individual's. Even the compulsion of a tyrant, who neither rules by law nor distributes public honors to those courageous in battle (*NE* 1115a32; also Aristotle 1984, 1295a18–25; Bodeus 1991, 129; Xenophon 1991, V:1–2, VI:12–6, VII:1–10), does not deprive an individual of the capacity for voluntary action. Even less so would the compulsion of law and honor do so. Acts performed under compulsion are necessarily painful, whereas actions done for their "nobility are done with pleasure," but Aristotle recognizes a kind of "mixed" class whereby men are actually praised for submitting to some disgrace or pain "as the price of some great and noble object" (*NE* 1110a22, b12). Aristotle argues that there are limits to such submission, matricide being one, but he raises here the possibility of praising actions for some "noble object" (*kalon ti*), as opposed to action for the sake of the noble itself. In addition, he suggests that such praiseworthy actions may involve the endurance of pain.

The second cause of involuntary action is ignorance. The ignorance that avoids blame is not a general ignorance displayed in moral choice but a particular ignorance of "the circumstances of the act and of the things affected by it" (*NE* 1111a1). Although "pleasure and nobility between them supply the motives of all actions whatsoever," it is the knowledge of particular circumstances that determines voluntary and involuntary action and hence the very possibility of virtue (*NE* 1110b12). Because Aristotle offers his own work to the

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legislator to assist in assigning rewards and punishments, he suggests the possibility of achieving some degree of control over chance and applying a general understanding of virtue to particular situations. In order to see whether this correction to the problem of particular ignorance can encourage the control of fear and confidence that courage demands, we must turn to Aristotle's discussion of voluntary actions.

A voluntary act is one in which "the origin lies in the agent, who knows the particular circumstances in which he is acting" (*NE* 1111a22). Thus, voluntary action requires freedom from compulsion and a kind of knowledge of particulars. It demands a recognition of chance, but Aristotle also emphasizes that anger and desire do not exempt an individual from moral responsibility inasmuch as "the irrational feelings are just as much a part of human nature as reason" (*NE* 1111b1–2). This determination seems to locate courage firmly within the realm of voluntary actions, insofar as courage along with moderation are said to be virtues of the irrational parts of the soul (*NE* 1117b24). The question of human control over fear and confidence is complicated by Aristotle's locating the virtue involving these emotions in the irrational parts of the soul. Are these parts of the soul educable? More generally, can the human control over chance situations be taught and encouraged with any degree of permanence if parts of the soul are recalcitrant to such education?

Aristotle begins to address these questions in his treatment of choice and deliberation. He argues that an act of virtue must be more narrowly defined than a simply voluntary act, which could include the actions of animals and children. Virtuous acts must be a manifestation of human rationality expressed through deliberation and choice. Aristotle concurs with common opinion that actions originating in the agent can be the product of desire (*epithumia*), spirit (*thumos*), opining (*doxesis*), or wishing (*boulesis*), but he argues that none of these can be identified as choice (*NE* 1111b12–1112a13). Although choice and wish are particularly akin, Aristotle distinguishes them in that choice, unlike wish, cannot aim at the impossible; it must be directed toward conditions within one's control (*NE* 1111b25–30). Choice is a voluntary act preceded by deliberation about things we can affect that, "though subject to rules that generally hold good, are uncertain in their issue" (*NE* 1112b10). Choice and deliberation, then, involve the attempt to control the uncertainty produced by particular circumstances (Bodeus 1991, 39). As such, choice involves means rather than ends. But if virtuous deeds are chosen as Aristotle maintains, must they not then be means to other goals? Yet, Aristotle clearly presents virtuous acts as intrinsically good. To what extent must virtue be understood as both an end in itself and a means to a more comprehensive goal?

Aristotle soon distinguishes the good individual (*ho spoudaios*), who is "the standard and measure of the noble and the pleasant," from the many (*hoi polloi*), who are misled by bodily pleasure (*NE* 1113a33–5). The mass of human beings aim at pleasure, "for it seems to them to be good, though it is not" (*NE* 1113b1). If the good individual is the measure of the noble and the pleasant, however, it is unclear whether the discourse of the broader political community that aims to determine the "advantageous and harmful, the just and unjust," can be granted the status of a solid foundation for virtuous activity (Aristotle 1984, 1253a15). Likewise, if the multitude is driven by pleasure, it is unclear how citizens can be brought to sacrifice and endure pain for the sake of the common good. An important modern view, especially in its Kantian formulation, would preserve virtue by isolating it from the self-interested pleasure seeking of political life, but it was Aristotle's project to preserve a connection between morality and happiness, the noble and the pleasant.

Aristotle suggests that if the fundamental alternatives in human life are the many beasts who seek only pleasure and the rare godlike individuals who alone determine the standards of nobility in abstraction from the political community, then political life and citizenship dissolve into one or the other. Yet, the presumption of moral responsibility, he argues, underlies all political life. This is seen both in the practice of private life and by the practice of lawgivers, who punish and extract redress from those who do evil and "honor those who do noble deeds, in order to encourage the one sort and to discourage the other" (*NE* 1113b25). The role of public honor in encouraging virtue establishes a link between honor and the performance of noble deeds. Between the extremes of the good individual and the mass of pleasure seekers lies the political community, which exercises its own version of deliberation and choice in the conferring of public honors, and which establishes law as a measure of right action. Although it is not clear whether a political community that fully incorporates the perspective of the good man and of the masses is possible, the direction of Aristotle's argument has serious implications for our understanding of courage.

As the discussion of moral responsibility reveals, the virtues are only possible if humans can achieve some degree of freedom from necessity. Necessity emerges as the antithesis of nobility. Aristotle suggests that necessity is rooted in the bodily concerns for self-preservation, physical pleasure, and material well-being. Considerations of necessity threaten the free moral agency required by nobility.[5] An individual may perform a courageous act, for example, not for the sake of its moral excellence but for some other material, social, or political end. In this case courageous action is not freely chosen but is only the means to some other good. It is a necessary means, even if it is not the only possible means. That we have bodies, that we are mortal, necessitates that we consider ends other than the noble. Courageous acts may also be necessitated by dangerous situations, which an individual would rather avoid. Again, our physical mortal lives make us vulnerable to a host of dangers, and in such cases there is even less choice and freedom involved than when

courage is chosen as a means. Both the instrumental and compulsory elements of necessity will recur as problems in Aristotle's account of courage.

Aristotle suggests that the practice of conferring honors is the political manifestation of a general recognition of nobility and an awareness of the human potential for some freedom from necessity. In this light, the distance from the fear of death that courage promises may be necessary for that degree of objectivity required for the deliberative and adjudicative functions of the city.[6] Aristotle supports the city's presumption of the possibility of moral responsibility. This conclusion is suggested if not proven by his claims that "our characters are the result of our conduct," and "we are in a sense ourselves partly the cause of our moral dispositions" (*NE* 1114a10, 1114b24–5). Yet, the extent to which the freedom from necessity required for courage is possible for the individual is left unclear by Aristotle's ambiguous use of the first person plural of the verb: Who are the "we" who are partly responsible for our moral dispositions? Is each individual partly the cause of his or her own character, or is the political community partly the cause of the character of each of its members? The truth, of course, may end up being a mixture of both.

COURAGE AS A MEAN

Aristotle complicates his presentation of the first moral virtue he discusses by designating two passions, fear and confidence, with which courage is involved (*NE* 1115a6; also 1107b1). This produces, in his analysis, two sets of extremes in relation to courage, one involving fear (cowardice and fearlessness, or insensitivity to pain), the other involving confidence (despondency and rashness) (*NE* 1115b25–1116a3).[7] Whatever the subtle internal differences between fearlessness and rashness (or between cowardice and despondency), however, their external manifestations would be virtually impossible to distinguish.[8] This may be why, as Aristotle's discussion proceeds, he collapses the two, speaking of courage as a mean, and cowardice and rashness as the extremes (*NE* 1116a5). Thus his discussion of the moral virtues begins by making fine distinctions about internal dispositions of soul that have no visible external manifestation in the corresponding acts of virtue and vice. Ethical matters, as he told us at the

beginning of Book 1 (*NE* 1094b12–5), do not admit of the same degree of precision as mathematics.

The determinations of cowardice and rashness are made even more difficult when we compare the virtue with the extremes between which it falls: "A brave man appears rash in contrast with a coward and cowardly in contrast with a rash man" (*NE* 1108b19). If appearances are so deceiving, then how is a political community able to make the proper distribution of honors for courage and punishments for cowardice and rashness? How can the community find any reliable means to encourage the virtue and discourage the vices? At this point, Aristotle observes merely that courage may be recognized as a kind of mixture of fear and confidence that involves feeling both in the right way, at the right time, and regarding the right things (*NE* 1115b10–7). Although the right time and the right things depend on particular circumstances, the right way to perform courageous acts is "for the sake of what is noble" (*NE* 1115b12).

The universality of the motive of courageous action—for the sake of the noble—stands in contrast to Aristotle's narrow definition of the circumstances in which courage is displayed: facing death in battle. To support this argument Aristotle dismisses several applications of the term courage applied "by analogy," such as bravely facing a flogging, death by drowning or by disease, and boldly risking disgrace. In the latter case he explains that common opinion may blur the distinction between shamelessness and courage because they both appear to display fearlessness, but he indicates that it is not lack of fear per se that marks courage; rather, it is the correct disposition toward fear in situations that allow noble action. He contrasts the defective character of the shameless individual with the commendable actions of the "equitable person, with a due sense of shame," who displays a fear of disgrace (*NE* 1115a15). The effect of Aristotle's correction of the common praise of shamelessness as courage is to leave room for a virtuous person who acts at least in part for the sake of honor. Like the individual who fears disgrace, the courageous person, despite appearances, is not fearless.

That courage involves risk in battle is "borne out by the principle on which public honors are bestowed in republics and under monarchies" (*NE* 1115a32). Aristotle presents the bestowing of public honors for courageous actions performed in the face of the "greatest and noblest dangers" as an almost universal function of political life (it is unclear whether tyranny would be included as a species of monarchy). Aristotle's argument here, however, has disturbing implications for his larger account of this virtue. He proves that courage involves battle by relying on the judgment of the political community, whereas he defines the courageous act as one performed solely for the sake of the noble. He raises the possibility that the opinions that ground and direct virtuous action are rooted in the city's understanding of those actions.[9] The discrepancy

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[6] Nichols (1992, 91) draws this connection between courage and justice: "While the courageous person sufficiently distances himself from his fear of death that he can risk his life, the judge sufficiently distances himself from the competition for goods that he can consider what a just distribution would be."

[7] Aristotle argues that the excess of fearlessness has no name, but he suggests madness or insensitivity to pain as possible categorizations. He offers no precise definition for the deficiency in confidence, but he calls such a person "despondent" (*duselpis*) in addition to being a coward (*NE* 1116a3).

[8] Hardie (1968, 140 and 145) and Pears (1980, 171) have even suggested that courage operates in two distinct modalities, the one intelligible in terms of fear and the other of confidence. Therefore, they argue it is possible that courage may involve two separate virtues.

[9] Faulkner (1972, 88) makes the point in a somewhat different context that the application of the noble as the universal standard for

between the proof that courage involves risk in battle, which is based on public honors the law bestows, and its purely noble motive reveals a deep ambiguity in Aristotle's treatment of courage.

By identifying courage with facing death in battle—as opposed, for example, to facing flogging or death by drowning or disease—Aristotle suggests that courage must possess some element of self-generated action aimed at controlling chance. Yet, as we have noted, the universal practice of cities in distributing battle honors confirms that courage has a strong political core. Thus, Aristotle indicates that courage requires individual control over chance, but it also emerges in a context greater than the individual. Cities go to war, but Aristotle suggests that the political may not exhaust the moral and intellectual possibilities for courage. Through his discussion of the specious forms of courage, he sheds further light on the tension between these different manifestations of courage.

THE SPECIOUS FORMS OF COURAGE

The potential disjunction between the external appearance of an action and the disposition of the actor emerges as a central theme in Aristotle's account of courage. Although virtuous action must be performed for the sake of the noble, Aristotle observes that many actions which appear virtuous are actually motivated by concerns other than nobility. He finds this difficulty to be inherent in the internal structure of the virtues themselves, but it is particularly prominent in his account of courage, for only there does he treat systematically the specious forms that appear to be the true virtue.

The first imperfect form is citizen courage (he politike), which is divided into two kinds. Citizen troops (1) appear to "endure dangers because of the legal penalties and the reproach attaching to cowardice, and the honors awarded to bravery," or (2) are driven into battle by fear of the commander and the desire to avoid the pain he will inflict for disobedience (NE 1116a15, 30). Either kind is manifestly contingent on conditions extrinsic to the nobility of the act itself, with one variety driven by a desire for honor and a sense of shame and the other by the avoidance of pain and fear of the commander.

Although Aristotle distinguishes citizen courage from true courage, he insists that its highest form "most closely" resembles true courage because it is "prompted by a virtue, namely the sense of shame, and by the desire for something noble, namely honor" (NE 1116a27). He thus corrects his earlier statement that shame is not a virtue but an emotion (NE 1108a30) and calls honor, which he earlier distinguished from virtue because of its dependence on the opinion of others (NE 1095b25–6), a noble thing (Joachim 1951, 116).

The importance of this supposedly specious form of courage is suggested as well by his use of an expression for citizen courage identical to that used in Book I to refer to the architectonic art of politics.[10]

Aristotle illustrates the two forms of citizen courage, based on love of honor or fear of commanders, by quotations from Homer (NE 1116a22–5, 34–5). The desire for honor possessed by Homeric heroes presupposes that they alone are responsible for their virtue. Only if it were self-generated would they deserve the honor they crave. They are prime candidates for a teaching that courage is for its own sake, or for the sake of the noble, which implies that their virtuous deeds are utterly free. But the presupposition of their heroism, the self-sufficiency of courage, is based on a delusion, for they do desire honor, even if as Aristotle tells us, honor is a noble thing. He does not merely reveal their contradiction; his insistence that courage requires facing death in battle corrects their delusion of noble self-sufficiency, because the city and its need for defense are both the occasion and the end of courage. This view of the Homeric heroes, and their delusion that their actions can escape all compulsion or necessity, leads to a brutalizing of politics. Deluded by his self-sufficiency, for example, Achilles watches his fellow Acheans slaughtered by the Trojans. (It is no accident that Aristotle uses the Homeric heroes as examples not only of an excessive love of honor but also of those who brutally compel their own soldiers to fight.) There is similarly a certain logic of harsh imperialism reflected in Pericles' view of the Athenian empire as a project freely chosen (Nichols 1992, 128–9; Orwin 1994, esp. chap. 1; compare Strauss 1964, 28).

Aristotle's implicit criticism of the Homeric heroes' delusion of self-sufficiency elucidates the defect in MacIntyre's attempt to view the virtues in total separation from considerations of utility or instrumentality. At the same time, by insisting on the specious courage of those who would skulk off the field if not for fear of the commander (NE 1116a34),[11] Aristotle refuses to reduce virtue to necessity in the manner of Machiavelli.[12] Perhaps somewhere between the proud Homeric commanders and their cowering soldiers lies Aris-

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virtuous action does not in itself supply the knowledge of the particular things we are to do. I concur with his suggestion that, for Aristotle, the law is the primary source of these more particular opinions, and "to this extent human action is not free, but decisively subject to the law and the community that forms the law."

[10] In both cases, Aristotle simply uses the feminine form of the adjective "political." Whereas it must be understood to modify courage in the discussion of that virtue (NE 1116a17), it presumably modifies techne in the phrase usually translated as "political science" (NE 1094a30, b15; 1141a21, b24). In this manner Aristotle diverges from the way he identifies political justice, to politikon dikaion (NE 1134a25–30, b18).

[11] There is a discrepancy between the rendition of this passage by Aristotle and by Homer (1951, II: 391). I agree with Charney (1988, 69–70) that Aristotle's presentation of true courage points to the deficiencies of the Homeric presentation, but I disagree with her conclusion that Aristotle's aim is to reveal that the "bravest acts of political courage are a form of cowardice." Rather, I contend that the combination of dependence and compulsion in citizen courage is related to Aristotle's argument that the poetic-heroic account of courage belies the complexity and variety of motives attending political action (compare Hardie 1968, 106; MacIntyre 1984, 157).

[12] Although Machiavelli advocates honor-loving commanders, he does so in a way that emphasizes the inescapable necessity that grounds this natural appetite. Moreover, he does not hold the courage driven by compulsion as any less moral than that freely generated. See the example of Hannibal in The Prince, chapter XVII

totle's examplar of courage, at once less scornful of necessity than the hero and yet more capable of moral responsibility than the impressed soldier.

The second specious form of courage relies on a certain kind of knowledge acquired through experience. The chief example Aristotle cites is "the type of bravery displayed . . . by [professional] soldiers" (*NE* 1116b3–6). He admits they are superior fighters to courageous men, just as the armed will ordinarily conquer the unarmed, and the stronger the weaker (*NE* 1116b13–5), but theirs is a specious form of courage. In view of the preceding discussion, we would expect Aristotle to explain that the noble, not knowledge or expertise, defines true courage. Surprisingly, he emphasizes the unreliability of professional troops. Because their confidence is based on their superiority to the enemy, they take flight in reversals of fortune, "fearing death more than disgrace" (*NE* 1116b19–23). Aristotle now praises the citizen soldiers (*ta politika*), for whom "it is shameful to flee, and for whom death is preferable to such safety" (*NE* 1116b20).

Aristotle's criticism of the courage of experience, moreover, relies on a crucial abstraction from the issue of time (Sherman 1989, 178–9 and 191–2). Aristotle fails to acknowledge that the inevitable result of the consistent exercise of courage in battle over time is experience. The virtues, he stated earlier, are inculcated by the repeated performance of certain acts (*NE* 1103a17). How, then, are we to distinguish the confidence arising from experience and the confidence properly belonging to true courage? The performance even of truly courageous acts must be a mixture of action for the sake of the noble and action affected by experience. Experience in battle can provide knowledge of the correct means to perform noble acts.

The third specious form of courage is spiritedness, which appears to be true courage because it is both a part of courage as well as its "most natural" form (*NE* 1116b24). The naturalness is rooted in a certain unflattering affinity between humans and enraged wounded animals. Aristotle admits that the truly courageous are motivated by nobility, but "spirit operates in them as well" (*NE* 1116b31). Spiritedness "when reinforced by deliberate choice and purpose . . . appears to be true courage" (*NE* 1117a5).[13] Aristotle's distinction between spiritedness and courage becomes even more obscure when he argues that spirited warriors are not guided by "reason" but by "emotion," for "the motive of their confidence is not the noble" (*to kalon*) (*NE* 1117a8).[14] If emotion is inconsistent with nobility, then how can spiritedness operate in true courage?

By forcing us to see the expanded role of reason in

virtue, thus distinguishing animal courage from true courage, Aristotle directs us to the problem of attempting to identify courage in abstraction from its particular causes. The connection between reason and courage allows the possibility of a broader meaning of purpose, different from the noble itself. In Aristotle's model of nobility, for example, how can we distinguish the actions of the citizens of a good city in a just cause from a bad city in an unjust cause (Jaffa 1952, 80–2)? I agree with Jaffa that the problem of patriotic courage lies in its connection to the varying forms of legal justice, which means it cannot be true courage in a certain or strict sense. Yet, inasmuch as courage is an emphatically political virtue, and legal justice varies by regime, the very possibility of regime in some sense requires courage, and thus any understanding of true courage would possess a patriotic dimension. Perhaps Aristotle's argument that spiritedness plus deliberate choice and purpose appears to be true courage suggests the importance of political regime for the virtues, inasmuch as spirited citizens defending a good city—one in which the citizens engage in deliberation and choice— becomes the closest instance to true courage that we can perceive.

The fourth kind of specious form is the courage of "good hope" (*NE* 1117a10).[15] It resembles true courage, because "both are confident" (*tharrousin*), but it differs in that it cannot endure changing tides of battle. The courage of hopefulness is born from an expectation of victory, even easy victory, and the confidence it produces is readily shattered by sudden reversals of fortune in battle. True courage, Aristotle argues, is manifest in "unforeseen dangers," when it "springs more from character" and "there is less time for preparation" (*NE* 1117a20). Courage in "sudden alarms" will be displayed because "it is noble to do so and base not to do so" (*NE* 1117a15–9). The problem is how to harmonize this fixed disposition displayed in sudden alarms with the deliberate choice that Aristotle associates with true courage in the previous section on spirit.[16] Although correct split-second battlefield decisions are certainly not impossible, Aristotle suggests that the conditions of battle clearly militate against the careful deliberation he identifies with virtue in his earlier treatment of moral responsibility. How, then, is the individual to know what is noble to do in each particular situation without engaging in careful deliberation and choice? In this section Aristotle seems to reduce the courageous individual to a kind of noble machine, an automaton that acts courageously but ceases to be a deliberative agent. Only if human beings were automata could their action be generated solely

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([1532] 1985, 67) and the examples of Manlius Torquatus and Camillus in *Discourses on Livy* ([1531] 1996, Book III, chap. 22–3).

[13] Mara (1995, 290) makes the interesting observation that Aristotle's use of Homer to illustrate spirited courage, as well as courage by compulsion, reveals that Homeric courage in Aristotle's presentation registers both an internal and external impulse at variance with true courage. Compare Charney 1988, 73.

[14] Rackham (Aristotle 1934, 169) quite inexplicably translates *to kalon* as "honor" at this point, but a more literal translation as "the noble" seems perfectly in keeping with the sense of Aristotle's argument.

[15] Here I depart slightly from Rackham, who translates *euelpides* as sanguine.

[16] Reeve (1992, 92–3) sees courage as an example of virtuous action involving "instantaneous or compressed deliberation and decision." I agree that actions performed from a "virtuous state" will be, at least indirectly, the result of prior deliberation and decision, but this does not negate the complexity of the issues explored in the account of the courage of experience. If anything, Aristotle seems to be indicating the difficulty of distinguishing actions arising from confidence and experience from those due to deliberation and choice; the former he criticizes, and the latter he applauds.

for the sake of the noble without any other, extrinsic consideration.

The fifth and final specious form of courage is produced by ignorance. This imperfect type resembles "good hope" but is inferior because its practitioners lack "sufficient worth" (*NE* 1117a24). The single example of the Argives who mistook the Lacaedaemonians for Sicyonians recalls the common thread in all the examples of specious courage (with the notable exception of the higher form of citizen courage); namely, it involves an initial display of confidence that crumbles in the course of battle. True courage clearly requires some knowledge of the particular danger one is facing. In view of Aristotle's initial argument that voluntary action requires knowledge of "particular circumstances," we are left to wonder how a general concern with acting solely for the sake of the noble can be made compatible with the knowledge of particular circumstances required to escape the false courage produced by ignorance.

Aristotle's account of the five specious forms of courage raises serious problems for his presentation of true courage. His inability to sustain a logic of action for the sake of the noble separate from extrinsic causes is revealed in the problematic and often contradictory character of this account. The example of courage provides the paradigm for Aristotle's understanding of the potential tension between an individual's internal disposition and the appearance of his or her external actions. Courage, inasmuch as it relates to the correct disposition toward fear, is particularly open to the confusion of external appearance and internal reality.[17] The etymological origins and development of the Greek word for fear (*phobos*) had associations both with fear as an internal feeling and with flight as an actual physical activity.[18] Aristotle suggests that the appearance of flight need not necessarily reflect the actions of individuals unable to control their fear, just as individuals who, at least initially, hold their ground in battle may do so not from true courage but because of hopefulness, ignorance, or compulsion.

PLEASURE AND PAIN

The difficulty in Aristotle's attempt to separate courageous acts for the sake of the noble from any extrinsic causes is clearly exposed in his account of the centrality of pleasure and pain in our understanding of courage. Aristotle originally presented courage as a mean relating to fear and confidence, but he now reveals that it mainly deals with fear and pain. "Courage itself is

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[17] This potential disjunction between external appearance and internal disposition also can be seen in Aristotle's treatment of other virtues, such as moderation, liberality, and truthfulness (*NE* 1108b19–23, 1127a27, 1127b8–9).

[18] Aristotle uses *phobos* to mean fear, but at least since the time of Homer the word also had strong connotations of physical flight. The same is true for the verb *phobeo*, which can mean either to frighten or to put to flight. This deep connection between the internal sensation and the external manifestation presents an ambiguity that Aristotle raises and explores in his discussion of the specious forms of courage. For another perspective, consider Herodotus (1988, 8.87–8) and Ward's (1999, 12–4) excellent treatment of this story.

attended by pain; it is justly praised, because it is harder to endure painful things than to abstain from pleasure" (*NE* 1117a35; compare 1117b25, 1119a5). Although he previously argued that the pleasantness of courage "is obscured by the attendant circumstances," he now concludes that the particular circumstances tell a truer tale than he previously suggested: "It is not true therefore of every virtue that its active exercise is essentially pleasant" (*NE* 1117b15). He moves even farther away from his initial argument that "the man who does not enjoy doing noble actions is not a good man at all" (*NE* 1099a17). To understand this dramatic shift in Aristotle's position, we must analyze his analogy to the boxers.

Aristotle employs this analogy to reveal how courage can be understood as a mixture of pleasure and pain or, more precisely, an endurance of pain for the sake of a later pleasure. For boxers, the blows and training are painful, but the end, "the wreath and honors of victory," is pleasant. This remarkable image brings some of the earlier problems into sharper focus. Does the warrior's end also include the pleasure from gaining wreaths and honors? Does the nobility of the warrior's end depend on victory? The boxer analogy exposes the tension between the noble and the good more clearly than Aristotle has suggested hitherto. Although earlier he argued that truly virtuous action is not supposed to have an end extrinsic to itself, now he ponders whether men "of flesh and blood" will endure pain for the sake of the pleasure inhering in a "small thing," presumably like a wreath or honors (*NE* 1117b5).

Aristotle exposes the tension between the pain involved in noble action and the unquestionable good of preserving one's life, especially if one enjoys a happy life. If the pleasant end is dwarfed by the attendant pain, then it is difficult to see how nobility alone can sustain courage. Aristotle concedes as much when he reveals that, contrary to expectation, the more an individual has every virtue and is happy, the more s/he will be pained by death, for s/he has more to lose and knows it (*NE* 1117b17). But these people are not the less courageous, Aristotle observes, and perhaps are even more so, because they prefer the noble in war to other goods. Aristotle nevertheless concludes that they may not make the best soldiers; those less courageous who have no good beside life to lose will therefore exchange it for a small gain. People of the greatest virtue have more difficulty sacrificing their own good for the good of the city than the most selfish mercenary.

The conclusion of the boxer analogy, which suggests that despite specious courage the professional is a better soldier than the truly courageous individual, must be seen in relation to Aristotle's earlier statements ranking citizen soldiers above professionals (*NE* 1116b15–20). Both the virtuous individual and the mercenary, paradoxically, abstract from the complexities involved in the action of a citizen soldier, who acts from a variety of motives and for a larger purpose, namely, the defense of the city. The citizen's defense of the city reflects the composite associations and layers of commitment to private and public interest that

characterize political life (Salkever 1986, 252).[19] Because professional and true courage are free from the concerns of any particular city, they both present problems for the city and its needs. In Aristotle's reevaluation near the end of the discussion of true courage, the citizen soldier, with his sense of shame and desire for honor mixed with compulsion and dependence, emerges as the better defender of the city than the truly courageous man.

Aristotle's treatment of the difficulty that pleasure and pain present for nobility in courage prepares for the refinements in his later argument in the *Ethics*. The intractable character of pleasure and pain plays a role in his discussion of the moral virtues generally, all of which, Aristotle says, involve pleasure and pain (*NE* 1105a12, 1109b4; but see 1126a20, 1126b11–36, 1128a7–27). The ubiquitous and ineradicable human desire for pleasure and avoidance of pain, Aristotle suggests in this discussion, belies a simple understanding of human nature presumed by the perspective of the noble. Aristotle's original presentation of nobility is complicated by his later suggestion that human nature "is not simple" (*NE* 1154b22–32). He implies that only if human nature and the dispositions of character were simple and unchanging would nobility, as opposed to other states of character, be the sole condition for virtuous action.

Aristotle ends the discussion of courage with this observation: "From what has been said it will not be difficult to form . . . a rough outline of its nature" (*NE* 1117b22). Perhaps the specious forms of courage that comprise this "rough outline" are not as discrete and distinct as his enumeration of them suggests. His admission that the specious forms often resemble one another and his argument that spirit operates in true courage suggest that the categories are separable in speech but may not be so in deed (*NE* 1102a31–3). Aristotle may not be juxtaposing true courage to the specious forms; rather, he may be indicating that courage must be properly understood as a composite, more closely resembling the composite association that it serves. From this perspective, the specious forms expressed in common civic discourse and rooted in notions of politics (*politike*), experience (*empeiria*), spirit (*thumos*), hopefulness (*euelpis*), and even ignorance (*agnoia*) can be understood as elements that may or may not operate to various degrees in particular acts of courage, although none alone can be simply identified with courage.

The tension in Aristotle's account between the necessity underlying courage and its appearance as virtuous action for the sake of the noble points to his own understanding of the complexity of political life. Aristotle presents courage as a trait that distances human beings from necessity. The practice of the virtues points beyond mere bodily pleasure toward a notion of the good that is more consistent with our awareness, however dimly, of our place in the whole. The good and the pleasant are not identical. Yet, his discussion ends in *aporia* inasmuch as the all-too-natural desire for pleasure and fear of pain hinder courage in battle. Insofar as courage, moreso than all the virtues, deals with the fear and pain revolving around self-preservation (a concern Aristotle argues that we share with the lower animals), the noble conflicts with a narrow but seminal construction of the good as the preservation of life itself (*NE* 1094b19). Each specious form of courage contains, to varying degrees, an animating principle of self-preservation that resists the stringent demands of the noble. Political life requires that human beings somehow transcend their animal, necessitous nature, but Aristotle's conclusion to the section on courage indicates that this is not a natural or inevitable process.

Yet, Aristotle also abstracts from necessity to present the virtues as something like beautiful symbols of our self-sufficiency, in light of which we understand ourselves. The virtues have no ends outside themselves to which they are subordinate and no causes other than the individual's free choice of good actions for their own sake. That we act for the sake of the noble is both an illusion and a sign that we are capable of rising above necessity. The virtues reflect the human desire to be free of accident, chance, and necessity. Although Aristotle's discussion of courage is saturated with the language of *kalon*, as we proceed to unearth some of its internal contradictions, the virtues become unintelligible as ends in themselves. They begin to appear as willful impositions not unlike Glaucon's polished statues of the just and unjust man (Plato 1968, 361d4). Just as Glaucon demanded that Socrates make justice perfectly visible, revealing the being of the just and unjust men in their appearance, Aristotle at first presents true courage as the perfect harmony between body and soul. We come to suspect that virtuous action cannot be understood separately from its consequences. The treatment of the specious forms of courage in particular calls attention to the intent behind virtuous action and the difficulty in distinguishing the true and apparent causes of human action. In the single chapter on the specious forms of courage, some form of the verb "to seem" (*dokein*) occurs nearly twice as often as in the other three chapters on courage combined.[20] To understand Aristotle's purpose in presenting courage in the manner he does, we must examine the tension between nobility and necessity.

THE TENSION BETWEEN NOBILITY AND NECESSITY

Much has been written on the rhetorical aspects of Aristotle's political and moral teaching (Ambler 1985; Nichols 1987; Tessitore 1996).[21] My previous discus-

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[19] My point here runs counter to that of Salkever (1986, 252), who argues that the problematic character of Aristotle's account of courage indicates an attack on the ancient Greek idea of political virility and is meant to blur the distinction between public and private things. I believe Aristotle's intention is to show the connection between the public and private concerns that are interwoven in the complex association of the city. Of course, to show a connection between two things presupposes a more primary distinction.

[20] It appears seven times in Book III, chapter viii, but only four times throughout chapters vi, vii, and ix.

[21] For different treatments of the audience of the *Ethics*, see Tessi-

sion of the problematic features of Aristotle's account of courage may shed light on his larger rhetorical purpose in the *Nicomachean Ethics*. Just as Aristotle presents each particular virtue as a mean between two extremes, his presentation of courage suggests that virtue can be viewed as a mean between nobility and necessity.

Aristotle exposes the problem of nobility by demonstrating the difficulty of grounding true courage solely in action performed for the sake of the noble. This severing of virtuous action from particular or contingent circumstances makes the possibility of any such action uncertain, as Aristotle indicates in his discussion of magnanimity (Tessitore 1996, 28–35).[22] Magnanimity is an ornament (*kosmos*) of the virtues, without which they are incomplete and therefore not virtues (*NE* 1123b30, 1124a2; Davis 1996, 4). Yet, as Aristotle makes clear in his account of the magnanimous individual, the concept of the noble itself produces a kind of illusion of self-sufficiency; nobility may provide an idea of the end of an action but no enduring motive for its performance. The noble is not a satisfactory motive for the performance of virtuous action because it lacks specific content, being an abstraction from particulars.

The magnanimous individual (literally, "greatsouled" [*megalopsychos*]) in Book IV exemplifies this illusion of self-sufficiency, for he "wonders at nothing," and "nothing is great to him" (*NE* 1125a3). This profound detachment is seen in his resistance to recognizing his debts and his general idleness (*NE* 1124b10–5, 26). It is worth noting that, in the initial list of virtues in Book II, magnanimity is described as a mean concerning honor and dishonor (*NE* 1107b21–3), but in the thematic treatment in Book IV, greatness, not honor, becomes the measure of magnanimity. To the great-souled, "even honor is a small thing" (*NE* 1124a19). Whereas honor and dishonor are supplied by the political community, greatness, Aristotle indicates, is not dependent on recognition. These individuals are honored for being great or doing great things, but they are not great because they are honored. For Aristotle, greatness itself, like nobility itself, exists independently of politics.

This illusory character of the noble is compounded by the dangers of excessive self-love in a city's prominent figures. The detachment encouraged by a concern for nobility can fuel the attempt to transcend the bounds of political life in a desire to experience the enjoyment of performing noble acts, which Aristotle presents as the only true reward for such action. Magnanimous individuals can experience this pleasure in a manner detached from the good of the political

community or even to its detriment (*NE* 1124a19, 1124b10–5, 1125a19).[23] In the pursuit of the noble, they forget their human needs, mirroring the truly courageous individual who forgets the elements of experience, compulsion, ignorance, spirit, and desire for honor that operate in courageous action.[24] And it goes without saying that if honor is a small thing to magnanimous people, of even less significance would be the bodily pleasures that most humans seek. Their intense gratification lies elsewhere, in a disdain for the many particular needs of human beings, which is potentially detrimental to the political community. Thus, the desire for the noble distorts our conception of politics and encourages a kind of idealized individualism that makes decent political life problematic.

Because Aristotle's presentation of the noble abstracts from the extrinsic causes of courage, it also obscures the fundamental causes of war. This difficulty may be seen more clearly if we reconsider citizen courage. The compulsion and dependence in citizen courage manifests a love of one's own, reflected in private property, as the cause of war most consistent with the city's interests. In contrast to courage for the sake of the noble, citizen courage is more closely related to the actual causes of war, such as the defense of hearth and home. The Guardians in Plato's *Republic*, whom Aristotle criticizes in Book II of the *Politics*, are rigorously educated in the noble, and their communized condition removes from them the motivations for citizen courage (Aristotle 1984, 1260b25–1264b25).

Aristotle identified two major problems with Socrates' communistic proposal: It leads to an excessive unity, which destroys the layers of association that compose the *polis*, and it undermines the virtues by abolishing the equipment required for their performance (Aristotle 1984, 1261b1–15, 1263a25–b13). These concerns apply to the noble itself as Aristotle presents it in the *Ethics*, for both the noble and Socratic communism abstract from particular physical and political necessities. By completely denying the instrumental and compulsory aspects of virtuous action, the aspirations of the noble can only find expression in a condition that allows the individual to act out of the pure motive of pursuing moral excellence, a disposition unsullied by concerns for the social and material demands of political life. Only after the abolition of private property and the private family can the Guardians perform virtuous action for its own sake, without the intrusion of such extrinsic motives as particular material concerns and private desires.

Necessity rooted in bodily pleasures and pains presents even greater difficulties than the noble in Aristotle's account of courage. Most human beings are driven primarily by pleasure, and those without self-restraint who act solely out of desire cannot exercise choice in

the fullest sense (*NE* 1113a35). Aristotle's inability to ground true courage in the needs of the city may be symbolic of the enormous problem pleasure poses for political life. Of the three ways of life posited in Book I—honor, pleasure, and contemplation—Aristotle dismisses from the discussion there the life dedicated to pleasure as "utterly slavish" and fit "only . . . for cattle." Yet, he acknowledges that it cannot be so easily dismissed, at least from discussions of politics, when he admits that the life of pleasure characterizes "the many" (*NE* 1095b19).

Necessity seems destined to make political life inherently tragic, because the "many take pleasure in things that conflict with one another" (*NE* 1099a12). Political life simply dissolves into the competition for material goods. The slavish may perform a kind of service for the political community—apolitical mercenaries "will barter their lives for trifling gains"—but the political possibilities for humankind are severely restricted when such mercenaries are more useful soldiers than the noble among the citizenry (*NE* 1117b20). If the slavish life dedicated to bodily pleasures and necessities is antithetical to decent political life, then Aristotle may be cautioning about the disadvantage of the idealized individualism of the few: If most human beings are unfit for political life, then where does one find man the political animal (Aristotle 1984, 1253a2)?

Aristotle's discussion of the noble serves as a way to gain distance from particular necessities and to allow reflection on the good in contradistinction to the pleasant. As Faulkner (1972, 86) suggests, perhaps "in moral science some exaggeration of human powers is salutary." Aristotle exposes a contradiction in the political understanding of virtue. The city projects a standard of nobility in order to make political life possible, by pulling human beings out of their necessitous animal nature, but the consequence of the extreme pursuit of nobility is an abstraction from the city's very real needs. It is precisely this exaggeration of human powers that produces the tension between the noble and the good. The tension is resolved in part by the notion of honor, which has as one component the recognition of our goodness in and by others, a recognition that allows us to avoid the dangers posed by the excessive pursuit of the noble and by the complete acquiescence to necessity.[25] Moreover, the recognition bestowed in honor can be modified and purified in the relations of justice, equity, and ultimately friendship, which both constitute human happiness and mark decent political life.

Aristotle's treatment of honor is as complex as his account of courage. In Book I, honor and the life of action are depreciated in the following terms: "Honor

after all seems (*dokei*) too superficial to be the Good which we are seeking; since it appears to depend on those who confer it more than upon him upon whom it is conferred" (*NE* 1095b25). Honor cannot be the good to which all actions aim because it depends on the judgment of others and points beyond the act itself to the desire to be recognized for one's virtue. Here Aristotle seems unequivocally to subordinate honor to virtue, or action for the sake of the noble. But as we move through the account of courage, as we have seen, the distinction between the noble and honor becomes blurred. Perhaps it is Aristotle's initial criticism of honor that will emerge as too "superficial."

Aristotle criticizes honor for encouraging dependence on the judgment of others, but it combines desire for pleasure with virtuous action in a manner that benefits the city. The awareness of one's imperfection that is signified by the desire for honor allows virtue to be chosen as both an end in itself and a means to another more comprehensive good. Honor, the active moral principle of the citizen soldier, links the individual and the community in a way not possible in the perspective dominated by the extremes of nobility and necessity. One is publicly honored both for one's own merit and for service to the political community. In describing honor as "something noble," Aristotle defines the relation between the two as that between the particular and the universal (*NE* 1116a28). The noble transcends any particular, and honor operates as a particular manifestation of the noble expressed through public and private rewards.

What is only implied in the *Ethics* is brought out more clearly in the *Politics*. There Aristotle (1984, 1281a1–3; see also 1252b29) explains that the *polis*, which comes into being for the sake of the necessary, or mere life, continues for the sake of the noble or good life. This double character of the city requires that virtue operate as both an end and a means. As Nichols (1992, 92) suggests, perhaps the entire account of courage has led to this signal recognition that the military element in the city most clearly "stands on the border between necessity and nobility, between preservation of life and living well." Honor may act as a bridge between these two aspects of political life. Courage, then, is paradigmatic for our understanding of the virtues inasmuch as the importance of public recognition of noble acts performed out of necessity is seen most clearly in the case of courage, which literally deals with the very survival of the political community.

CONCLUSION

In presenting his view of honor as the political expression of a kind of mean with respect to the excessive pursuit of nobility and the complete acquiescence to necessity, Aristotle offers a remedy to the dangers posed by both the MacIntyrean and the Machiavellian understanding of the possibilities for moral virtue. Aristotle signals that the attraction toward idealized individualism seen in MacIntyre's praise of virtue for its own sake is animated by the intense personal enjoyment of virtuous actors seeking to claim all the

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[25] Whereas Mara (1995, 281, 291–3) views Aristotle as critical of honor inasmuch as it promotes "the attempt to exert control over others," I argue that Aristotle presents nobility, not honor, as the dangerous impulse toward imperial domination. Indeed, Aristotle views honor as a corrective to the problem of the noble. I believe he rehabilitates honor and corrects the perspective of the morally serious as a way to resist the complete functionalization of the virtues by a view of political life, narrowly understood.

reward of nobility, or as much as possible, for themselves. The unbridled pursuit of nobility as an expression of self-love can be destructive to decent communities when individuals seek to transcend the constraints of political life. Aristotle identifies dangers in the pursuit of nobility that MacIntyre largely ignores.

Yet, in recognizing honor as a way to identify private pleasure with the public good, Aristotle articulates a means to avoid those dangers, and he does so in a manner that also avoids Machiavelli's tendency to redefine honor simply in terms of necessity. Even Machiavelli's attempt to make the love of honor the animating principle of the citizen soldier stands in the service of his critique of all previous understandings of moral virtue.[26] For Machiavelli, the harsh necessity rooted in the demands of individual self-preservation and communal survival subsumes and redefines the notion of honor in terms of this inescapable necessity. As such, his appeal to the love of honor is inseparable from the love of acquisition and the lupine politics that reflect his view of the fundamentally necessitarian character of human existence. In contrast, Aristotle's treatment of honor presents a greater scope for the human capacity to transcend necessity. The concern for moral excellence may always be tied to the needs of political life, but the very possibility that human beings may act as morally responsible agents in pursuit of an end beyond direct physical and political necessity—in a way neither totally instrumental nor utterly subject to compulsion—suggests more elevated possibilities for moral virtue and political life than Machiavelli allows. Aristotle's view of the possibilities for moral virtue is at once more constrained than that of MacIntyre and more free than that of Machiavelli.

Because Aristotle's discussion of courage blurs the distinction between nobility and honor, it forces a reassessment of the position of shame in the realm of the moral virtues. Early on Aristotle argues that, although "it is praised," shame "is not a virtue" (*NE* 1108b1). It cannot be a virtue at this point in the account because the context it presupposes is fundamentally defective: having done something ill for which one is ashamed. Later, however, shame emerges as a state of character between virtue and vice that typifies the vast majority of human beings (*NE* 1150a10–5). Shame and honor provide a correction to Aristotle's presentation of nobility because they register a cognizance of the inherently mixed character of human actions, the actions of beings who always, at least potentially, are disposed to vice. Aristotle implies that this is the structure of the virtues (*NE* 1129b20–5; Davis 1996, 3). Indeed, he reveals in the *Politics* that war, and therefore courage in battle, is at best a necessary evil (Aristotle 1984, 1333a35). Thus, courage

highlights the presupposition of potential vice that underlies his broader treatment of moral virtue.

The ambiguous nature of shame, which marks the "man of middle character," surfaces in Aristotle's attribution of desire for honor and fear of shame to the citizen soldier.[27] Perhaps the citizen's fear of shame, which depends in a crucial sense on the judgment of the community, reveals the full import of the earlier and somewhat cryptic reference to the "man of middle character" (*NE* 1108a34). The formulation that shame is not a virtue per se, but reveals a middle character, confirms that there are other medial states in addition to virtue (Burger 1991, 127; Pears 1980, 172, 176).[28] I suggest that shame occupies a kind of middle ground between virtue and vice; it supplies an awareness of one's deficiency in failing to perform a virtuous act, and hence reveals the "middle character" of those political acts rooted in necessity, but points to higher ends. Also, the religious connotation of shame evident in Aristotle's account of citizen courage at the Battle of the Temple of Hermes (*NE* 1116b19) may allow for a consideration of piety in his political science that is not immediately apparent in an otherwise radically secular account of courage as a moral virtue.

Another important function of honor is its capacity to link Aristotle's notion of virtue to his idea of justice and friendship. The role of the legislator in distributing honors is a reminder that the people who act solely for the sake of the noble forget their dependence on the city and its laws. These individuals lack self-knowledge. The final effect of Aristotle's discussion of courage is to challenge the simple distinction between the moral and intellectual virtues. How does one know it is right to be moral? Aristotle's implicit response—that the knowledge of the actor relies at least somewhat on the confirmation of the community—is only a partial answer. Knowledge of the rightness of moral action involves recognizing and harmonizing both self-interest and communal interest, which seems to require the perspective of the lawgiver or political philosopher. This dual aspect of public honors, that is, their material cause is the community's needs and their efficient cause is the legislator's knowledge of the correct principles for distributing honors, provides an example of the practical-moral and theoretical-intellectual virtues that mark Aristotle's political science.

The discussion of courage foreshadows the prominent place Aristotle gives to distributive justice in Book V. The legislator distributes honors and thus encourages virtue and discourages vice. To some extent both courage and justice support the perspective of the morally serious individual. Aristotle presents these

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[26] Machiavelli's subsumption of honor into necessity is a major feature of his argument for the indefeasible character of the human passions. A dramatic example of this argument is his reflection on the effects of the Agrarian Law in Rome (Machiavelli [1531] 1996, I.37.1). For two recent studies that powerfully illustrate Machiavelli's understanding of the inescapable necessity rooted in self-interest and the love of acquisition, see Mansfield 1996 and Sullivan 1996.

[27] For the attribution of shame as a motive for citizen soldiers, see *NE* 1116a27–30. It is important to note that in this passage and at 1108b1, where he refers to shame as a mean, Aristotle uses *aidos* rather than *aischune*. The latter signifies shame or disgrace in a negative sense, as attached to bad acts, whereas the former has a positive religious connotation, such as awe and reverence. See also Plato 1984, 12b1–c3.

[28] I agree with Hardie that courage is inextricably bound with shame, but I see its role as more pervasive than simply providing a self-regarding motive "for not seeking to survive at any cost" (Hardie 1968, 330).

virtues not only as connected to actions that are intrinsically praiseworthy but also as revealing the harsh necessity underlying political life. Courage is important not merely because it is good in itself; justice is important because it is concerned with the equitable distribution of goods, of which there is not an inexhaustible supply (*NE* 1129a30–b6, 1132b35). Aristotle shows that the perspective of the morally serious individual must be corrected in light of politics, but he also argues that the *polis* must resist the total functionalization of the virtues. His concern to support the distribution of public honors to morally serious individuals is a reminder that the *spoudaios* should remain a kind of standard for citizenship, a standard for both legislators and the citizenry. The significance of the assignment of public honors, a key element of distributive justice, is indicated in the introductory statement in Book III: The treatment of moral virtue will be of use to both "those examining virtue" and "to the legislator in assigning honors and punishments" (*NE* 1109b35).[29] In light of the problems in Aristotle's account of courage, we are prepared to accept the possibility that "the legislator" and "those examining virtue" are not necessarily different kinds of individuals. Aristotle both accepts the practical failure of most regimes to provide a sound moral education to the citizenry and offers a way to correct this problem.

The position of the morally serious individual (*ho spoudaios*), who is "the standard and measure of the noble and the pleasant," is mirrored in Aristotle's presentation of the courage that is born out of concern for nobility. Aristotle's exposition of the limits and shortcomings that attend the specious forms of courage popularly misconstrued in active political life elucidates the problems faced by legislators who are concerned about the proper moral education of the citizenry. Both the morally serious individual and the *Nicomachean Ethics* provide salutary examples of the moral freedom of which humans are capable. By disclosing the link between virtue and honor, Aristotle's treatment of courage presents the virtues as both worthy of practice by morally serious individuals and as emphatically honorable from the perspective of the community's political leaders. Aristotle admits that moral science can never reach the degree of precision of mathematics, which suggests that honoring all the virtues for the correct reasons has not, and may never be, within the capacity of most political regimes. Yet, he indicates that the ability to encourage certain actions through the bestowal of public honors is a ubiquitous feature of all regimes (*NE* 1113b25). His treatise on moral virtue offers the possibility of more fully incorporating the morally serious individual into the political life of actual regimes because it provides guidance for the legislator who wants to encourage the practice of the virtues among the citizenry in order to produce a morally serious city.

The importance of public honor for courage suggests that the discussion of justice, the social virtue par

excellence in Book V, can be a corrective to the treatment of the virtues in books III and IV. The dependence on others that was the ground of Aristotle's criticism of honor emerges in the later books as a salutary concomitant of the goods constituted by justice and friendship. Especially through the latter, Aristotle validates our need to confirm our own goodness through the recognition of another self or selves. In a sense, Aristotle's differing approaches to the issues raised by honor—as it relates to the noble, justice, and friendship—mark the stages of development in the *Ethics*. The centrality of honor undermines the noble illusion of self-sufficiency and leads to a recognition that justice involves others; finally, friendship more fully displays the salutary potential of honor by allowing another to confirm our own goodness in a more refined and particular sense. The centrality of honor in Aristotle's political science presents a human good that both reflects and transcends political life, narrowly understood. Although honor is not unproblematic and can never overcome the tensions in political life, there is a flexibility in this fundamental political phenomenon that allows its basic principles to be adapted to the more elevated considerations of justice and friendship that mark Aristotle's political science. Honor acts as a crucial signpost pointing beyond itself to a more complete good.

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[29] For an explanation of my departure from Rackham here, see note 4.

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Rethinking Moral Economy
THOMAS CLAY ARNOLD *Emporia State University*

I establish three closely related claims. The first two are interpretive, the third theoretical. (1) The prevailing conception of moral economy in political science, presupposed by opponents as well as advocates, rests too heavily on the distinction between nonmarket and market-based societies. (2) The prevailing conception of moral economy reduces to the unduly narrow claim that economic incorporation of a nonmarket people is the basis for the moral indignation that leads to resistance and rebellion. (3) Reconceptualizing moral economy in terms of social goods reveals additional grounds for politically significant moral indignation and permits moral-economic political analysis of a larger set of cases and phenomena. Water politics in the arid American West illustrate the power of a conception of moral economy based on social goods.

Political scientists debate as intensely as ever the idea of a moral economy, which contains both descriptive and prescriptive elements. The descriptive element reflects the contributions of, among others, anthropologists and refers to the various, essentially noneconomic norms and obligations (e.g., reciprocity) that mediate the central social, political, and/or economic relations of a given (almost always pre- or nonmarket) people. The prescriptive element refers to moral economy's status and value as an instrument for social and political analysis. Whether used to evaluate the distinctive social and political features of various systems of exchange (Booth 1993a; Dalton 1961; Polanyi 1957) or to explain insurgency and rebellion (Gosner 1992; Scott 1976; Thompson 1971), the idea of a moral economy is a claim about the conduct of inquiry, of what to study and how. For many political scientists committed to alternative forms of inquiry, moral economy easily means above all else spirited discussion about "the most crucial questions of method and interpretation" (Booth 1993b, 953).

Parties to this debate in political science differ sharply over the value of what Frerejohn (1991, 301, n. 1) has fruitfully described as "the ethnographic starting point," the assertion, embraced by moral economists, that "the proper place to begin social analysis is with the meanings embedded in the social practices in question," that social practices "form the basis for the identification of actors and choices." Described more fully below, the debate over moral-economic political science reflects in part moral economists' opposition to the assumptions and approaches of rational-choice forms of inquiry, that is to say, economic assumptions about human behavior applied in settings in which, moral economists argue, narrowly self-interested calculations of gain or efficiency are absent or secondary.

Curiously, the spirited debate in political science has not featured the concept itself. In stark contrast with the disputes over the value of moral-economic political science, the question of what constitutes a moral

economy draws little attention or controversy. Yet, moral-economic political science is only as effective as the concept of moral economy on which it rests. Silence on this central issue begs two questions. What is the underlying conception of moral economy presupposed by the parties to the dispute? More important, is it adequate? The next two sections reconstruct and critique the prevailing but largely unquestioned conception of moral economy in political science. Reconstruction and critique establish two points: The traditional conception of moral economy in political science (1) rests too heavily on the distinction between nonmarket and market-based societies and (2) reduces to the unduly narrow claim that economic incorporation of a nonmarket people is the basis for the moral indignation that leads to resistance and rebellion. The remaining two sections develop a more effective conception. Combining overlooked aspects of early moral-economic analysis with insights drawn from recent studies of community and collective action, I recast the concept in light of the constitutive, communal, and, especially, nested properties of social goods.

A conception of moral economy based on social goods captures the grounds for politically significant moral indignation more precisely than does the prevailing view. It demonstrates that the grounds for politically significant moral indignation do not lie only or even predominantly at the level of clashing economies or cultures. They lie instead at the level of specific social goods, at the intersection of nested sets of meaning and value called into question by equally specific changes in circumstance.

My revision also refines analysis. Given the nested properties of social goods, instances of resistance to what traditional moral economists often describe as commodification need not be viewed as resistance to all forms or degrees of commodity exchange, even of the good in question. Moreover, by focusing on specific social goods, rather than on overarching economic systems, I account for politically significant (although not necessarily rebellious) moral economies where traditional moral economists would least expect to find them: within modern, market-structured communities and societies. Water politics in the arid American West illustrate the powers of a conception of moral economy based on social goods.

Thomas Clay Arnold is Associate Professor of Political Science, Emporia State University, Emporia, KS 66801-5087.

RECONSTRUCTION

The traditional notion of moral economy in political science reflects the seminal works of Karl Polanyi (1957) and E. P. Thompson (1971). It features the extraordinary phenomenon of a poor, premarket people contesting the dictates of a much more modern economic order.[1] Polanyi contributes the important distinction between embedded and autonomous economies. Embedded, typically ancient or primitive economies submerge production and exchange to the purposes and practices of far more significant social, political, or religious institutions. Autonomous economies function differently. Production and exchange are much more significant, increasingly serve economic ends, and operate according to the constraints of pervasive, impersonal markets. Autonomous economies define the modern era.

Thompson contributes several things. Beyond popularizing the term *moral economy*, he explains how and why the transition from an embedded economy to an autonomous market generated social and political unrest, even sustained organized violence. Drawing on his study of eighteenth-century English food riots, Thompson (1971, 78, 79) finds the "delicate tissue" of traditional social norms and reciprocities unable to accommodate the "cash-nexus" of the emerging market order. The clash was often traumatic. Those most at the mercy of the emerging order regarded the transition as unjust. Moral outrage and direct action followed. Thompson's most significant contribution is his understated concept of a moral economy, which political scientists do not incorporate as fully as they should. Generalizing from the particulars of the food riots, Thompson conceives moral economy as a popular consensus about what distinguishes legitimate from illegitimate practices, a consensus rooted in the past and capable of inspiring action (pp. 78–9, 108, 112, 131–6).

Reflecting the discipline's longstanding desire to account for peasant and Third World insurrections (Bates and Curry 1992; Lichbach 1994), the prevailing concept in political science emphasizes conflict and resistance. This emphasis speaks far more to a pattern of behavior rather than, as Thompson instructs, the nature of and sources for communal notions of legitimacy. Illustrations abound. Consider Scott's (1976) analysis of peasant rebellions in early-twentieth-century Burma and Vietnam. Peasants rebelled, he concludes, out of their desire to resist intensifying European colonialism and to restore ancient peasant ways and rights. Eric Wolf (1969, 276) explains peasant wars in Cuba, Russia, China, Mexico, and Algeria in similar

terms, emphasizing the predatory nature and culturally disruptive effects of "North Atlantic Capitalism."

This pattern of behavior has even been used to reinterpret aspects of U.S. social and political history, in particular the actions of nineteenth-century premarket farmers in large portions of Georgia, Missouri, and Texas. Convinced that their communities and attendant "habits of mutuality" were threatened by a growing and impersonal market, yeoman farmers resorted to, among other things, populist revolt and social banditry (Hahn 1983, 53; see also McMath 1985; Thelen 1986). Resistance to economic incorporation also emerges in Strickland's (1985) study of newly freed blacks in the rice-planting sections of the South Carolina Low Country. In this instance, Low Country blacks jealously retained the independence and unsupervised time long afforded slaves by the traditional manner of rice cultivation (task work largely "out of the master's eye") despite postwar efforts by enterprising whites to regulate labor rationally on the basis of time, supervision, and wages.[2] Blacks resisted the emerging order simply by refusing to participate. Their subsistence skills and, unlike other sections of the South, the refuge of their well-established, extended families sustained the resistance. Judging from the literature, resistance to economic incorporation is indeed the preferred, and typically the only, recognized pattern.

The prevailing conception of moral economy is also a response to a prominent issue in contemporary political thought, "the controversy over 'the economic approach to human behavior,' over rational choice and related theories, *and especially over the understanding of nonmarket societies*" (Booth 1994, 653, emphasis added). This dispute dominates the literature, rendering *moral economy* more a label for the debate than a category for organizing experience. Although the arguments for and especially against moral-economic lines of inquiry do not debate the concept per se, they are revealing. Summarized below, they confirm the narrowness of the prevailing view and point, if only indirectly, to a compelling alternative.

Traditional moral economists maintain that, insofar as the economic approach privileges the self-interested individual, who narrowly but rationally calculates on the basis of unforgiving competition and the principle of utility maximization, it cannot fruitfully engage the many instances in which economic behavior is embedded in noneconomic institutions and values. Traditional moral economists argue that scholars gain little, if anything, by analyzing behavior in terms of what the actors themselves would recognize as strange or immoral, if even comprehensible. Scott (1976, 166) concludes that a strictly economic approach overlooks the more telling sociocultural components of behavior; it

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[1] This notion is found throughout the literature, among early advocates as well as recent critics. Scott (1976) is the leading example of early moral-economic analysis in political science. More recent studies include Posusney 1993 and Heilke 1997. Among contemporary critics are Bates and Curry (1992) and Booth (1994), whose works are second-order analyses of the topic, although both rely in part on the authors' earlier studies of premarket societies, African and ancient Greek in particular.

[2] "The real social and cultural significance of the task system lay in the fact that a slave could finish the day's work at his or her own chosen rate.... Slaves who worked quickly often assisted others who were slower, thereby making task labor a collective experience.... Task work thus helped to fashion a moral economy that prized the virtues of independence, self-determination, and personal achievement, while encouraging collective responsibility for the completion of assignments" (Strickland 1985, 145).

misses "the central fact that the peasant is born into a society and culture that provide him a fund of moral values, a set of concrete social relationships, a pattern of expectations about the behavior of others, and a sense of how those in his culture have proceeded to similar goals in the past."

In the traditional view, the more appropriate emphasis is on cultural forms and values, on what Hahn (1983, 6, 85) describes as the "web of social life," meaning "ideas about justice, independence, obligation, and other aspects of social and political life, rooted in specific relationships and refracted through historical experiences." Scott (1976, 167) agrees: "Woven into the tissue of peasant behavior, then, whether in normal local routines or in the violence of an uprising, is the structure of a shared moral universe, a common notion of what is just. It is this moral heritage that, in peasant revolts, selects certain targets rather than others, certain forms rather than others, and that makes possible ... action born of moral outrage." The abstract, atomistic, and narrowly utilitarian assumptions of economic analysis simply will not do. Instead, analysis must be phenomenological, capable of comprehending the cultural self-understandings of the actors themselves, that is, their sense of propriety, justice, obligation, and the like (Scott 1976, 3, 4).[3]

Responses to these arguments vary but fall into one of three categories. The most common is the claim that the norms and reciprocities of any given moral economy are reducible to and best explained in terms of self-interested, political economy. Popkin (1979, 18), for instance, in a study of Vietnamese peasants that overlaps Scott's, argues that "by applying theories of individual decision-making to villages, we can begin to develop a deductive understanding of peasant institutions and move the analysis back one step to the level of the individual. By using the concepts of individual choice and decision-making, we can discuss how and why groups of individuals decide to adopt some sets of norms while rejecting others." Peering beneath the veneer of collective behavior and a shared moral universe, Popkin finds a familiar "unifying investment logic" (p. 244) to Vietnamese peasant life, a logic less materialistic or income-oriented than that in the West, but rational and maximizing nonetheless.

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[3] Compare this kind of criticism of rational-choice modes of analysis with those of Sen (1977) and Monroe (1996), two scholars who work outside the tradition of moral economy. Sen recognizes that calculations of (even personal) welfare are much more complex than the undiluted model of self-interest indicates. They involve multiple rankings of different kinds of preferences, some of which are based on a sense of duty, as affected by morals and culture and as mediated by "commitment" to religion, class, group, or community (1977, 326–9).

Based upon her study of altruism, Monroe (1996) rejects individualistic self-interest as "an all-embracing theory of human behavior" because it "distorts and limits our understanding of what it means to be a human being" (p. 236). Altruists in particular act well outside the context of calculated costs and benefits; their often personally risky acts of selflessness are instead spontaneous and rooted in their deeply felt connection to all other human beings. Monroe prefers an approach she describes as "perspectival" (p. 15), in which nonutilitarian world views, core values, and a sense of self figure at least as prominently as self-interest.

A second and related response is that moral economists have a "too-pinched conception of the economic" (Booth 1994, 659). In short, they fail to appreciate its influence in social formations other than that of the market. Studies of the firm, family, and government demonstrate, critics argue, how obviously nonmarket features and institutions are clearly the product of primary economic forces and calculations. Summarizing the conclusions of Douglass North, Robert Bates, Michael Hechter, Edward LeClair, and Neil Smelser, Booth writes that nonmarket economies are best "understood in the light of the *transaction costs* associated with markets" (p. 658, emphasis added). The logic of this response is clear—converting nonmarket institutions into full-blown market ones simply incurs too many information, measurement, and enforcement costs. Retaining nonmarket features is in these instances cost efficient. The point, however, is that nonmarket does not necessarily mean noneconomic. For Booth, peasant moral economies in particular "lend themselves to retranslation into the vocabulary of the economic approach," whereupon "it becomes apparent that many of their behaviors and institutional forms are economizing responses to a situation of severe risk in regard to subsistence goods" (p. 659). The work on transaction costs by North and others questions the assumption of many traditional moral economists that modernity is distinguished by the role and place of economic calculation.

The third and even more substantive response addresses directly the "master assumption" (Booth 1994, 653) of moral economists, the concept of embeddedness. According to Granovetter (1985), embeddedness is in the end a matter of the degree to which economic behavior is affected by or submerged in social relations. Traditional moral economists err in two ways. First, moral economists so deeply embed economic behavior in what they think are social relations that "economic" behavior is not literally economic (oversocialization). Second, what moral economists present as economic behavior is actually an attribute of a "generalized morality," which Granovetter defines as a widely shared set of "implicit agreements to certain kinds of regard for others" (p. 489, citing Arrow 1974, 26). Generalized moralities, however, lead to other difficulties.

According to Granovetter (1985), generalized moralities, whatever their content, are not social relations. Truly social relations involve relatively continuous, concrete, and specific interpersonal ties; actors, fully aware of these ties and circumstances, respond accordingly. Truly social relations are ongoing, "continuously constructed and reconstructed during interaction" (p. 486). In contrast, generalized moralities are external, fairly fixed systems of norms and values internalized through socialization. Because they are internalized, their behavior-inducing clout is "contained," so to speak, "inside an individual's head" (p. 486). Therefore, behavior is "automatic," even "mechanical," and ongoing social relations are "irrelevant" (p. 486). Moral economists who embed behavior in generalized moralities paradoxically retain that to which they ob-

ject in the strictly economic approach: "atomized decision making even when decisions are seen to involve more than one individual" (pp. 486–7).

Booth adds to the critique of embeddedness. He rejects the use of the concept by moral economists to distinguish sharply the character and manner of submerged, premodern economies from modern, autonomous ones, a distinction that many of them take somewhat for granted. Contrary to the claims of Polanyi in particular, he believes close study of ancient Greece proves that narrowly rational, economic behavior is never so embedded that it is "theoretically indistinct," not to mention "invisible" (Booth 1993a, 6; 1994, 654, 655). Furthermore, study of modern market society proves that narrowly rational, economic behavior is never so autonomous that the very concept of economy must be regarded as "a product of modern society" and not truly applicable "to the theoretical/ moral universe of non- or precapitalist worlds" (Booth 1993a, 6). In the end, the notion of embeddedness blinds moral economists to the analytically important fact that all economies (ancient and modern) are simultaneously economizing and normative in nature, although in varying degrees.[4]

CRITIQUE

According to its severest critics, moral-economic political analysis is flawed. It misreads the nature of human behavior and, therefore, the politics that flow from it. It may stimulate reflection but is not a particularly powerful or credible line of inquiry. Rejection, however, is premature. Rejection presumes that the traditional notion of moral economy is conclusive. It is not. Interestingly, Booth's study of ancient Greece is itself cause for rethinking the concept, although for reasons other than those Booth emphasizes.[5]

According to Booth, the moral architecture and economy of ancient Greece reflected fundamental ends

and values of the household, the primary economic unit. As epitomized by Ischomachos in Xenophon's *Oeconomicus*, the household rested on careful, even rational calculations of utility. More precisely, its economy turned on "the managerial role of the wife [in relation to servants and children] ... the need for overseers [in relation to slaves], and ... the issue of how to motivate the wife and overseer to do the master's work efficiently" (Booth 1994, 659). The value of efficiency, however, lay not in maximizing the master's wealth but his leisure, a prerequisite for meaningful participation in political affairs and for living a good and beautiful life.

The example of ancient Greece, used by Booth to refute the "radical exceptionalism" (1994, 662) of market society, yields other insights. In short, that society does not conform to what contemporary political science would have us believe constitutes a moral economy. It was unquestionably a normatively embedded economy and society, which Booth describes as anticommercial, as dedicated to a life of "detachment from provisioning activity" (p. 660), but it was not a marginal society of subsistence-oriented peasants rebelling against the callous forces of economic incorporation. The Greek example challenges political science's preference for conceiving moral economy in terms of premarket communities and cultures clashing with far more commercial ones. Equally telling challenges are not limited to cases drawn from the precapitalist world, as two recent studies demonstrate.

Somerset County, Maryland

Community, culture, and economic development are the subjects of Meredith Ramsay's (1996) research on Princess Anne and Crisfield, Maryland, two Somerset County communities recently gripped by economic crisis. Both were in trouble because longstanding modes of production (agriculture in Princess Anne, commercial fishing and seafood packing in Crisfield) sharply and irrevocably declined between 1986 and 1991, making Maryland's poorest county only poorer. Calls for economic revitalization were frequent and popular, but residents routinely rejected commercial real estate projects. In fact, elite-sponsored real estate initiatives in Princess Anne spawned an unprecedented electoral uprising. Voters swept out of office a number of progrowth establishment officials and even elected their first black representative. Ramsay attributes these and other outcomes to "a nearly universal commitment to a shared way of life" (1996, 8), despite racial, class, and other divisions, a way of life in which "community mattered" (p. 9).

Two aspects of Ramsay's study stand out. First, her study is firmly grounded in the moral-economic axiom that "all economies are enmeshed in the political, social, and moral life of particular places" (1996, 9).[6] Echoing Hahn, Ramsay emphasizes "way-of-life vari-

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[4] According to Booth, market society is itself normatively embedded. In particular, labor and exchange now reflect the freedom, equality, and moral pluralism of modernity rather than, as in the past, the hierarchy and domination of the preliberal household. The transition to market society was not, as Polanyi claims, a fateful disembedding of economy and society but an historic "moral redrawing of the community and of the place of the economy within it" (Booth 1994, 661). This perspective distinguishes Booth as a critic of moral-economic political science. Although sharply critical of one (from his point of view, fundamental) feature of it (analysis predicated in part on the embeddedness distinction), Booth supports another by analytically linking the economy of a given time and society to the normative context within which that economy is situated. In Booth's case, however, the linkage is general rather than specific, at the level of economic systems writ large. As such, his quasimoral-economic argument cannot address the issue at the center of my social goods version: how and why moral economies (some even contrary to the spirit of the modern market society) emerge within commercialized market societies.

[5] Booth's effective critique of a sharp, Polanyian distinction between submerged and autonomous economies does not directly question traditional moral economists' related claim that economic incorporation of a pre- or nonmarket people often leads to collective action rooted in moral outrage. What needs reappraisal is whether resistance to economic incorporation is today the only or even most important foundation for a moral economy.

[6] Ramsay (1996, 135, n. 18) acknowledges Scott's influence, characterizing her own work on Maryland as an "independent collaboration."

ables" (p. 107). A more finely tuned grasp of local policy is achieved, she writes, by paying "careful attention to the . . . social structures and cultural values that give security, predictability, character, and meaning to the life of a given community" (p. 22). In Somerset County social structures and relations were such that residents were reluctant to abandon or degrade them in the name of economic development.

Second, Ramsay's study is not of a pre- or nonmarket people. Local agriculture and fishing conformed to and operated within the larger market economy. Given modern modes of production, the politically significant moral economies of Princess Anne and Crisfield differ in origin, form, and to some degree content from those associated with the Asian, African, or South American corporate villages emphasized in the literature.[7]

Owens Valley, California

John Walton's (1992) study of the Owens Valley in California compliments Ramsay's examination of the social roots of local action, although in his research rebellion returns as the critical event. The tale is remarkable. At the turn of the century this rural valley just east of the Sierra Nevada range confronted the City of Los Angeles over access to valley water. In 1924, at least twenty years into the confrontation, the dispute turned violent when residents seized and bombed the city's water-related holdings and structures within the valley. Led by two bankers, the bombings were numerous, strategic, and popular. Local support frustrated efforts by Los Angeles to prosecute the perpetrators. One investigator for the city was convinced "no local grand jury would indict, and no court convict" the forty to sixty people he believed directly involved (Walton 1992, 169). Los Angeles got the water it wanted, but dynamiting, arson, and other acts of rebellion continued periodically into the 1970s, ending only when new environmental laws allowed valley residents to wage their resistance through the courts. Walton's explanation of this extraordinary affair combines elements from a number of schools of thought. At the center of his explanation, however, lies an account of moral economy different from and superior to that usually found in the discipline.

Walton takes to heart Thompson's statements about a popular consensus rooted in the past and capable of inspiring action. He agrees with Scott, Hahn, and others that operative conceptions of justice and injustice emerge from local history, from the concrete meanings, traditions, and mechanisms of daily life. The history of Owens Valley, from initial white settlement

at least up to the moment of rebellion, was one of persistent difficulty, sacrifice, and isolation. Hardship was the unrelenting order of the day, and collective endeavor—in the form of an elaborate, if not always tolerant, string of voluntary organizations (civil society)—was the only abundant social resource. These frontier people were willing to endure hardship and sacrifice in the hope that prosperity would someday be theirs, as their pioneering heritage told them it would. This valley of expectant capitalists saw the opportunities and markets associated with mines, railroads, and reclamation as their commercial salvation. Unlike the communities in Burma and Vietnam studied by Scott, security lay in perfecting, not rejecting, the spirit of enterprise.

According to Walton (pp. 90–103), the moral economy of Owens Valley reflected its unique situation and history. It combined a vibrant but as yet unrealized commitment to commercial enterprise with a propensity for civic organization and collective endeavor. The combination proved explosive when hopes for prosperity were repeatedly raised and dashed. Mining companies and railroads flirted with the valley but never established ongoing economic ventures, leaving it as destitute as ever. Reclamation was the last and best hope and proved the greatest disappointment. Chosen by the newly formed Reclamation Service as an ideal site for fulfilling the promise of the 1902 Reclamation Act, which was designed to promote western settlement, small-scale family farming, and democracy through irrigation of arid lands (no more than 320 acres of irrigated land per couple), the valley finally seemed poised for transformation. Abundant irrigation water, residents noted, would lead to an agricultural bonanza, population growth, a cash economy, and economic security.

Moral outrage was swift and pronounced when the residents learned that Los Angeles had cheated them (through fraud in some cases, intimidation in others) by obtaining title to key parcels and all-important water rights (pp. 154–6). Even worse, the city had used its influence with state and federal officials to convince President Theodore Roosevelt, ironically a prominent advocate for the Reclamation Act and its ideals, to support a change in the project: Rather than use the water for farming in the Owens Valley, export it to Los Angeles to subsidize the city's already phenomenal growth. Certain they were the victims of a massive injustice (the "Rape of the Owens Valley"), residents drew on their civic traditions and rebelled (pp. xviii, 168–82, 292, 312).

The studies by Ramsay and Walton support the argument that the prevailing conception of moral economy in political science is unduly narrow. Given its emphasis on the distinction between premarket and market societies, the prevailing conception of moral economy in political science cannot explain the non-rebellious moral economies of market-structured communities in Maryland, which had achieved a degree of economic development but were reluctant to develop further, even in the face of economic decline. Given its emphasis on resistance to commercial incorporation,

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[7] The power of the prevailing conception of moral economy in political science is in part terminological, as is evident in Ramsay's study. Drawing on Scott's (1976) language, Ramsay refers to Princess Anne and Crisfield as subsistence communities, an awkward choice at best. Scott's communities are precapitalist and peasant; Ramsay's are not, even though they have a risk-minimizing, communal ethic that is "based upon trust . . . and mutual obligation" and is dedicated to preserving "a particular moral order" (Ramsey 1996, 120). Moral economy must be conceived in such a way as to account more accurately for the broadly similar norms and reciprocities that emanate from sharply different social and economic circumstances.

the prevailing conception of moral economy cannot explain the moral indignation and rebellion of a non-market valley in California desperate for commercial incorporation. These and related cases require an altogether different approach to the issue of a moral economy. Thompson's notion of communal legitimacy offers a promising alternative.

As employed by Thompson, legitimacy is recognition by a community that a given state of affairs conforms to known and accepted rules and principles. Well-known and long accepted rules and principles are the ground, Thompson argues, for consensus and collective action. Moral economists loosely share this view but too often couch legitimacy in the diffuse language of shared universes or traditional norms and obligations, restating in effect Max Weber's famous but generic reference to the legitimacy of the "eternal yesterday" (Gerth and Mills 1958, 78). What is even more problematic, legitimacy is complex; no one set of legitimizing rules or principles prevails in or across all cases. Depending on circumstances, market rules and principles may well give way to political, religious, or still other principles. Consequently, phrases such as "shared moral universe" (Scott 1976, 167) and "shared way of life" (Ramsay 1996, 8) too often suppress rather than reveal the various sources of communal legitimacy available at any one time; they inhibit a clearer understanding of why communities choose one set of legitimizing rules and principles over another.[8]

Basing moral-economic political analysis on social goods is a much more effective alternative. Given their nature and properties, described below, social goods harbor the principles communities canvass when judging whether specific developments are legitimate.

SOCIAL GOODS

Goods are objects and qualities whose possession or consumption confers some kind of benefit and satisfies human needs and wants. For many, goods are unquestionably material and commercial in nature. Everyday illustrations include foreign and domestic goods, durable and nondurable goods, basic and luxury goods. Contemporary philosophers agree that goods satisfy human needs and wants but acknowledge and often emphasize their less material and noncommercial forms. Examples from contemporary work include primary and secondary goods (Rawls 1971), goods of excellence and goods of effectiveness (MacIntyre 1988), additive and nonsubtractive generic goods (Gewirth 1978, 1982), and mutual and convergent goods (Taylor 1989). These deliberately abstract categories feature such goods as opportunities, powers, rights, security, community, and well-being, that is, goods "for which it is the task of a theory of justice to provide distributive principles" (Swift 1995, 224).

The concept of social goods developed below clearly falls into the latter category and draws crucially on Walzer (1983, 6–10). The following version, however, differs in certain respects. Whereas Walzer uses social goods as his foundation for developing a theory of justice that champions pluralism and complex equality, my concept addresses the narrower issue of collective action that emanates from communal perceptions of legitimacy. This reorientation brings into even sharper relief the central role of social goods on the identities, obligations, and relationships of both persons and a people. In other words, a collective action orientation features the goods-based senses of self and community Walzer's general theory of justice clearly incorporates but need not always emphasize in the same way (see below). By analyzing social goods as grounds for collective action I extend Walzer's concept, revealing in turn both the nested nature of social goods and the basis for my theory of moral economy.

In Walzer's justice-based application as well as my own, the notion of social goods is first and foremost a statement about the universal nature of all human goods. Even goods considered pure commodities are social, for they consist of shared understandings about the beneficial characteristics attributed to a given object. Any identification of an object as a good unavoidably draws on culturally constructed and culturally transmitted ideas about human needs, wants, and benefits. "Goods don't just appear in the hands of distributive agents who do with them as they like"; goods first "come into people's minds" (Walzer 1983, 6–7). Like the meaning of words, the value and meaningfulness of goods are socially determined. In Walzer's theory of justice, the principles of shared understandings and socially determined value are central: "All distributions are just or unjust relative to the social meanings of the goods at stake" (p. 9). More precisely, when we understand the social meaning of a given good, "we understand how, by whom, and for what reasons it ought to be distributed" (p. 9).[9] Although central, these principles do not exhaust Walzer's theory. Significantly, his account also includes constitutive and communal senses of social goods, and when these are analyzed explicitly from the perspective of collective action, they speak directly to the issue of sources for communal perceptions of legitimacy.

Constitutive social goods establish and symbolize important senses of self. They reflect a manner of individual and collective identification that is characteristic of human beings (Appadurai 1986; Douglas and Isherwood 1979). Humans acquire "concrete identities" through the ways in which they "conceive and create, and then possess and employ social goods"

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[8] Walton makes a similar point: "The fact that repertoires [of politically significant collective action] are associated with culture does not tell us *where standards of justice come from*, how they affect the state and the economy, or whether culture is a causal force or simply a setting" (1992, 325, emphasis added).

[9] No one principle of distribution holds in all cases. Some goods are justly distributed according to principles of merit, others according to principles of exchange or need, and so on. As a general rule, however, *"no social good x should be distributed to men and women who possess some other good y merely because they possess y and without regard to the meaning of x"* (Walzer 1983, 20, emphasis in original). In effect, each social good is its own sphere of justice. My concept of moral economy is consistent with and supportive of Walzer's general theory of justice.

(Walzer 1983, 8). People inevitably take stock of themselves and interpret the identities of others largely in terms of social goods. Moreover, patterned use, display, and exchange of social goods place people in specific social, economic, and even political relations. Among the Nuer of Africa, for instance, cattle connect people to one another in vital ways, not just as herdsman, buyer, or seller. Cattle structure the status, capacities, and obligations of individuals. Owned by families, cattle reflect and reproduce the all-important "network of kinship ties" (Evans-Pritchard 1940, 17). Nuer allocate cattle accordingly, and their movement from one *kraal* to another is "equivalent to lines in a genealogical chart" (p. 18). Couples finalize the union of marriage through ritualistic slaughter of cattle, and their legal status as a married couple is in part "defined by cattle rights and obligations" (p. 17). Cattle are not simply beasts of burden or sources for food, psychologically and sociologically they are an integral part of what it means to be a Nuer. They are social goods in ways compatible with but deeper than that conveyed by the notion of shared understandings alone; cattle are the foundation for important processes, roles, identities, and relationships without which it would be very difficult to speak of Nuer society and culture.

The value of these identity-orienting, role-shaping goods extends beyond their purely material or commercial properties. Insofar as constitutive social goods structure the status and obligations of persons, their value includes the meaningfulness of the relationships and the senses of self generated. Under certain circumstances, this identity-related value overrides their narrower commodity value. Choices in these instances may well reflect rationales other than the strictly economic. Equally important, at least from the perspective of a theory of moral economy, deeply valued identities and relationships are latent but easily triggered sources for assessing the legitimacy of both proposed and emerging developments.

Consider rice in Japan. It is, of course, a food, yet it is much more. According to Ohnuki-Tierney (1993, 102), rice is the critical metaphor by and through which Japanese "think about themselves in relation to other peoples." These reflections are in turn an equally critical part of their concept of "the collective self" (p. 102). It was in terms of short-grain rice (agri)culture that the Japanese recognized themselves as a people significantly different from the meat and wheat oriented West and, more important, the long-grain rice cultures of Asia, in particular China. Although Japan is no longer predominantly agricultural, a rice-based sense of self persists. Rice and rice paddies remain the defining symbols of Japan and its ancestral land. Compared to bamboo, sashimi, or many other Japanese foods and symbols, rice is sacred. Its harvest and consumption often are a matter of ritual and collective celebration, its whiteness is a symbol of Japanese purity and virtue. This significance reaches and affects the conduct of everyday life. Rice is the most common offering in the ancestral alcoves found in many Japanese homes, a gesture that preserves a sense of family continuity as well as conveys respect. The role of rice in reproducing and maintaining a sense of family extends to the meal itself:

> Whereas dishes are served on individual plates ahead of time in urban Japanese households today, rice alone remains the common food to be distributed by the woman of the house. Commensality within the household is symbolized by the act of distributing rice from a single container.... The collective self of the family—the most basic unit of Japanese society—is constructed through sharing rice during daily meals (p. 95).

Rice is a truly social good, and its longstanding and ongoing role in establishing a Japanese sense of collective self is at times a prominent factor in evaluating emerging developments. In the late 1980s, for example, when officials announced plans to import California rice (it was far more plentiful and much less expensive than the declining supplies of Japanese rice), the social value of rice prevailed over its value as a staple.[10] Although the Japanese import and consume other American food products, they vigorously opposed and ultimately defeated the proposal to import rice. Urban Japanese, women in particular, made the difference. Their opposition was broad and decisive but contrary to their interests as consumers. Even though California rice had been sown from short-grain stocks imported from Japan, the Japanese found it impure, "foreign," a corruption of the "last sacred realm," and a threat to Japanese identity and autonomy (Ohnuki-Tierney 1993, 111). In this instance, the seemingly irrational opposition to importation reflected a commitment to a deeper, more important value, unrelated to price.[11]

In addition to their influence on identity and status, select social goods are intrinsically communal in nature. These goods produce (or at least reproduce) the sensibilities and "special commitment to one another" (Walzer 1983, 62) that make associated people a community. Although relative to each community, intrinsically communal social goods are the foundation for truly common actions. Unlike other goods, they are not simply "convergent" in nature, a matter for me and (then) for you. Intrinsically communal social goods are mutual and indivisible, a matter for us (Taylor 1989, 166–9). The goods of security, welfare, and membership illustrate; in each case participation in a common life is, as Walzer (1983, 65) puts it, "simultaneously the prerequisite of provision and one of its products." Because these goods are mutual and indivisible, they link people as citizens, and as citizens people recognize joint responsibilities and pursue what are now unmis-

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[10] Walzer distinguishes between the not always compatible moral and physical "necessities" of goods. Which set of necessities prevails depends on circumstances. Bread, Walzer (1983, 8) notes, is not just a food item; it is "the body of Christ, the symbol of the Sabbath, the means of hospitality, and so on." For the devout who view bread as symbolic of important religious values and relationships, it may well be something that, depending on circumstances, people should bake and destroy rather than sell or eat.

[11] Reminiscent of Monroe, Ohnuki-Tierney (1993, 9) finds the economic assumptions of rational-choice theory "inappropriate." Rice, she concludes, "is not simply food to fill the stomach. Japanese attitudes and behaviors toward rice are not governed by economic rationale" (p. 29). Instead, they reflect the complex social psychology of rice as self.

takably collective affairs. If, as Walzer argues, membership in human community is "the most important good" (p. 29) we distribute to one another, then intrinsically communal social goods are an additional and perhaps even more potent basis on which people judge legitimacy.

Social goods are not as singular or discrete as analysis must sometimes portray them. As with rice in Japan, social goods can and do harbor more than one kind of meaning and value, particularly if the good in question takes a commodity form. Like the nested pots in a backpacker's kit, multiple sets of meaning and value serve different purposes but rest within the confines of one another. Tobacco, for instance, has long been grown for its commercial value, and fortunes have been made from its cultivation. In 1849, Missouri planters produced more than 17 million pounds of it, surpassing even North Carolina. Planters sold their tobacco in the markets of St. Louis and New Orleans, but they did not regard it simply as a cash crop. For Missouri planters, much like their Kentucky and Virginia ancestors, tobacco was a social good; the rhythms and rituals of its cultivation were an important source for "collective identity" and "harmony" (Phillips 2000, 123). More than even profit, verdant fields of tobacco were a "shared sign" of community health and membership, as central to the collective sense of self (and unrelated to market price) as rice paddies for the Japanese.[12] Defying the logic of markets and profits, Missouri planters increased production only modestly when tobacco prices were at all time highs (other states tripled production) but doubled production when their tobacco-centered senses of self and community were assailed by a decade-long ruinous collapse in prices (pp. 123, 124). Similar to the rice example, tobacco illustrates important points for moral-economic political analysis. Beyond the fact that many goods are nested complexes of value that differ in kind as well as degree, the constitutive and communal properties of social goods often set the boundaries within which their other values operate. A social good's commodity status, then (should it have one), need not clash automatically or completely with that good's less commercial roles.

A focus on social goods facilitates analysis in still other ways. Multiple but nested values and meanings account for the diverse (but not always perfectly compatible) practices surrounding given goods. Nested values and diverse practices explain in turn the basis for conflict. Significantly, the conflict related to such complex social goods as rice and tobacco is not just about more favorable allocations or greater material gain; it involves what *kind* of value should prevail in particular, often changing situations. Nested sets of meaning and value also explain how, given even only slightly changed circumstances, previously accepted practices (such as commodity-like exchange) may come to generate perceptions of illegitimacy. In Japan, rice is in many respects a commodity, appropriately subject to the competitive pressures of commercial exchange, but only to a point. Beyond that point, when rice-based and quintessentially Japanese identities and relationships are compromised, rice's social value prevails. Likewise for tobacco in nineteenth-century Missouri (and elsewhere); it was an economically valuable commodity but was even more valuable as a medium for stable and fulfilling social relations.[13]

In some communities social goods are clustered as well as nested. Clustered social goods intensify felt impressions of belonging and membership, adding texture and weight to the notion of culturally distinct but shared ways of life. Linkages among clusters are such that, for those who enjoy them, affiliated goods may well stand or fall together. Among yeoman farmers and artisans in nineteenth-century Missouri, for instance, work, family life, religion, and politics were distinct but deeply intertwined social goods; each reflected and in turn sustained an economy in which production, consumption, and pricing were governed by local, self-sufficient communities based upon personal networks and face-to-face relations. Farmers and artisans understood all too well the implications of expanding railroads; impersonal, commercial society would alter not just the local economy but life as they knew it (Thelen 1986, 9–25).

Given their nature and meaningfulness, constitutive and intrinsically communal social goods are sources for shared notions of legitimacy. Roles, identities, and communities grounded in social goods contain within them the criteria for evaluating specific developments as appropriate or inappropriate, at least for those who recognize the goods in question as social goods. These meanings and related criteria are the foundation for a conception of moral economy based on social goods. A preliminary sketch, demonstration, and justification of this concept follows.

MORAL ECONOMY REDEFINED

Conceptually, moral economy remains a popular consensus rooted in the past and capable of inspiring action. With Thompson, it reflects a claim about the nature of and sources for communal notions of legitimacy. In this version of moral economy, however, the

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[12] Tobacco culture fostered powerful and often insular conceptions of community. According to Waldrep (1993, 13–4), who did research in western Kentucky and Tennessee, "tobacco farmers developed unusually close ties with their neighbors. Even a small crop of tobacco required so much effort that [they] swapped off working on each other's farms. Women frequently joined their husbands and fathers in the fields. . . . Neighborhood women worked so closely together to clothe their families, sharing patterns and sewing techniques, that communities developed distinctive styles of dress and residents could immediately identify outsiders by their clothes. Only rarely did neighborhood girls marry outside the community." Breen (1984, 251, 261) speaks of an early American tobacco culture in which cultivation was simultaneously the basis for "social cohesion," a mechanism for forging a "public identity," and "emblematic" of the larger social order's past and future.

[13] Tobacco-based senses of self and community fueled the Black Patch War (1890–1915) in western Kentucky and Tennessee, a region famous for its dark tobacco and fierce hostility to outside interests bent on undermining local community control. See Waldrep 1993.

popular consensus about what distinguishes legitimate from illegitimate practices resides in the nested meanings of specific constitutive and/or communal social goods. These meanings harbor implicit notions about legitimate practices as well as legitimate relations in and among affected groups and individuals.[14] A significant threat to or a sustained, deliberate deviation from accepted practices and relations prompts organized response, which can range from public criticism, legal challenges, and policy initiatives to more unconventional and violent forms of protest, including rebellion. Water in the arid American West is a particularly good illustration for two reasons.

First, water has long been associated with a number of generally desirable but frequently competing values. It is valued not only as a source of sustenance but also as an instrument of agriculture, object of beauty, industrial commodity, means of transportation, community good, fuel for urban development, clean and pure resource, and place for recreation and wildlife habitat (Wilkenson 1990). The West's persistent aridity (many areas receive less than 12 inches of precipitation a year) complicates decision making, because all water-related values cannot be realized equally. Moreover, realization of any one value may well preclude that of one or more others. Consequently, the just allocation of scarce water is a constant challenge, the fertile setting for a moral economy of a complex good.

Second, water is a constitutive and intrinsically communal social good. Westerners identify themselves and relate to others in terms of water, which plays much the same role as rice in Japan or tobacco in nineteenth-century Missouri. The most important forms of relation and identification derive from the fact that long-term community and polity in the arid American West are not possible without an ongoing ability to procure and allocate water. Figuratively if not literally "at the center of human communities" (Bates et al. 1993, 14), water is embedded in the very notion of collective existence. It relates community residents as citizens, impressing upon them in particular their "mutual dependency, common enterprise, and joint responsibility" (Sax 1990, 17).[15]

Founded in part on water, western communities and their residents recognize and frequently interact with one another in terms of shared or contested rivers, watersheds, and aquifers. Water rights and practices, diversion structures, and a variety of water-oriented districts, institutions, and organizations identify people in even more particular ways: water rights holder versus nonholders, junior versus senior appropriator,

up- versus downstream community, lower versus upper basin state, and so on. The web of physical, legal, and administrative structures that capture and allocate water constitutes in effect a social and political matrix that individuals, communities, and states invariably take into account. In other words, water is a vital medium for social and political relations, a medium at the center of processes of community, even regional self-identification (Brown and Ingram 1987).

Water's status as a constitutive and intrinsically communal social good accounts for the fact that westerners conceive and cherish water in terms beyond those of mere economic utility. It is not simply a liquid with certain physical properties, a thing unto itself, a commodity whose value is realized only or essentially in economic exchange. Water symbolizes inherently desirable states of affairs achieved and experienced only in concert with others, in particular the mutual advantages of "schools, churches, and social life," of a civic and moral as well as economic progress (Mead 1903, 382; see also Hundley 1992; Pisani 1984; Sherow 1990).[16] Because of these deeply valued and clustered senses of community and self, water issues turn on far more than questions of how to allocate efficiently an increasingly scarce yet increasingly valuable resource. Also involved is the more difficult but important question of which water-related value should prevail and why in given situations. As in the examples of rice and tobacco, water-related senses of community and self, especially when threatened, establish the context for evaluation and action.

The deeply rooted and politically significant moral economy of water springs from its constitutive and intrinsically communal properties. Long valued for its role in creating and maintaining civically rewarding communities, water has a moral thrust that is fundamentally negative; in effect, water allocations in the end should not undermine achieved or expected community. Simply put, transfers and allocations that threaten or undermine the integrity of community are illegitimate. Subordinating the disposition of water to the integrity of community is legitimate because the autonomy, welfare, and identity of western communities are tied directly to water.[17]

The embedded and negative nature of water's moral economy affects how westerns invoke it. Because embedded, its longstanding meanings and understandings at times are taken for granted. Consequently, westerners invoke it sporadically, often in response to specific circumstances and with the objective of more firmly

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[14] My idea is similar to Wuthnow's (1987) concept of a moral order: a concept "essentially about culture ... an attribute of social relations" (p. 58). Moral orders "express boundaries," so that we may know "the place of things," and they include "implicit categories that define proper relations among individuals and groups" (p. 69).

[15] These intrinsically communal properties qualify water as a sphere of security and welfare, which Walzer (1983, 64) describes in part as "political community for the sake of provision, provision for the sake of community." The communal properties and effects of water in the arid West can be compared to those of education, sumptuary laws, relief, and ransom of captives in Walzer's analysis of medieval Jewish communities (pp. 71–4).

[16] Ingram and Oggins (1990, 5, 10) emphasize the noneconomic view of water. In a survey of leaders in Arizona, New Mexico, and western Texas whose communities stood to lose water through intensified water marketing (economically driven rural-to-urban transfers), the overwhelming majority characterized the loss as damaging to rural communities and not subject to compensation. According to one, when a community loses its water it "loses its present, its past, and its future."

[17] Bates et al. (1993, 182) refer to this as a principle of equity, rooted "in the shared, public nature of water." Equity dictates, they conclude, that water-related decisions involve and reflect "the whole community, not just those parts that were there first or that have money or power" (p. 178).

securing the integrity of specific communities (including states).[18] These invocations pair acts of rediscovery with calls for change. In each instance westerners negotiate water's multiple uses and meanings in light of both new circumstances and water's fundamental moral imperative.

Beyond public and often emotionally charged declarations of concern or injustice,[19] westerners invoke the moral economy of water through appeals for reform, new legislation, reinterpretation of existing laws, or more direct action, including violence (Babbitt 1988; Dunbar 1983; Pisani 1984; Sherow 1990; Walton 1992).[20] Results have been broadly and consistently regulatory. Many of these regulations, unlike those associated with other natural resources, shelter water from fully self-interested, privatized, or market-driven transactions, which qualifies water as an example of what Walzer calls a "blocked exchange."[21] The limits westerners have placed on the exchange, sale, appropriation, or transfer of water include granting it public ownership or public resource status; nonexport laws; public trust, welfare, or interest review of proposed transactions; beneficial use requirements (officially interpreted in some states as a prohibition on speculation in water for profit); and, since the Owens Valley affair, area-of-origin statutes, which include vesting irrigation and other water-related districts or associations with a veto over all out-of-watershed transfers (DuMars and Minnis 1989; Hoffman-Dooley 1996; MacDonnell 1990; MacDonnell and Howe 1986; Sax 1989; Tregarthen 1983).[22] Coupled with longstanding fears about water monopolies (Alston 1978), these limitations reflect deep concerns about water-related measures that would subject states or communities to external, arbitrary control and, consequently, compromise their identities as states and communities.[23] Rooted in the past and in water's status as a constitutive and intrinsically communal social good, the moral economy of water remains the basis for determining the legitimacy of a number of water-related activities.

A moral-economic political analysis based on social goods is justified for at least three reasons. First, this concept, unlike the prevailing view, is not limited by time or culture. Social goods and their attendant moral economies are characteristic of modern as well as premodern communities. Political science need not restrict moral-economic analysis to the clash of embedded versus autonomous economies, or, for that matter, to the phenomenon of rebellion. Second, social goods and moral economies are plural, not singular. Each moral economy is a separate although often nested sphere of action-inspiring legitimacy. Unlike the overriding economic utility of rational-choice theory or the undifferentiated webs-of-life of traditional moral economy, my revision honors the moral complexity of human communities, and it captures much more precisely the grounds for politically significant moral indignation. Third, the grounding in social goods minimizes the risk of under- or oversocialized conceptions of economic behavior. Because of the mutual, constitutive, and subjectively meaningful properties of specific social goods, moral economy is embedded in concrete, ongoing social relations, not in generalized, mechanical moralities or romanticized pasts. In sum, important kinds of political and economic activity reflect the inherently cultural properties of meaningful goods, and a moral-economic analysis based on social goods is especially well suited to explain them.

[18] This sporadic aspect supports Walzer's argument that the distributional autonomy of social goods is a critical principle, that the demand for autonomy is "more likely to make for occasional reformation and rebellion than for everyday enforcement" (Walzer 1983, 10).

[19] For example, residents across six counties in the San Luis Valley of Colorado recently responded to plans to sell the valley's groundwater to Denver and other large cities. They placed a notice in the *Denver Post*: "We, the undersigned citizens, are committed to preserving the water within Colorado's San Luis Valley for land and life. Private marketers are drawing up plans to take water from the Valley and sell to other parts of Colorado or other regions. *We are opposed to these plans*. ... We are committed to fight efforts to take water from this beautiful place, the San Luis Valley. Help us to protect land and life" (Johnston et al. 1996, 5 B, emphasis in the original).

[20] Secretary of the Interior Babbitt, a former governor of Arizona, characterized unfettered, market-driven rural-to-urban transfers of water as "economic Darwinism" and recommended protective regulations and oversight. Otherwise, he predicted, "big cities will inexorably squeeze the water and the life out of small communities" (Babbitt 1988, 7).

[21] "Blocked exchanges set limits on the dominance of wealth" (Walzer 1983, 99). Due to their shared understandings, some goods (e.g., human beings, rights, criminal justice) properly fall outside the sphere of money altogether. Other goods (medical care, education, and, I argue, water in the arid American West) are partially blocked; market-like exchanges are permitted but only to the extent that the sphere of money does not jeopardize the core, noncommodity values associated with those goods.

[22] Area-of-origin statutes limit interbasin transfers to prevent environmental, political, and/or economic losses to the areas from which the water is transferred.

[23] Westerners' historical objections to water monopolies effectively illustrate Walzer's theory of social-goods, dominance, and monopoly. Walzer (1983, 10–3) defines dominance and monopoly as the unjust conversion of one social good into another. In the end, he concludes, "dominance of goods makes for the domination of people" (p. 19), which is precisely what westerners fear about water monopolies.

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Facilitating Communication across Lines of Political Difference: The Role of Mass Media

DIANA C. MUTZ *The Ohio State University*
PAUL S. MARTIN *University of Oklahoma*

*W*e use national survey data to examine the extent to which various sources of political information expose people to dissimilar political views. We hypothesize that the individual's ability and desire to exercise selective exposure is a key factor in determining whether a given source produces exposure to dissimilar views. Although a lack of diverse perspectives is a common complaint against American news media, we find that individuals are exposed to far more dissimilar political views via news media than through interpersonal political discussants. The media advantage is rooted in the relative difficulty of selectively exposing oneself to those sources of information, as well as the lesser desire to do so, given the impersonal nature of mass media.

The extent to which people are exposed to cross-cutting political viewpoints has become of increasing concern to observers of American politics. Advocates of deliberative democracy believe such exposure is essential in order for alternatives to be contrasted effectively (Fishkin 1991). Others consider exposure to dissimilar views indispensable in forming valid opinions and in learning to appreciate the perspectives of others (Arendt 1968; Benhabib 1992). Still others point to the value of exposure to cross-cutting views for purposes of establishing political legitimacy. Exposure to cross-cutting views ensures "that no one could see the end result as arbitrary, rather than reasonable and justifiable, even if not what he or she happened to see as most justifiable" (Fearon 1998, 62, emphasis in original).

Exposure to conflicting views is deemed a central element—if not the sine qua non—of the kind of political dialogue needed to maintain a democratic citizenry (e.g., Barber 1984; Bellah et al. 1985; Habermas 1989). In contrast, political talk that centers on reinforcing a shared viewpoint does little to encourage deliberation on multiple perspectives or promote a public sphere (Calhoun 1988, 220; Schudson 1995). According to the most often cited proponent of communication across lines of difference, John Stuart Mill, "if the opinion is right, [people] are deprived of the opportunity of exchanging error for truth; if wrong, they lose what is almost as great a benefit, the clearer perception and livelier impression of truth produced by its collision with error" (Mill [1859] 1956, 21). Mill's statement points to two potential benefits of exposure to oppositional views, the opportunity to change one's mind and adopt a normatively better viewpoint, and the deeper understanding of one's own position acquired through confronting different perspectives. A third benefit is legitimation of an undesired outcome.

To the extent that people are at least exposed to rationales for views with which they disagree, even an outcome they do not like acquires greater legitimacy.

For example, the literature on political tolerance argues that education is important because it "*puts a person in touch with people whose ideas and values are different from one's own*" (Stouffer 1955, 127, emphasis in original). Likewise, differences in tolerance between men and women and between urban and rural residents have been attributed to the more parochial contacts of women and rural dwellers (Nunn, Crockett, and Williams 1978; Sullivan, Piereson, and Marcus 1982). Along similar lines, authoritarianism is negatively related to diversity of experience (Altemeyer 1996; Marcus et al. 1995), and politically diverse personal networks increase awareness of oppositional viewpoints and political tolerance (Mutz 1999).

In short, both in political theory and empirical work, there is near unanimous agreement that exposure to diverse political views is good for democracy and should be encouraged. Most social scientists concur that political attitudes and opinions are formed through social interaction, political discussion, and personal reflection, and these processes are of a higher quality when people are exposed to dissimilar perspectives. Nonetheless, there is little empirical work on the contexts in which such exposure occurs. Moreover, the recent trend toward residential balkanization based on shared lifestyles heightens concerns about communication across lines of political difference in the United States. To the extent that people live among homogeneous others in self-selected enclaves, their exposure to dissimilar views may be limited.

Some theorists propose that the future of communication across lines of political difference lies in technologies that transcend geographic space. As Calhoun (1988, 225) argues, "in modern societies, most of the information we have about people different from ourselves comes not through any direct relationships, even the casual ones formed constantly in urban streets and shops. Rather it comes through print and electronic media." Yet, much of what is known about the structure and news gathering practices of American media suggests that they are unlikely to play a very useful role. The goal of this study is to evaluate conflicting claims

regarding the media's contribution to cross-cutting political exposure.

We begin by reviewing relevant research on interpersonal communication and the mass media. We then use two national surveys to test the proposition that the media make a greater contribution than interpersonal networks to Americans' exposure to dissimilar political views. Drawing on survey data across different media environments, we examine three independent tests of whether selective exposure explains our finding of greater exposure to cross-cutting views through news media. The results suggest that the structure of Americans' information environments places an extraordinary burden on the mass media to bring diverse perspectives to public attention, a burden the news media may be increasingly ill-equipped to shoulder.

INTERPERSONAL EXPOSURE TO DISSIMILAR POLITICAL VIEWS

The verdict with respect to Americans' interpersonal information environments has become increasingly bleak in the last few decades. The kind of people with whom any given individual discusses politics is a function of two factors: the availability of discussion partners in one's immediate environment and the amount of selectivity exercised in the choice of partners (Huckfeldt and Sprague 1995a). Research suggests that both factors now operate primarily to produce greater homogeneity in interpersonal interactions. With respect to availability, residential patterns suggest increasingly spatially segregated living even within the heterogeneous populations of large cities, thus prompting many to argue that Americans are increasingly separated from those with political views different from their own (e.g., Calhoun 1988).[1] Residential balkanization does not necessarily mean that hordes of Americans are choosing to live among people who share their political views because of those views. Indeed, few Americans assign politics such a central role in their lives. De facto selectivity is far more likely (Freedman and Sears 1965), that is, people may choose a particular location because it is convenient to local co-ops, or a golf course, or the schools they want their children to attend, and they find themselves among others who based selection on similar considerations. The initial goal may not have been politically like-minded neighbors, but that is achieved to the extent that lifestyle considerations correlate with political perspectives.

Exposure to dissimilar views also is inhibited by the tendency for people to select politically like-minded discussion partners (Huckfeldt and Sprague 1995a). An

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[1] Because these trends have been documented strictly for residential contexts, they do not necessarily point to greater homogeneity of political views in people's larger social network. For example, encounters in the workplace provide far more exposure to dissimilar political views than do contacts with neighbors (see, e.g., Mutz and Mondak 1998). Nonetheless, place of residence has become more of a lifestyle choice (Katznelson and Weir 1986), and increasing residential segregation has been noted according to race (Harrison and Bennett 1995), education (Frey 1995), age (Frey 1995), and income (Levy 1995).

ongoing obstacle to the formation of a public sphere is the persistent trade-off between amount of interaction and heterogeneity of interaction. Strong ties and frequent contact tend to characterize homogeneous interactions, which do not bring new views to one's attention (Granovetter 1973). Quantitative evidence of selective exposure in interpersonal political communication is buttressed by qualitative accounts of the courage required to speak up among heterogeneous others (e.g., Mansbridge 1980; Schudson 1984), as well as the lengths to which people sometimes go in order to avoid discussing politics (Eliasoph 1998). Selectivity appears to play a significant role in the kinds of conversations people choose to have and, thus, the kinds of political networks they form. If residential choices increasingly facilitate de facto selective exposure, and if people actively dodge any political conflict that enters their lives, then the prospects for cross-cutting interpersonal interactions appear quite bleak indeed.

THE MEDIA CONTRIBUTION TO CROSS-CUTTING EXPOSURE

Few concepts have played as important a role in the history of research on mass communication as the notion that people selectively expose themselves to like-minded media content (Katz 1981). Beginning with the Erie County election study (Lazarsfeld, Berelson, and Gaudet 1944), this assumption became part of the conventional view that the media have limited effects on political attitudes (Klapper 1960). That tradition is now considered passé, but the issue of whether and to what extent people may selectively expose themselves to media content has never been fully resolved (e.g., Freedman and Sears 1965; Frey 1986; Katz 1968; Sweeney and Gruber 1984; Zillmann and Bryant 1985). Findings have been so inconsistent as to discourage much research, although studies of selective attention and bias in information processing have continued apace. Moreover, even if the many studies of selective exposure had converged on a central finding, they might not apply today, due to changes in the U.S. media environment.

Evidence of selective exposure in interpersonal relations is incontrovertible, but it is less clear with respect to the news media. Laboratory experiments that give people a choice of exposure to pro- or counterattitudinal media messages have yielded mixed results (see Frey 1986 for a review). Furthermore, such studies are very limited in what they can reveal about life outside the laboratory, where people do not always have a choice, are not always forewarned about the political content of a message before exposure, and tend to use a particular medium habitually rather than on a story-by-story basis. Particularly in light of residential balkanization, this question is worth reopening. Although the media are often criticized for presenting a very biased (e.g., Schiller 1986) or at least very narrow range of opinions and arguments on public issues (e.g., Hallin 1986), it is doubtful that interpersonal communication

environments are any less parochial or provide greater diversity.

The idea that the media may serve as an extension of a geographically defined social context has been suggested before, but typically the emphasis is on how mass media exposure differs from face-to-face conversation (e.g., Sennett [1977] 1992). An obvious difference is the extent to which they allow interactivity. Nonetheless, if we allow that despite their noninteractive nature media may have the capacity to accomplish some of what Mill and others consider beneficial about cross-cutting communication, then exposure to dissimilar views via the media deserves attention. It is worth noting that all three of the primary benefits of exposure that Mill outlined—persuasion to a normatively better view, deeper understanding of one's own views, and the enhanced legitimacy of political decisions—are possible without face-to-face interaction. We do not mean to suggest that interactivity is of no value; rather, even without it, the media may make a significant contribution. It is possible to separate the broader issue of whether interpersonal or mass communication does more to advance the causes of democracy from the question of which kinds of channels best serve the need for exposure to cross-cutting political perspectives. We focus on this admittedly limited but nonetheless essential component of political communication.

IMPLICATIONS OF AVAILABILITY AND SELECTIVITY

We hypothesize that mainstream news media will surpass interpersonal communication in their capacity to expose people to cross-cutting political perspectives for two reasons. First, there is a greater availability of dissimilar views in Americans' media environments than in their physical environments. Second, compared to personal interactions, people have less ability and desire to exercise selective exposure to news media content. With respect to the availability of dissimilar views, the media clearly have an advantage over face-to-face communication. Mainstream reporters are generally encouraged to illustrate stories with frequent references to people or groups who express conflicting views, in the typical point-counterpoint format, and as an appeal to large audiences tend to cover a range of opinions (e.g., Zaller 1992). As businesses, American news media are certainly not insulated from pressures to reflect public opinion, but national political news in local newspapers tends not to reflect local opinion (Dalton, Beck, and Huckfeldt 1998). In other words, the news media are not subject to the more narrow geographic constraints of face-to-face relationships, and they do not reflect the structurally dictated homogeneity of immediate neighborhoods or communities.

With respect to selective exposure, it is easier to avoid exposure to the views of personal acquaintances than to views expressed in national news. With daily newspapers and most national television news, the ability to exercise choice on the basis of partisanship is severely limited. Few communities have more than one daily newspaper from which to choose; moreover, few

newspapers have readily recognized political complexions that facilitate selective exposure to like-minded political views. Likewise, the political tone of national television news is very similar across channels. Of course, the same cannot be said about news magazines, talk shows, and political web sites. In more specialized media, people may well be able to select a news source that shares their political bent.

Aside from the ability to choose, media may produce less of a desire to exercise selective exposure as well. People often refrain from political discussions with heterogeneous others to avoid normative social pressure or the discomfort of public disagreement (Bennett, Fisher, and Resnick 1994; Ulbig and Funk 1999). These same people may be willing to expose themselves to media presentations, however, precisely because there is no personal interaction.

To summarize, we hypothesize that people are more likely to expose themselves to dissonant opinions through mediated rather than interpersonal communication, largely because of the lack of selective exposure involved. After describing our research design, we will evaluate contributions of the media and interpersonal communication to cross-cutting exposure and present results from three tests of our general thesis.

RESEARCH DESIGN

We used data from a representative national telephone survey sponsored by the Spencer Foundation and executed by the University of Wisconsin Survey Center in fall 1996, immediately before the presidential election. This survey included a battery of items that tapped the frequency with which respondents talked about politics with up to three political discussants, plus five separate items assessing the extent to which they agreed with the views of each discussant named (see Appendix A). These five items were combined into an additive scale that measured the extent to which people's networks expose them to political views unlike their own.[2] The sample of 780 respondents provided information on more than 1,700 discussants. To obtain information about social contexts in which weak ties are especially likely, we asked respondents the same five questions with respect to people they know through work and voluntary associations, which yielded two similar indices of exposure to political difference in these contexts. Finally, the same battery was asked with reference to the views respondents encountered through reading newspapers, watching television news, reading news magazines, and watching or listening to talk shows, after initial screening for use of a particular medium.[3]

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[2] Each of the five items was standardized and then combined into an additive index for each discussant and for each media source. To facilitate comparisons across information sources, they were also standardized with respect to the grand mean across all potential sources of exposure to dissimilar views. For the three primary political discussants, Cronbach's alpha indicated that these five items scaled relatively well: .78, .81, and .81 for the first, second, and third discussant, respectively.

[3] Again, these items scaled acceptably: alphas of .73, .73, .69, and .81

Although the sample is relatively small, the Spencer survey provides a great depth of information about exposure to political disagreement through both mass and interpersonal channels. Moreover, multiple indicators of the dependent variable make it possible to create indices that offer more reliable measures of the extent to which a given source provides exposure to oppositional views, as well as the extent to which it provides dissonant contact independent of the frequency of that contact.

The survey results were supplemented with data from the American and British components of the Cross-National Election Project (CNEP), obtained during the 1992 elections (for details on these studies, see Beck, Dalton, and Huckfeldt 1992; Heath et al. 1992). Although the CNEP data provide only one comparable single-item measure of the extent of exposure to disagreement through media and interpersonal channels, they allow us to replicate and extend our initial findings using data that capture both perceptions of information sources' views (those of up to five discussants, plus newspapers and television) and independent assessments of the extent of political disagreement, which were made possible by a "snowball" sample of respondents' discussants, plus a content analysis of respondents' newspapers.

Aside from the single-item (based on choice of presidential candidate) measure of political disagreement, the CNEP differs from the Spencer survey in one other respect. The CNEP asked respondents to volunteer the names of four people with whom they discussed "important matters," whereas the Spencer survey asked for people with whom they talked about "government, elections and politics." For the fifth discussant in the CNEP questionnaire, respondents were asked with whom they talked most "about the events of the recent presidential election campaign," which generated a more explicitly political discussion partner. Research on name generators suggests that the explicitly political frame will produce more non-relatives and discussants with whom there are weak ties (Huckfeldt and Sprague 1995b). Thus, the Spencer survey is more likely to generate discussants who will be politically dissimilar to the respondent.

SOURCES OF EXPOSURE TO CROSS-CUTTING POLITICAL VIEWS

Figure 1, which is based on the combined index constructed from these five parallel measures, summarizes overall levels of exposure to dissimilar political views by mass and interpersonal information sources. Whether the items are considered separately or in summary form, the findings are highly robust. As hypothesized, mainstream news media, especially newspapers and television, occupy the highest end of the disagreement continuum, followed closely by news magazines and more distantly by talk shows. This pattern makes a great deal of sense in light of people's

relative ability to exercise selective exposure to each of these media. It is more surprising that all the media sources, including news magazines and talk shows, surpass interpersonal sources in the extent to which respondents perceive them to involve views substantially different from their own.[4]

The degree of intimacy between main respondents and their discussants follows a highly predictable pattern: Closeness is inversely related to exposure to conflicting political views. Exposure to dissonant views is most likely with casual acquaintances (mean exposure to dissimilar views = .12), followed by friends (mean = −.24), close friends (mean = −1.11), and spouses or relatives (mean = −1.76). This significant linear trend indicates that cross-cutting exposure depends critically on contact with people who are not close friends or family ($F = 18.35, p < .001$). Moreover, as found in previous studies, the most frequent interactions tend to occur with the most politically homogeneous discussion partners (Huckfeldt and Sprague 1995a).

Even items that asked in very general terms about the people with whom respondents discuss politics—those known through work or a voluntary association (rather than a named discussant)—did not generate levels of dissimilarity as high as those for mediated sources. These general items purposely directed respondents' attention to potentially weak ties that would have the greatest probability of putting them in contact with dissimilar views, but the responses still suggested that these sources were relatively homogeneous compared to the media. Likewise, an index of disagreement drawn strictly from nonrelative discussants confirmed that both newspapers and television expose respondents to significantly greater political disagreement than do interpersonal discussants.

The paired t-tests in the note to Figure 1 make it clear that these mean differences cannot be explained by political or demographic differences among the users of various information sources; in each case, the users of any two sources are compared to themselves. In all possible comparisons of media sources and discussants, the discussants were less likely to expose respondents to dissimilar political views.

Paired comparisons maximize the sample size for each of the tests, but they are inappropriate for hypothesis testing because they do not adjust the observed significance levels for the fact that so many comparisons are being made. Thus, to test our main hypothesis—that mainstream media provide more exposure to dissimilar views than face-to-face communication—we used a repeated measures analysis of variance; the

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for newspapers, national television news, news magazines, and talk shows, respectively.

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[4] Given the progressive decline in concord with sequentially named discussants shown in Figure 1, it is possible that a fourth, fifth, or sixth discussant would have even more heterogeneous views, but it appears that few people can name that many political discussants. In our sample only 31% of respondents named a third person. In studies that ask for five political discussants, the proportion who do not name a fourth and fifth increases sharply (e.g., Huckfeldt and Sprague 1995b), thus making it unlikely that asking about additional discussants could overcome the large difference that we have observed.

FIGURE 1. Perceived Exposure to Dissimilar Political Views by Source
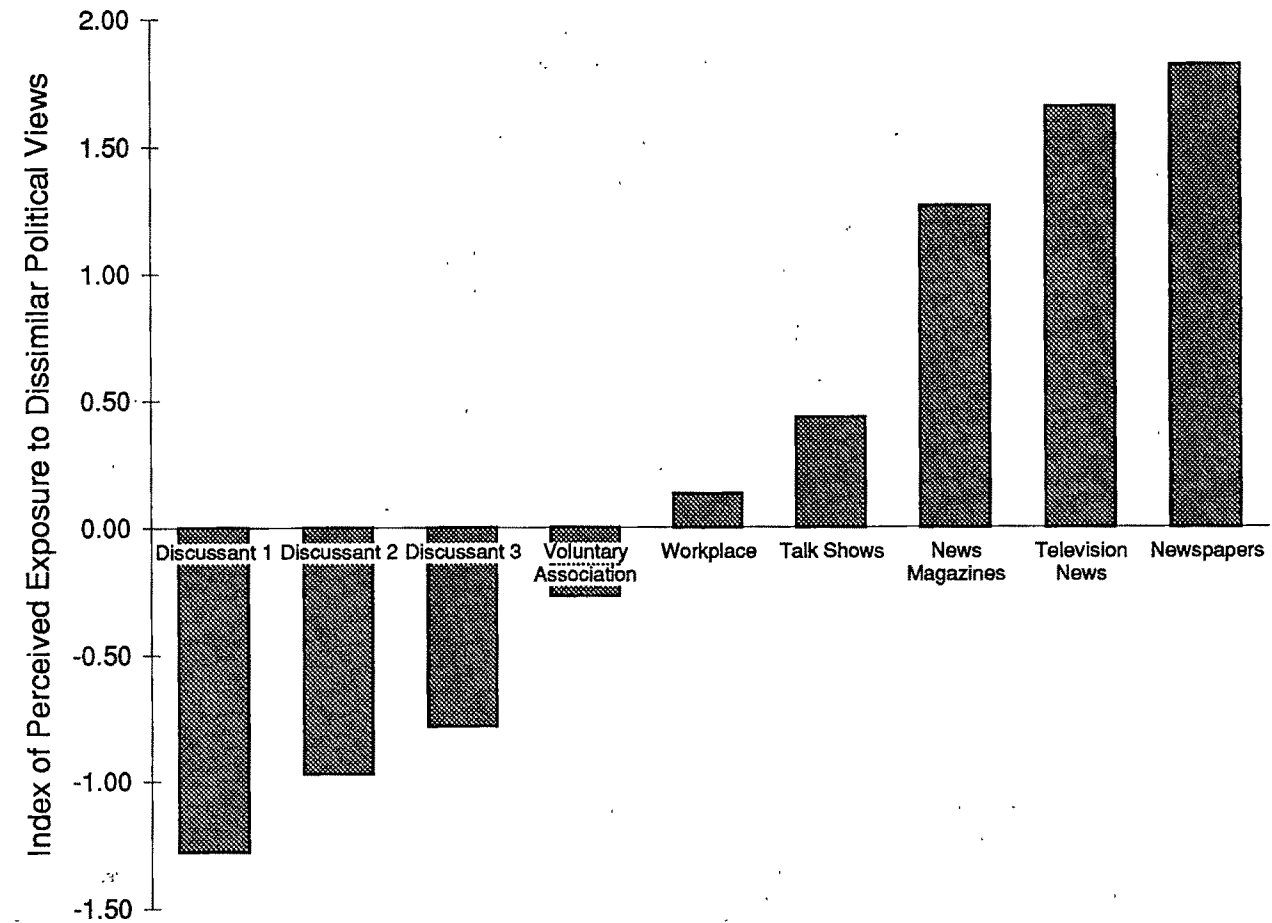
Source: Spencer survey, 1996.
Note: Sample sizes are below in parentheses, with two-tailed significance levels as noted. Statistical tests are based on pairwise comparisons.

Source	(n)	Disc. 1	Disc. 2	Disc. 3	Vol. Assn.	Work-place	Talk Shows	News Mags.	TV News
Disc. 1	(715)								
Disc. 2	(606)	.246							
Disc. 3	(446)	.069	.453						
Vol. Assn.	(597)	.000	.000	.015					
Workplace	(502)	.000	.000	.003	.049				
Talk Shows	(209)	.000	.000	.031	.028	.829			
News Mags.	(151)	.000	.000	.000	.001	.002	.246		
TV News	(478)	.000	.000	.000	.000	.000	.000	.053	
Newspapers	(640)	.000	.000	.000	.000	.000	.001	.061	.064

information source was designated as a within-subjects factor, and simple contrasts were done between a reference condition (interpersonal exposure to cross-cutting views) and mediated exposure to cross-cutting views. This procedure eliminates the problem of inflating the error rate with multiple t-tests, but its disadvantage when used to compare all means in Figure 1 is that the sample size for the analysis is constrained to the smallest group of media users.

In order to retain a more representative sample, we ran these comparisons for interpersonal communication versus newspaper readers and television viewers only, and we used three progressively more challenging standards for interpersonal exposure to cross-cutting

views: (1) the average dissimilarity of views within the respondent's political network, (2) the dissimilarity of the third (most politically dissimilar) discussant named, and (3) the dissimilarity of views among coworkers. The omnibus F tests for all three analyses were highly significant ($F = 214.35$, 54.77, and 40.39, respectively, $p < .001$ in all cases), as were the individual contrasts between interpersonal exposure versus television news ($F = 263.30$, 69.21, and 39.84, respectively, $p < .001$ in all cases) and interpersonal exposure versus newspaper news ($F = 301.65$, 71.35, and 66.02, respectively, $p < .001$ in all cases). Finally, we also included income, education, political interest, and partisanship in those models to see whether the gap

FIGURE 2.   Exposure to Dissimilar Political Views by Source and Partisanship

Legend:
- ▨ Average named discussant
- ▦ Voluntary association
- ☐ Workplace
- ▨ Television
- ■ Newspapers

Y-axis: Index of Perceived Exposure to Dissimilar Political Views

X-axis categories: Republicans, Independents, Democrats
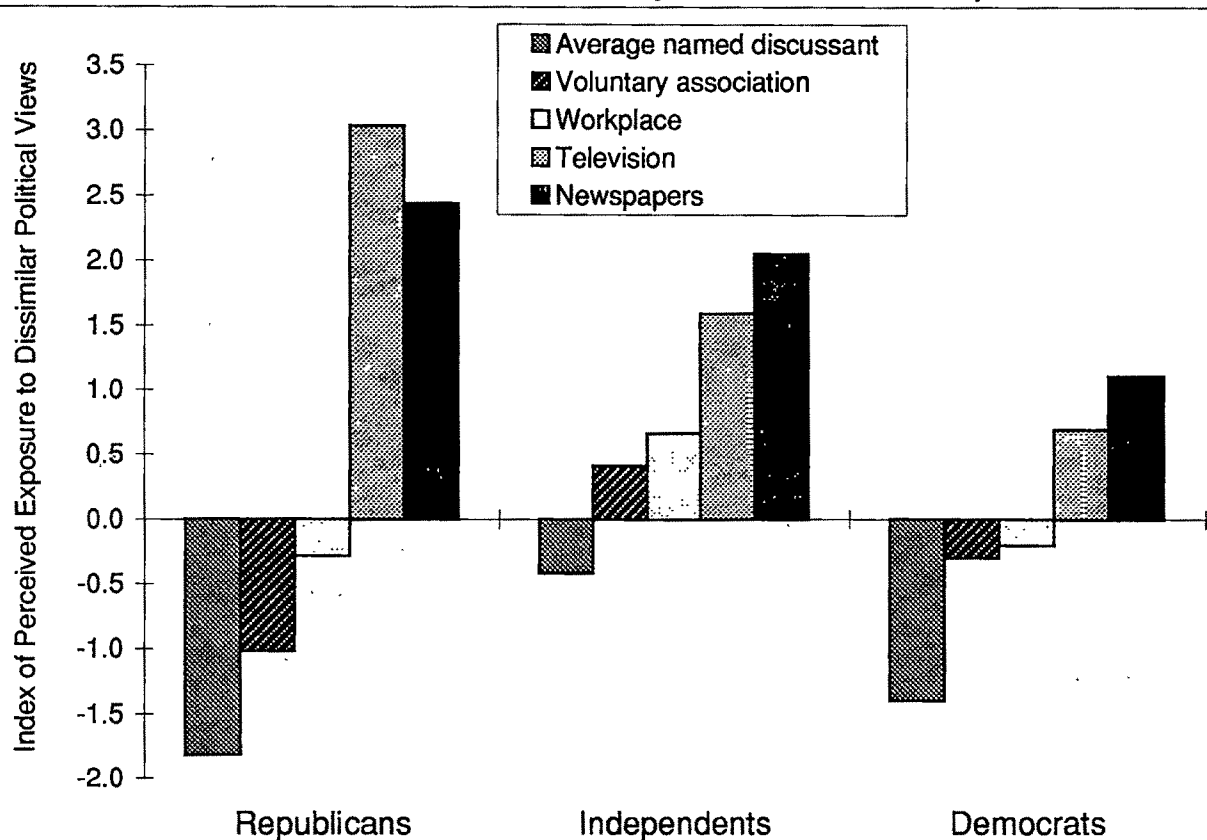
Source: Spencer survey, 1996.
Note: Within each of the three subgroups, all comparisons between media sources (television or newspapers) and interpersonal sources (average named discussant, workplace discussants, or voluntary association discussants) were significantly different and in the hypothesized direction ($p < .05$).

was especially pronounced in particular subsets of the population. The observed gap persisted in all cases, although partisanship was a particularly influential covariate.

As Figure 2 illustrates, there are important differences by party: Democrats tend to find mainstream news sources more agreeable, whereas Republicans have significantly more homogeneous interpersonal networks. These two tendencies combine to increase the size of the gap between media and interpersonal exposure to dissimilar views among Republicans, but it is both sizable and statistically significant among Democrats and independents as well.[5] In other words, the fundamental advantage of media in exposing people to dissonant information transcends partisanship.

The differences between extent of exposure to political dissonance through mainstream news media and through interpersonal networks are clearly large, robust, and statistically significant, but one might question whether the pattern observed in these data is

"real" or largely perceptual.[6] Unlike snowball samples, which ask named discussants about their political views, or content analyses of media messages, our measures examined thus far do not tap the actual similarity or dissimilarity of views but respondents' perceptions. These perceptions are subject to distor-

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[6] Another possible challenge to our interpretation is that, for people whose views are outside the mainstream, measures of disagreement constructed from party or candidate preference questions may not adequately operationalize exposure to disagreement. Many critics of the press highlight not so much an imbalance in presenting mainstream Republican or Democratic views as a dearth of more radical perspectives on either side of the spectrum. It should be noted, however, that three items in the index make no reference to parties or candidates, and the pattern of findings is virtually identical using an index comprised of only these three items. Thus, we do not believe that the components of the index addressing only mainstream political views account for the findings.

It is also possible that, even though the indices are comprised of identical items for media and interpersonal communication, a word such as "often" means something very different when applied to media versus interpersonal political communication (Schaeffer 1991). Relative frequency is involved in only one of the five items in the index. In addition, this argument should work against our hypothesis, because phrases indicating relative frequency will tend to mean larger absolute frequencies when the activity is more frequent, and interpersonal exposure to political news is less frequent than mediated exposure.

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[5] A repeated measures analysis of variance strictly among Democrats that simultaneously compared the interpersonal discussant average with exposure to difference through television and newspapers produced very strong findings (omnibus $F = 51.68$; contrast between personal network and TV: $F = 62.39$; contrast between personal network and newspapers: $F = 77.06$; all $p < .001$).

TABLE 1. Accuracy of Respondent Perceptions

Independent Assessment by Actual Discussant or Newspaper Coders	Respondents Perceive Discussants to Favor				Respondents Perceive Newspaper to Favor			
	Bush	Clinton	Perot	No One	Bush	Clinton	Perot	No One
A. Perceived versus Independently Assessed Agreement and Disagreement								
Bush	83%	7%	12%	43%	0%	0%	0%	0%
Clinton	7	81	19	35	71	72	67	61
Perot	9	9	63	14	4	1	0	2
No one	2	4	6	8	25	27	33	37
Percentage	100%	100%	100%	100%	100%	100%	100%	100%
Number of cases	(400)	(458)	(186)	(65)	(112)	(364)	(3)	(367)
B. Accuracy and Inaccuracy by Respondents' Views								
Inaccurate/strong projection	8%				4%			
Inaccurate/weak projection	4%				17%			
Accurate perceptions	78%				48%			
Inaccurate/weak contrast	4%				27%			
Inaccurate/strong contrast	5%				4%			
Percentage	99%				100%			
Number of cases	(1,036)				(788)			

Source: Cross-National Election Project, American component (Beck, Dalton, and Huckfeldt, 1992).
Note: For Part A, the unit of analysis on the left is the total pool of dyads for which information was available from both main respondent and discussant interviews; on the right the pool is all main respondents who read newspapers for which content analyses were available (see Appendix B). For Part B, the unit of analysis is the same but sample sizes are slightly lower because the perceptions of respondents with inaccurate perceptions who did not express a preference could not be categorized as contrast or projection. In Part B, we code a perception as projection when a respondent misperceives the newspaper/discussant to agree with his/her own views but independent assessments suggest that they do not agree. We characterize this as strong when the respondent prefers one candidate but the newspaper/discussant actually prefers a completely different candidate, and weak when the respondent misperceives a neutral newspaper/discussant to favor his/her own preferred candidate. We code a perception as contrast when a respondent misperceives a newspaper/discussant to disagree when independent assessments suggest that they agree. We characterize this as strong when they favor the same candidate but the respondent perceives the newspaper/discussant to favor a different candidate, and weak when the respondent misperceives a neutral newspaper/discussant to favor a different candidate from his/her own preference. Percentages may not add to 100 due to rounding.

tion and inaccuracy; for example, a great deal of interpersonal disagreement goes unrecognized, and errors sometimes occur in recognizing agreement as well (Huckfeldt et al. 1995).

For our purposes, we consider perceptual measures of disagreement more appropriate than actual measures. In order for disagreement to stimulate the kind of thought processes and reevaluation cited by Mill and others, it must be perceived as such. If people so misperceive others' views as to consider them to be in agreement, then they will not be prompted to reconsider their opinions, broaden their perspectives, and so forth. Likewise, if people perceive that others disagree—even if this is not the case—then the same benefits may derive despite the lack of objective disagreement. Thus, in one important sense, it may be meaningless to ask whether people are "really" exposed to views different from their own. If discussants are hesitant to make their difference of opinion known to friends, or if people persistently ignore differences that are communicated to them, then such interactions contribute little to the kinds of benefits political theorists have proposed. Moreover, Huckfeldt and Sprague (1995a) found that respondents were influenced exclusively by the views they perceived their discussants to have, not by their actual views.

Nevertheless, have we observed anything more than a conglomeration of systematic perceptual distortions?

The question is important because research in two areas highlights the systematic distortion in the perceptions of others' views that can result from strong partisan leanings. False consensus studies suggest that people will overestimate the extent of their agreement with others, and studies of the hostile media phenomenon indicate that people sometimes overestimate the extent of their true disagreement with the mass media (Giner-Sorolla and Chaiken 1994; Vallone, Ross, and Lepper 1985). Separately or in combination, these perceptual distortions may account for our finding of greater exposure to perceived political disagreement through the mass media than through interpersonal communication.

Drawing on the CNEP data, Table 1 shows that most perceptions of interpersonal agreement are accurate; overall, for all respondent-discussant dyads, respondents correctly named the political views of over 70% of their discussants. We sorted based on whether the distortion was in the direction of the main respondent's view (projection) or away from it (contrast). We then classified these cases as strong, if the misperception was the opposite of the discussant's true preference, or weak, for example, when a true independent or undecided discussant was perceived as a Democrat by a Democratic respondent.

As the bottom left panel of Table 1B shows, roughly 12% of respondents erred in the direction of their own

FIGURE 3.   Perceived and Independently Measured Exposure to Dissimilar Political Views by Source
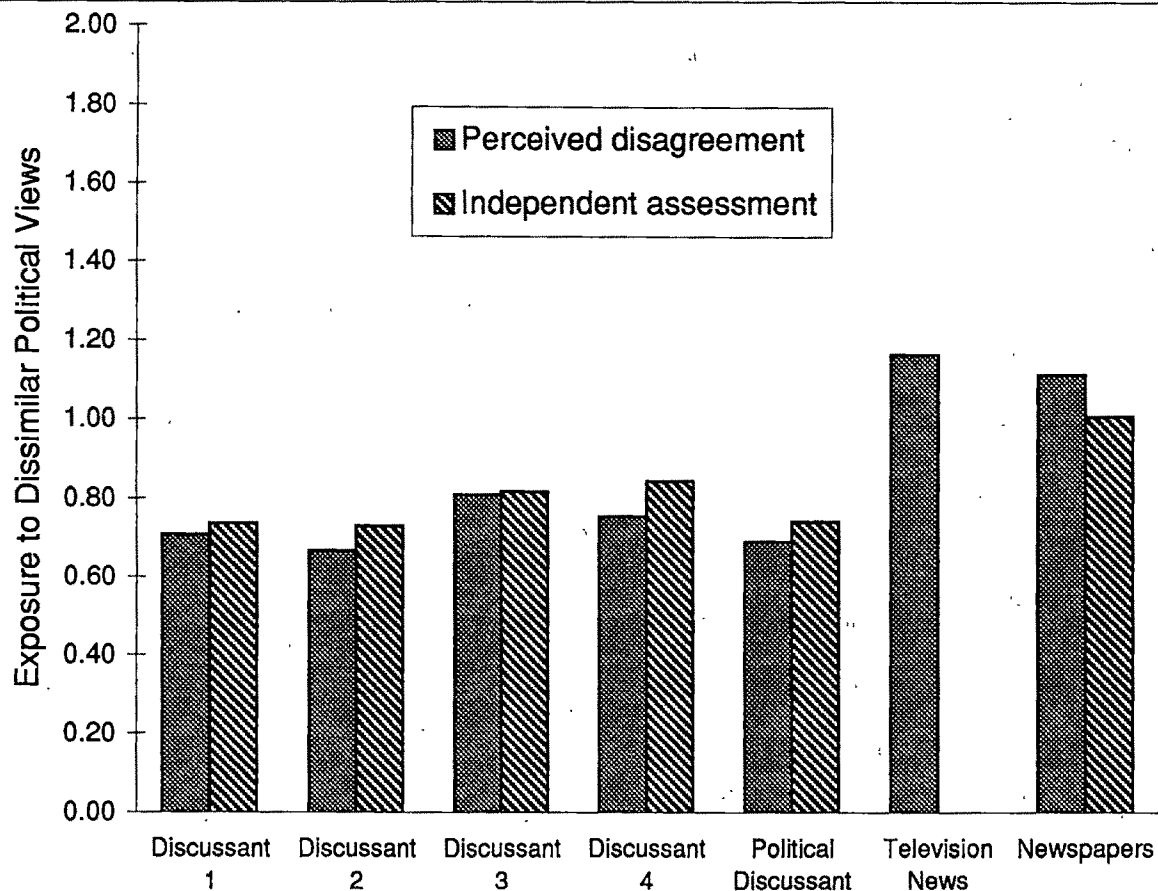
Source: Cross-National Election Project, American component (Beck, Dalton, and Huckfeldt 1992).
Note: No independent assessment of television news content was available.

views, and 9% in the direction opposite their own views. The results suggest significant projection $(t = 2.72, p < .01)$, consistent with much of the psychological literature on this topic (e.g., Fabrigar and Krosnick 1995; Krueger and Clement 1994).[7] In other words, systematically more errors occurred in the direction of perceiving greater interpersonal agreement (projection) than was actually the case, just as the false consensus hypothesis predicts. The bottom line is that people are exposed to more interpersonal disagreement than they recognize, but because they fail to recognize it, it probably has little capacity to produce the beneficial effects of cross-cutting exposure. Conversely, as shown in Table 1A for newspapers, there is a tendency to perceive greater disagreement (contrast) with the media than content analyses suggests to be the case.[8] For newspapers, 21% of respondents showed signs of projection, and a significantly greater 31% showed signs of contrast $(t = 3.56, p < .001)$.

In Figure 3, the CNEP U.S. data seem to confirm the pattern observed in the Spencer survey, in that people perceive media to expose them to more oppositional views than do their personal networks. As shown by the solid bars, this is true across the four "important matters" discussants as well as for the explicitly "political" discussant. The striped bars in Figure 3 represent independent assessments provided by discussants and drawn from independent coding of respondents' newspapers.[9] Figure 3 suggests that the true extent of exposure to oppositional views may be slightly overestimated in the case of newspapers and somewhat underestimated in the case of interpersonal relations. But most important, even if one uses the "real" measures based on discussant reports and content analyses, newspapers still provide significantly greater exposure to views different from the respondents' own $(t = 5.74, p < .001)$. In other words, the advantage of news media over interpersonal channels in relaying political

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[7] To test whether the errors were random or systematically in the direction of respondents' own views, we assigned values of −2 to +2 to the contrast and projection scores, with accurate perceptions equal to 0. We then tested the hypothesis that the mean was equal to 0.
[8] We use newspapers for this comparison because content analysis is available.

[9] Whether a publication favored one candidate or none was established by running a repeated-measures analysis of variance on the articles coded for each newspaper to see whether there was a statistically significant difference in favorability and, if so, in what direction. If no statistically significant difference emerged, the newspaper was coded as favoring no one.

disagreement is real, not simply a matter of perceptual distortion.

Although the Spencer data do not include independent measures of exposure to disagreement by source, the results are consistent with the CNEP finding that the relative media advantage is not simply a function of perceptual distortion. The perceptual distortion inherent in both false consensus and hostile media effects is driven by partisanship; in the former case, it leads people to perceive greater support for their own views than truly exists in their environment, and in the latter case, it leads them to perceive greater opposition. Another way to grasp the extent of partisan distortion in the perceptual data is to examine whether the same relative advantage of news media over interpersonal sources exists among political moderates or nonpartisans.[10] Figure 2 illustrated that even independents (or those without ideological leanings), that is, people with little or no partisan predisposition to distort their impressions of conflict and consensus, believe the news media expose them to dissimilar political views more than interpersonal sources (newspapers: $t = 6.97, p < .001$; television: $t = 6.61, p < .001$).

Figure 4 makes an additional point concerning the relationship between partisan extremity and exposure to cross-cutting political perspectives. If we were to plot the hostile media hypothesis on one of these graphs it would be U-shaped; studies demonstrating a hostile media effect show that partisans at *either* end of the spectrum will report more exposure to oppositional views, even when the exact same media content is involved (Vallone, Rose, and Lepper 1985). If a medium were instead perceived to be more congenial to one side of the political spectrum than the other, we would expect a linear pattern: People at one end of the spectrum would find it least dissimilar to their own views, people at the other end would consider it dissimilar, and most independents and nonpartisans would fall somewhere between.

Figure 4 plots all the available indices of exposure to disagreement, for both mediated and interpersonal channels, across levels of partisanship. In addition to showing the means for each level of partisanship, it reveals either linear or curvilinear patterns depending on whether trend tests across the means indicated a significant linear or curvilinear component, with the highest order significant pattern displayed. In the top panel of Figure 4, for all media sources there is a significant linear pattern; compared to Democrats, Republicans perceive television and newspaper news as exposing them to a great many more views unlike their own. For talk radio, the pattern is reversed but still linear: Democrats are more likely than Republicans to find that the views expressed are in disagreement with their own, and independents fall in the middle. Despite the relative consonance of views that Republicans perceive in talk shows, they believe this medium offers

dissimilar views to a greater extent than do their interpersonal networks (repeated-measures analysis for television versus personal network average among Republicans, $t = 2.44, p < .05$).

Although the earlier finding of greater contrast than projection in perceptions of newspaper content seemed consistent with the hostile media hypothesis, Figure 4 makes the further point that it would be erroneous to describe this pattern as one in which partisans on *both* ends of the political spectrum see the news media as more hostile to their views than do independents (cf. Dalton, Beck, and Huckfeldt 1998).[11] Democrats perceive newspapers and television news to be relatively agreeable but, as shown in Figure 2, Democrats still consider these sources more dissonant than their interpersonal networks.

As shown in the bottom panel of Figure 4, in contrast to the linear patterns for media, interpersonal assessments of exposure to dissimilar views consistently yield an inverted U-shape. This indicates that, as compared to independents, strong partisans are less exposed to views unlike their own. The pattern holds for both the index for average discussants and contacts in the workplace or through voluntary associations.

If people successfully exercise selective exposure, then one would expect to see a pattern in which strong partisans are most likely to be exposed to consonant information, and weaker or nonpartisans less so. Indeed, the fact that this pattern is true for interpersonal sources but not for mainstream media lends support to our argument that selective exposure is less influential in shaping people's exposure to mediated than to interpersonal information. There are, of course, other differences between strong and weak partisans, but it is worth noting that the generally narrower latitudes of acceptance of the strongly partisan should work against this finding; strong partisans should find more views expressed by media and by other people disagreeable if only because not much of a difference of opinion is required for strong partisans to perceive a viewpoint as unlike their own (e.g., Sherif and Hovland 1961).

Findings from the CNEP replicate these patterns. For television news and newspapers, perceived disagreement is a linear function of partisanship. For interpersonal communication, strong partisans on both sides of the political spectrum are exposed to less disagreement than independents. Moreover, this pattern remains curvilinear whether based on respondents' perceptions or the independent reports of discussants.

In sum, our evidence suggests that most people perceive substantial political harmony in their interpersonal associations and tend to misperceive disagree-

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[10] Independents and those who refuse to identify themselves in partisan terms may be quite different, but examination of these groups separately and collectively produced the same pattern of results.

[11] We use the same data as these authors for much of our analyses, but we arrive at different conclusions about a hostile media effect because the single variable for partisanship (ranging from strong Democrat to strong Republican) on which they base their interpretation cannot distinguish between a hostile media pattern and the partisan media perception we observe, i.e., only Republicans (but not Democrats) perceive media coverage as more hostile to their views (see Giner-Sorolla and Chaiken 1994; Vallone, Ross, and Lepper 1985).

FIGURE 4. Perceived Dissimilarity of Views by Source and Strength of Partisanship

Media Sources

Interpersonal Sources
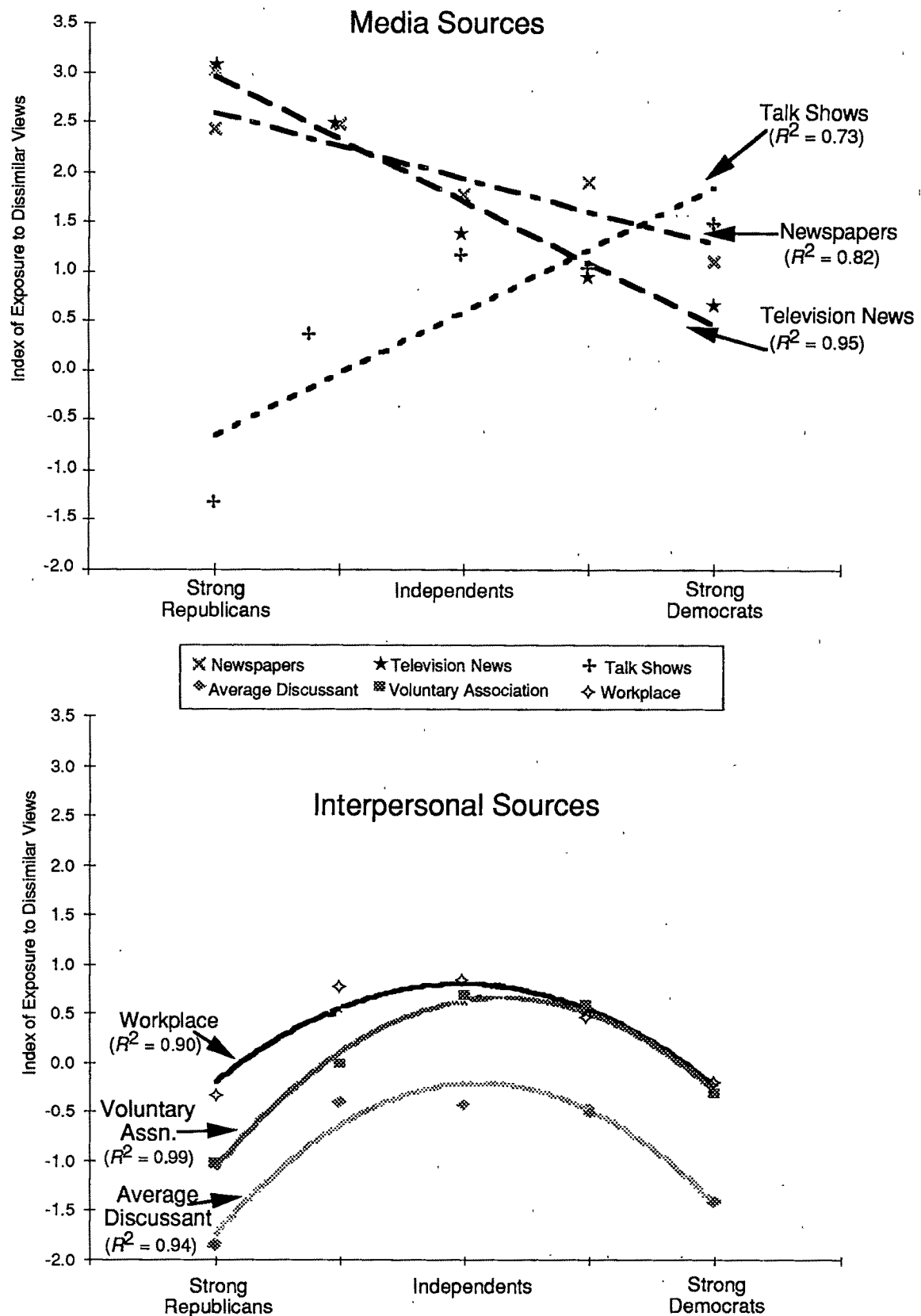
ment that may exist. Political diversity is clearly not a goal in social relationships for most Americans; people tend to cultivate homogeneous interpersonal networks, and those with strong partisan attitudes are particularly likely to be surrounded by similar others. In comparison, the mainstream media expose people to more political disagreement, regardless of partisanship or the exremity of their views.

EXPLAINING THE MEDIA ADVANTAGE

What is it about media that enables these communication channels to more successfully expose people to cross-cutting political viewpoints? We suggest that people often seek political reinforcement from their information sources, whether mass or interpersonal, but that the extent to which people are exposed to dissimilar views through a given source reflects variation in their motivations to exercise selective exposure and the ease with which it is possible to do so. This hypothesis can be tested in three different ways with the Spencer data. First, choice should facilitate less exposure to dissonant views. Therefore, one would expect people with a choice of local daily newspaper to agree more with the views in the one selected than do those with no choice.

Second, our theory also implies that findings should depend upon the media culture of a particular time and place. One would expect, for example, that our findings would be time-bound even in the United States; in the era of the partisan press it is unlikely that people received much except partisan reinforcement from their daily papers (Schudson 1981). Newspapers of that period would not have fared any better than face-to-face communication in exposing people to cross-cutting views. While we have no suitable historical data to test this hypothesis, we can approximate such a comparison by comparing the contemporary United States to a country where the press is more closely aligned with parties and political views. Drawing on the British component of the CNEP, we compare exposure to cross-cutting views in American and British newspapers and interpersonal networks. We expect that selective exposure is much easier when the media are overtly partisan, and British newspapers should play a less important role than the American press in exposing people to dissimilar political views.

Third, we examine individual differences in the extent to which people are comfortable with face-to-face conflict and thus are differentially motivated to exercise selective exposure. Although people in general dislike conflict, there are individual variations in how strongly people are motivated to avoid it (Ulbig and Funk 1999). We hypothesize that the media will be responsible for a particularly large proportion of a person's total exposure to dissonant views if he or she is uncomfortable with face-to-face disagreement.

TABLE 2. The Effect of Availability of Multiple Local Newspapers on Exposure to Dissimilar Views in the United States

	Coefficient (s.e.)	t-value
More than one newspaper available in area	−.43* (.20)	2.20
Republican Party identification	−.07 (.22)	.30
Democratic Party identification	−.72** (.22)	3.25
Education	−.01 (.04)	.26
Age	.00 (.01)	.49
Sex	−.25 (.19)	1.34
Race	.37 (.28)	1.34
Income	.01 (.01)	1.80
Constant	1.56* (.71)	2.19
R^2	.06	
(n)	(460)	

Source: Spencer survey, 1996.
Note: Availability is coded 1 for zip code areas with only one newspaper, 2 for areas with more than one. Entries are ordinary least-squares regression coefficients, with standard errors in parentheses. *$p < .05$, **$p < .01$.

Local News Options

If the motivation to selectively expose one's self influences political information consumption (despite the difficulty in fully realizing this goal), then those who live in areas with a choice of local daily newspapers should be exposed to fewer dissonant views in the one they choose. Of course, the difference in political slant between local newspapers can be very subtle, but nonetheless the population in multiple newspaper towns has some degree of choice. To test our hypothesis, respondents in the Spencer survey were matched with information about the circulation of daily newspapers in their zip code area, as obtained from the Audit Bureau of Circulation. Zip code areas with only one daily were coded 1; those with more than one were coded 2. Because correlates such as income, education, or partisanship might produce a spurious relationship or mask a significant relationship between exposure to dissimilar views and access to multiple newspapers, control variables were included. For example, markets with multiple newspapers are likely to be better educated and urban, and these demographic characteristics may be associated with the likelihood of exposure to diverse viewpoints.

As shown in Table 2, respondents in areas with more than one newspaper reported significantly less exposure to dissimilar views through their daily newspaper, even after controlling for partisanship and other variables, although the model as a whole accounts for very little variance in exposure to disagreement. Given the often subtle differences in the political complexion of newspapers within a locale, the modest size of the effect is not surprising.

TABLE 3. Influence of Partisanship on Exposure to Dissimilar Views by Source and Country

	Exposure to Dissimilar Political Views	
	United States	Great Britain
Through Interpersonal Discussions		
Republican/Conservative	$-.34^{***}$ (.04)	$-.27^{***}$ (.05)
Democrat/Labour	$-.43^{***}$ (.04)	$-.25^{***}$ (.05)
Frequency of discussion	.01 (.06)	$-.08^{*}$ (.03)
Political interest	.04 (.04)	.00 (.03)
Education	$-.01$ (.01)	$.04^{*}$ (.02)
Age	$-.00$ (.00)	.00 (.00)
Gender	$-.08$ (.07)	$-.09$ (.07)
Initial log-likelihood	-1556.77	-1436.15
Final log-likelihood	-1477.30	-1408.56
χ^2	158.95^{***}	55.19^{***}
(*n*)	(998)	(1,074)
Through Newspapers		
Republican/Conservative	$.14^{***}$ (.04)	$-.28^{***}$ (.04)
Democrat/Labour	$-.27^{***}$ (.04)	$-.10^{**}$ (.04)
Frequency of reading	.04 (.02)	$-.02$ (.05)
Political interest	$-.04$ (.05)	$-.07^{**}$ (.03)
Education	$-.01$ (.01)	$-.03^{*}$ (.02)
Age	$-.01^{**}$ (.00)	$-.00$ (.00)
Gender	.03 (.07)	$-.01$ (.06)
Initial log-likelihood	-1048.27	-1586.51
Final log-likelihood	-991.84	-1542.03
χ^2	112.86^{***}	88.94^{***}
(*n*)	(1,033)	(1,687)

Source: Cross-National Election Project, British and American components, 1992.
Note: For Interpersonal discussion, the dependent variable is the average exposure to dissimilar views through all the respondent's discussants; for newspapers, it is the extent of exposure to dissimilar political views through the respondent's most often read newspaper on the same scale. Entries are ordered probit coefficients, with standard errors in parentheses. Cutpoints are not shown. $^*p < .05$, $^{**}p < .01$, $^{***}p < .001$.

A Partisan Media Environment

A media environment with more sharply defined political differences between newspapers provides a better test of whether selective exposure is exercised when conditions make it easier to do so. British voters are well aware of the partisan slant in their newspapers, and there are many competing products from which they can choose (Curtice, Schmitt-Beck, and Schrott 1998, 12). If we are correct that the largely nonpartisan U.S. press makes it difficult for people to selectively expose themselves to media content, then we would expect a markedly different pattern among British respondents. Because British partisans should find it relatively easy to exercise selective exposure, the relationship between partisanship and exposure to dissimilar views should take the same curvilinear shape as for interpersonal sources; that is, strong partisans on both sides should be less exposed to dissimilar political views.

Table 3 examines the relationship between strength of partisanship and exposure to dissimilar views through the media and interpersonal communication for British and American respondents. In the top panel we find in both samples the same pattern shown in Figure 4 for interpersonal discussants in the Spencer survey. As the significantly negative coefficients in the top two rows of Table 3 indicate, partisanship in both countries reduces the amount of cross-cutting interpersonal exposure that people experience. In the bottom

panel, the hypothesized difference between the amount of selective exposure exercised in the two news environments is confirmed. As anticipated, British partisans on both sides are significantly less likely than Americans to be exposed in their newspapers to political views with which they disagree. In other words, they can and do find news sources that mesh with their political predispositions. Just as with face-to-face exposure, strong partisanship in Britain promotes greater selective exposure.

In contrast, the American case reveals a simple linear pattern: compared to nonpartisans, Democrats are less likely to encounter dissimilar views in their newspapers than are nonpartisans, but Republicans are more likely than are nonpartisans to do so. To confirm a general pattern of selective exposure, both party coefficients need to be significant and negative, but the coefficient for Republicans is significant and positive. In other words, given the opportunity to exercise selective exposure through their choice of newspaper, respondents could do so in the highly partisan British news environment, but fewer could do so in the American case. The differing results can be seen in the marginals for these dependent variables as well. Among newspaper readers, 51% in Britain read a paper that shares their political leanings, but in the United States the figure is only 16%. Conversely, 35% in the United States read a paper that explicitly disagrees with their political leanings, whereas the com-

parable figure in Britain is only 20%. In both countries there is more cross-cutting exposure through newspapers than through interpersonal networks, but the difference between the extent to which newspapers and interpersonal communication provide exposure to dissimilar views is significantly larger for Americans ($t = 4.58$, $p < .001$). Selective exposure clearly occurs under the right real world conditions; when people have a choice, they tend to use it to reduce their exposure to cross-cutting political views.

Comfort with Face-to-Face Controversy

In addition to limited opportunities to exercise selective exposure, the media advantage over interpersonal sources may be due to its impersonal nature, which circumvents the discomfort of face-to-face controversy. Beyond fewer opportunities to exercise selective exposure, Americans may have less motivation to do so with mass media. We hypothesize that people will obtain a larger relative proportion of their total exposure to dissimilar views from interpersonal communication if they are comfortable with face-to-face conflict. As a test we used two survey items designed to assess the communication patterns encouraged during early political socialization in the family (see Appendix A for question wording and McLeod and Chaffee 1972 for a review of the literature). For our purposes, it matters little whether these items tap accurate recollections of parental behavior or reflect current attitudes toward controversy. In either case, we anticipate that people who are uncomfortable with face-to-face conflict are likely to get more of their dissonant information from the media relative to people who are comfortable with interpersonal conflict.

We compared the average level of exposure to disagreement in respondents' interpersonal networks with the average level for television and newspapers, subtracting the former from the latter. A high score on this measure indicates greater media exposure to dissonance, and a low score indicates greater interpersonal exposure. Because these difference measures are not independent of overall exposure levels, we included a control for the total amount, that is, the sum of media and interpersonal exposure. This allowed us to estimate the relative importance of the media, controlling for the effect of overall exposure levels. Due to the obvious role of general political interest in motivating exposure, it was also included.

As shown in Table 4, the total amount of exposure to dissimilar views positively predicts the importance of media relative to interpersonal sources. The relative importance of media is especially great for Republicans, who have significantly more homogeneous interpersonal networks than Democrats. In contrast, a high level of education makes the media less important vis-à-vis interpersonal sources. Most important for our hypothesis about why people turn to the media is the controversy scale, which is a significant negative predictor of the relative importance of the media. In other words, as expected, people who are comfortable with face-to-face conflict get a relatively larger amount of

TABLE 4. Relative Importance of the Media in Exposure to Dissimilar Views by Comfort with Controversy

	Coefficient (s.e.)	t-value
Comfort with face-to-face controversy	−.37* (.16)	2.35
Total exposure to dissimilar views (media average + interpersonal average)	.47*** (.05)	9.21
Political interest (high)	.35 (.34)	1.02
Republican Party identification	3.74*** (.70)	5.33
Democratic Party identification	.58 (.66)	.89
Education	−.26* (.13)	2.06
Age	.01 (.02)	.40
Income	.01* (.00)	2.30
Race	−.65 (.82)	.79
Gender	−.67 (.56)	1.19
Constant	5.59* (2.36)	2.37
R^2	.38	
(n)	(305)	

Source: Spencer survey, 1996.
Note: The dependent variable is the relative importance of the media (averaged level) compared to interpersonal network (averaged level). Entries are ordinary least-squares regression coefficients, with standard errors in parentheses. *$p < .05$, ***$p < .001$.

their dissonant information interpersonally rather than from the media. The opposite is true for people who find controversy unpleasant; they are more likely to be exposed to dissimilar views through the media. This finding supports our proposition that impersonality helps explain the media advantage. Variability in selective exposure reflects not only the availability of partisan information sources, as shown in tables 2 and 3, but also individual differences in the desire to avoid interpersonal conflict.

DISCUSSION

Communication across lines of political difference is essential to the social psychological basis of a pluralist society. Pluralism requires that a society be able to endure ongoing political and moral disagreement (e.g., Berlin 1969). Furthermore, according to most democratic theorists, the expression of opposing political views is integral to the democratic process. Yet, the desire for harmony in interpersonal relationships makes political disagreement both relatively infrequent and particularly difficult to convey successfully. In addition, residential balkanization in the United States is decreasing the likelihood of heterogeneous face-to-face interaction. These factors make it all the more important to understand the avenues through which people obtain exposure to dissimilar political views. Our findings suggest that media are far more important than interpersonal networks in exposing people to views unlike their own. As a result, the media have the potential to make an extremely important contribution

to awareness of diverse political perspectives and thus to national political integration.[12]

Our findings also have implications for many debates regarding the media and public opinion in the United States. On the one hand, the stereotypes that Lippmann (1922) feared capable of eliminating exposure to dissonant viewpoints altogether appear to be potent but limited forces. Selective exposure is exercised with respect to interpersonal contacts in the contemporary United States, to be sure, but at least as of the 1990s, the structure of mainstream media makes it difficult for people to screen out large amounts of oppositional information. On the other hand, cross-cutting exposure and intrapersonal awareness of oppositional views does not necessarily translate into the kind of face-to-face deliberation that theorists such as Dewey (1927) had in mind. Nonetheless, traditional news media such as newspapers and television news hold the potential for creating greater awareness of cross-cutting views.

Many theorists, like Dewey, have suggested that only face-to-face deliberation can bring about the kind of democratic ends implied by the notion of a public sphere (e.g., Barber 1984; Fishkin 1991). Others emphasize that, "in modern societies, public deliberation is (and probably must be) largely mediated" (Page 1996). The usual rationale is that the mass media may be fine for relaying information, but they are not very useful for fostering public discourse and collective action, because they are essentially one-way communication to people in spatially dispersed and privatized settings (Carey 1987).[13] This argument makes considerable sense if collective action or consensus-building is the outcome of interest. But if one focuses instead on the importance of exposure to political perspectives in order to promote awareness of diverse viewpoints, political tolerance, and benefits of this kind, then the inherent limitations on the media's contributions are less obvious.

The sheer idea that mass media might serve to the benefit of the public sphere strikes most as heretical. But, in assessing media's contribution, there is a tendency to rely on an ideal standard drawn from interpersonal communication, typically akin to the ideal

speech situation (Habermas 1989) in which everyone has an equal opportunity to participate, everyone listens to and carefully considers others' views, and so forth (e.g., Fishkin 1991; Knight and Johnson 1994). This "conversational ideal" is not drawn from real-world experience, but it is often used as the standard against which media are judged and found wanting: "We are not really interested in what face-to-face communication is like: rather, we have developed a notion that all communication *should* be like a certain model of conversation, whether that model really exists or not" (Schudson 1982, 43, emphasis in original).

Instead, if we compare the reality of face-to-face political communication with the reality of media political communication, we have a more solid foundation on which to base conclusions about the contributions that both forms may make to a public sphere. There is a disappointing tendency toward homogeneity in face-to-face political communication, whereas the media can transcend interpersonal geography and expose people to views unlike their own. Regardless of one's views on whether the media present a broad enough range of perspectives, it is clear that for many Americans they are the main source of exposure to cross-cutting political views. Compared to most interpersonal networks, they are hotbeds of diversity, not because the media are doing such an exemplary job pursuing diversity, but because individuals are doing such a poor one, in part because of the desire to avoid conflict in interpersonal situations. The media send multiple conflicting messages, and in so doing they advance an important aspect of the democratic process.

Of course, the patterns we have observed could simply be interpreted as evidence of the media's limited influence on political attitudes. That is, if homogeneous interpersonal channels persuade people to hold certain views, and the media are not equally persuasive, then the media will, as a result, appear to provide more exposure to dissimilar views. Even if this were the case, it still could be argued that media play an important role in furthering cross-cutting exposure in a way that interpersonal communication does not and perhaps cannot. Previous studies have found limited support for the social cohesion view of the effect of political discussants (cf. Huckfeldt and Sprague 1995a; Kenny 1994), that is, that the cohesiveness of social relationships determines their persuasiveness. But regardless of the relative power one attributes to mass or interpersonal communication to persuade people to change their mind, any exposure to differing views that does not produce instantaneous compliance is a valuable component of public opinion formation and of the process of legitimizing differing views within a pluralist system.

In the contemporary United States, the major problem with looking to media to fulfill this role is the current trend toward highly specialized rather than mass channels. The breakup of the network broadcast monopoly, the proliferation of Internet news sources, and the potential they offer for tailoring news to one's own interests and prejudices are only a few signs of this

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[12] Our findings are consistent across two data sets and two presidential elections, but this could be a function of similarities between the 1992 and 1996 elections. For example, the perception among Republicans and Democrats of media favoritism toward Clinton may be a function of his front-runner status in both cases. Nonetheless, our findings are consistent with many previous studies in suggesting that Americans perceive media content as to the left of their positions and consider journalists more liberal than the general population (Weaver and Wilhoit 1996). This does not necessarily mean that the content journalists produce favors one side, but the perception is that the mainstream media lean left if they lean at all (Watts et al.).

[13] Much has been made of the importance of direct interpersonal relationships in nurturing a sphere of politically oriented discourse, but a growing number of perspectives challenge that idea. Sanders (1997), for example, points out that the formal standards for the deliberative ideal are seldom, if ever, met. As usually practiced, face-to-face deliberative decision making may essentially perpetuate and exacerbate social inequalities. Schudson (1997) disputes the conversational ideal that pervades current writing on the quality of public life, arguing that democracy has little to do with the kind of intimacy and community that characterize this romanticized view of face-to-face interaction.

trend. As the number of potential news sources multiplies, consumers must choose among them, and that exercise of choice may lead to less diversity of political exposure. Although media observers have long lamented the lack of choice in news sources, the proliferation of choice creates new problems. Advertisers and media firms are now working together to create "electronic equivalents of gated communities" (Turow 1997, 2). The market segments they identify often are not based explicitly on political views, but "lifestyle" categories are hardly independent of political leanings.

Turow (1997, 3) argues that "segment-making media," those which "encourage small slices of society to talk to themselves," are on the rise, whereas "society-making media," which "have the potential to get all those segments to talk to each other," are on the decline. This leaves the future of the mass audience in question (cf. Neuman 1991). These developments obviously do not bode well for the future of communication across lines of political difference. In addition to reducing the amount of direct exposure to dissonant views, specialized media and fragmented audiences may have secondary negative effects on interpersonal communication, since shared viewing or reading may encourage conversations across partisan lines (Katz 1996).

Ironically, the mass media's greatest strength may not be facilitation of the "Great Community" of solidarity envisioned in the early twentieth century by media philosophers such as Cooley and Dewey (Simonson 1996). Instead, media's greatest potential lies in its impersonal exposure of audiences to cross-cutting views, an essential form of communication in a highly pluralistic society. In order to sustain this benefit, however, news media must be structured so as to limit the public's capacity for selective exposure.

APPENDIX A. THE SPENCER SURVEY

Design

This national telephone survey was conducted by the University of Wisconsin Survey Center from September 1996 through election eve using random digit dialing. Each number was screened to verify that it was associated with a household. The person selected for the interview was randomly chosen from among household members at least 18 years old, with no substitutions allowed. The response rate was 47%, calculated as the proportion of completed interviews divided by total sample (which includes those who never answered and all other nonresponses and refusals) minus the nonsample numbers. This is virtually identical to the rate obtained in the CNEP survey. Interviews averaged 25 minutes. A maximum of 30 calls were made to each nonanswering or otherwise unresolved telephone number.

Discussant Generator

"From time to time, people discuss government, elections, and politics with other people. We'd like to know the first names or just the initials of people you talk with about these matters. These people might be from your family, from work, from the neighborhood, from some other organization you

belong to, or they might be from somewhere else. Who is the person you've talked with most about politics? [Discussant #1] Aside from this person, who is the person you've talked with most about politics? [Discussant #2] Aside from anyone you've already mentioned, is there anyone else you've talked with about politics? [Discussant #3]" If at any point the respondent could not give a name: "Well then, can you give the first name of the person with whom you were most likely to have informal conversations during the course of the past few months?"

Media Source Generators

Newspapers: "During the past week, did you read one or more newspapers? Which newspaper was that?"

Television News: "During the past week, did you watch any national news programs on television? What national news show was it that you watched? Which news program do you watch most often [if respondent names more than one]?"

News Magazines: "During the past week, did you read any news or current events magazines, such as *U.S. News, Time, Newsweek*, or some other news magazine? Which magazine was that?"

Political Talk Shows: "During the past week, did you happen to see a talk show on television or hear a talk show on the radio that included some discussion of political or social issues? What talk show was that?"

Voluntary Associations and Workplace Generators

Respondents were first asked about membership and participation in a list of types of voluntary associations, similar to those used in the General Social Survey. People who claimed membership in more than one such group were randomly assigned to one of the groups for a follow-up sequence, which asked identically worded questions parallel to those asked about the political discussants. These same five items were also asked with reference to coworkers, clients, or customers that employed respondents encountered in the workplace. The five items asking about voluntary associations produced an alpha of .74, and the same items referring to the workplace scaled at .72.

Five-Item Index of Exposure to Dissimilar Views

1. "Compared with [named discussant, media outlet, voluntary association, or workplace], would you say that your political views are much the same [low], somewhat different, or very different [high]?"
2. "Do you think [named discussant, media outlet, voluntary association, or workplace] normally favors Republicans or Democrats, or both, or neither?" Scoring: same as respondent's partisanship (low), different from respondent's partisanship (high), or neither.
3. "Which presidential candidate, if any, does [named discussant, media outlet, voluntary association, or workplace] favor? Clinton, Dole, Perot, or some other candidate?" Scoring: same as respondent's preference (low), different from respondent's preference (high), or neither.
4. "Overall, do you feel [named discussant, media outlet, voluntary association, or workplace] shares most of your views on political issues [low], opposes them [high], or doesn't [it/person's name] do either one?"
5. Media version: "How often do you disagree with political views you [read/hear] about in/on the [media outlet]?

Often [high], sometimes, rarely, or never [low]?" Discussant version: "When you discuss politics with [named discussant, voluntary association, or workplace], do you disagree often [high], sometimes, rarely or never [low]?"

Closeness of Relationship

"Is [named discussant] a close friend, just a friend, or just someone that you regularly come into contact with?" Spouses and relatives were coded into a fourth category.

Frequency of Political Discussion

Discussant Version: "When you talk with [named discussant], do you discuss politics a lot, some, a little, or very rarely?"

Media Version: "When you read/watch [named media outlet], do you read/watch stories about politics a lot, some, a little, or not at all?"

Partisanship

"Generally speaking, do you consider yourself a Republican, Democrat, independent, or what?"

Political Interest

"Some people seem to follow what's going on in government and public affairs most of the time, whether there's an election going on or not. Others aren't that interested. Would you say you follow what's going on in government and public affairs most of the time, some of the time, only now and then, or hardly at all?"

Comfort with Controversy

"When you were growing up, about how often did your [parents or guardians] take the position that certain topics are better left undiscussed? Often, sometimes, rarely, or never?" Scoring: 1 = often . . . 4 = never.

"How often did they have spirited discussions on controversial matters like politics or religion? Often, sometimes, rarely, or never?" Scoring: 1 = never . . . 4 = often.

The scale based on these two measures ranged from 2 to 8.

APPENDIX B. CROSS-NATIONAL ELECTION PROJECT SURVEY: AMERICAN AND BRITISH COMPONENTS

Design

The American component of the CNEP combined a national survey with a survey of discussants named by main respondents and a content analysis of 40 newspapers conducted during the 1992 presidential election (for details and reliability of content measures, see Dalton, Beck, and Huckfeldt 1998; Dalton et al. 1998). The national survey was drawn from 40 county clusters around the United States (48 states), and the major newspaper read by residents in each of the counties was selected for content analysis. The British survey component was included as a module in the British General Election Cross-Section survey of 1992. Full details of the survey design and administration are given in Beck, Dalton, and Huckfeldt (1992) for the American survey, and Heath et al. (1992) for the British survey.

The newspaper sampling was done every third day and every Sunday from Labor Day to Election Day; a maximum of 27 days were coded for each paper, depending on its circulation pattern. On each coding day all stories that dealt directly with the presidential campaign and/or candidates and that appeared in the first half of the first news section were coded, as well as any special election pages and campaign-related stories on the editorial pages. Coders were instructed as follows: "Code the overall content of the article that involves Bush, Quayle, or the Bush/Quayle campaign in terms of its favorability or unfavorability to the Bush campaign. Evaluate the article from the perspective of the Bush campaign and assess the content of the article from this perspective. In other words, would the Bush campaign like seeing this article in print?" (Dalton, Beck, and Huckfeldt 1992). The same instructions applied for Clinton/Gore and for Perot. We aggregated across all articles in a newspaper to obtain an average favorability/unfavorability score for each candidate. Newspapers were coded as favoring a candidate if his score was higher than that of the other two candidates and the differences were statistically significant at the $p < .05$ level.

Discussant Generator

"Now let's shift our attention to another area. From time to time, most people discuss important matters with other people. Looking back over the last six months, I'd like to know the people you talked with about matters that are important to you. Can you think of anyone? What is this person's first name? Is there anyone else you talk with about matters that are important to you?" Up to four names are accepted, then: "Aside from anyone you have already mentioned, who is the person you talked with most about the events of the recent presidential election campaign?"

Perceived Disagreement with the Media

Respondents were asked: "In the presidential election, which one of the candidates did you prefer?" They also were asked: "Which presidential candidate do you think [the newspaper] favored during the campaign, or didn't it favor any candidate?" The two responses were compared, and scoring was as follows: 0 = absolute agreement (respondent and newspaper concur), 1 = mixed (either respondent or newspaper is independent/neutral), and 2 = disagreement (respondent and newspaper disagree). Television news magazine and talk show scoring was the same.

For the British sample, similar items were used with respect to the Conservative or Labour Party, and scoring was the same.

Perceived Disagreement with Discussant

A comparison was made between self-reported candidate support and the perceived support of Clinton, Bush, or Perot by the named discussants: "Which candidate do you think [discussant] supported in the presidential election this year?" Scoring: 0 = absolute agreement (respondent and discussant concur), 1 = mixed (either respondent or discussant is independent/neutral), and 2 = disagreement (respondent and discussant disagree).

A similar scale with respect to the Conservative and Labour parties was used for the British sample, and scoring was the same. To compare a single indicator of interpersonal exposure to disagreement, perceived disagreement was averaged across however many discussants a respondent named.

Independently Assessed Agreement/Disagreement

For newspapers, assessment is based on the candidate that the respondent supported and the favorability score for the relevant newspaper as determined by content analysis. Scoring: 0 = absolute agreement (respondent and newspaper concur), 1 = mixed (either respondent or newspaper is independent/neutral), and 2 = disagreement (respondent and newspaper disagree). For discussants, assessment is based on the respondent's self-report and the named discussants' self-reports, scored on the same three-point scale.

Accuracy, Projection, and Contrast

Accuracy, projection, and contrast were measured by the ability of main respondents to identify correctly the candidate supported by either their named discussants (as revealed in the discussant survey) or by their local newspaper (as revealed in content analysis). If the perceptions of the main respondent matched the independent assessment by coders or discussants, then they were scored as accurate. If the respondent judged the discussant or newspaper to be in more agreement than was the case, the respondent was scored as projecting. If the respondent judged the discussant or newspaper to be in less agreement than was the case, the respondent was scored as contrasting.

American Partisanship

(1) "Many people lean toward a particular political party for a long time, although they may occasionally vote for a different party. Do you generally lean toward a particular party?" (2) "If you had to choose, do you think of yourself as closer to the Republican or the Democratic party?" (3) "Taken altogether, how strongly or weakly do you lean toward the [Republican, Democratic] party? Would you say very strongly, fairly strongly, moderately, fairly weakly, or very weakly?"

British Partisanship

Form A: "Generally speaking, what do you think of yourself as [names of political parties]? Do you think of yourself as closer to one party? Which? How strong [name party] are you?" Form B: "Generally speaking, what do you think of yourself as?" "Which of the following parties do you feel a little closer to?" "How strong [name party] are you?"

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The Political Response to Black Insurgency: A Critical Test of Competing Theories of the State

RICHARD C. FORDING *University of Kentucky*

A lthough empirical studies have concluded that political leaders in democratic systems often respond to mass unrest by expanding the welfare state, most of this research fails to explain adequately why the state responds as it does. I test the validity of pluralist and social control theories of state response by examining black insurgency in the United States during the 1960s and 1970s. Using pooled time-series analysis, I estimate the relationship between state AFDC recipient rates, state incarceration rates, and black political violence, testing a series of specific hypotheses that distinguish between these two competing theories. The results lend much support to the social control characterization of state response and may help explain trends in welfare and criminal justice policies over the last two decades.

There has been considerable debate among social scientists concerning the role of violent protest in the politics and policies of modern democracies. Much of the discussion centers on the consequences of mass violence and, in particular, whether the state responds by providing benefits to the insurgent group. Contrary to classic pluralist accounts of group influence and public policymaking in modern democracies, most empirical work concludes that mass political violence often results in a favorable response from the state, usually in the form of welfare state expansion. This research includes studies of state response to unrest among the unemployed and labor during the Great Depression (e.g., Goldfield 1989; Jenkins and Brents 1989; Piven and Cloward 1971, 1977), to black violence during the 1960s and 1970s (e.g., Fording 1997; Hicks and Swank 1983; Isaac and Kelly 1981; Piven and Cloward 1971), and to labor insurgency in Western Europe (Swank 1983).

Despite the evidence of a relationship between mass insurgency and welfare state expansion, there is little information about the process by which this occurs or the state's motivation. A favorable response to unrest by the state might plausibly result from one of two processes. On the one hand, the state may respond favorably as a strategy to control unrest. For example, transfer payments might be increased to pacify the insurgents, thus resulting in demobilization and cessation of violence. This is often labeled the social control perspective and has been put forth by neomarxists to explain the development and evolution of the welfare state as well as the survival of capitalism (e.g., Offe 1974; Poulantzas 1973).

On the other hand, a somewhat modified form of pluralist theory views collective violence as a strategy employed by otherwise powerless groups to achieve access to the policymaking agenda. Presumably, they gain sympathy and support through increased visibility and then can effectively compete and bargain with other interests to obtain policy changes favorable to

their interests (e.g., Cobb, Ross, and Ross 1976; Lipsky 1970).

The two models describe very different processes by which social and political change occur. More important, perhaps, they suggest vastly different roles for the state in policymaking and depict contrasting power relationships between aggrieved groups and policymakers. The purpose of this research is to determine which of these processes most accurately characterizes the response to insurgency in the United States.

First, I specify more fully the alternative theories of state response and explain why the literature does not distinguish between them. Next, I develop a series of hypotheses that seek to determine which of the theoretical models is most accurate. I then present the results of an empirical analysis that distinguishes between these competing explanations by estimating the relationship among state rates for recipients of Aid to Families with Dependent Children (AFDC), state incarceration rates, and black political violence.

ALTERNATIVE MODELS OF STATE RESPONSE TO INSURGENCY

The Social Control Perspective

A key insight of social control theory is that influence may be exerted in one of two general ways.[1] First, behavior may be controlled by threats of negative sanctions in the event of noncompliance. Some theories refer to this as coercive control. Alternatively, influence may be exerted by offering incentives or rewards in exchange for compliance, which is often labeled beneficient control. In the context of mass insurgency, the social control perspective posits that the state, acting in the long-term interest of elites, will seek to minimize the effect of insurgency by increasing the level of social control, whether it be coercive, beneficent, or some combination of the two.

Although the most visible form of control is coercive, many theorists suggest that the state is not likely to rely

Richard C. Fording is Assistant Professor of Political Science, University of Kentucky, Lexington, KY 40506-0027.

[1] "Social control" is used across several fields in the social sciences. My use of the term most closely resembles that emerging from the conflict perspective, the theoretical origins of which can be traced to Marx. See Liska (1997) for a review of this literature.

FIGURE 1.   The Social Control Model of State Response to Insurgency
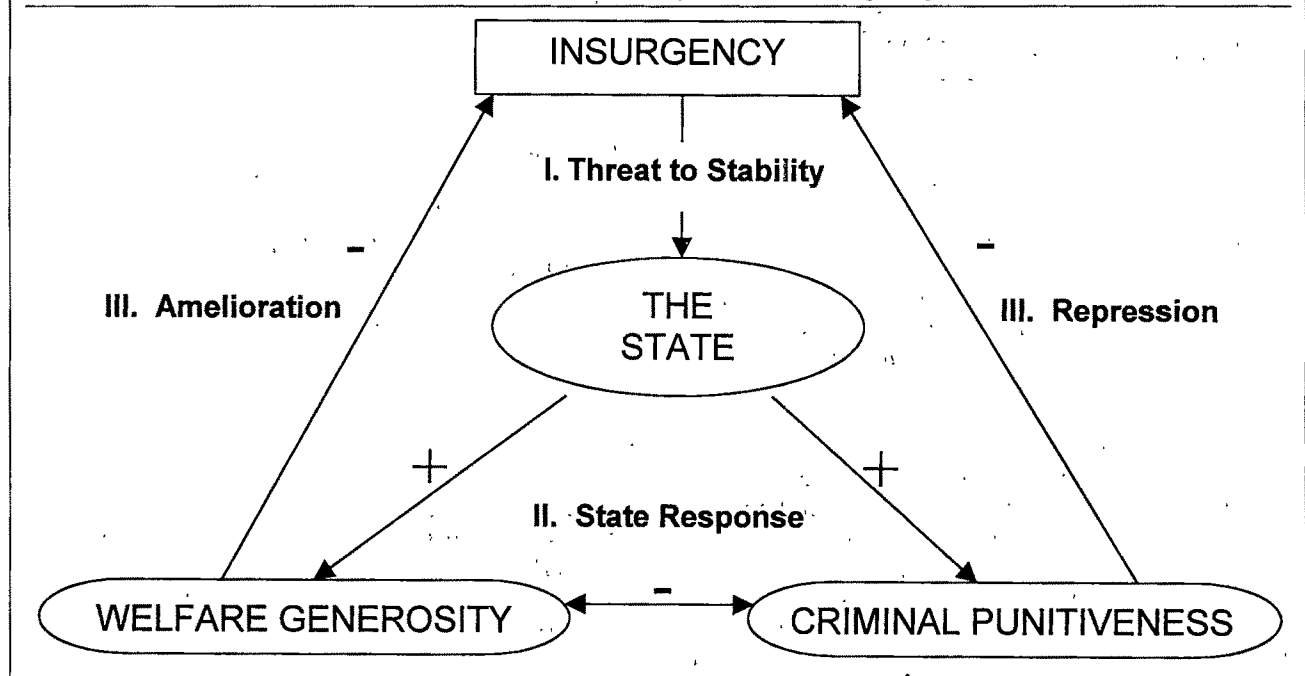
upon it exclusively in managing insurgency. One of the most controversial applications of this theory is put forth by Piven and Cloward (1971). Consistent with more general arguments of some neomarxists (e.g., Offe 1975; Poulantzas 1973), Piven and Cloward argue that an important means by which the state maintains political stability, and thus preserves capitalism, is through periodic expansion of the welfare state. More specifically, during periods of mass insurgency, the state expands the relief rolls. This serves the dual function of addressing the grievances of the poor and restoring legitimacy to the incumbent government. When disorder subsides, the state slowly contracts the welfare rolls to keep labor markets competitive. This historical dynamic, they argue, was manifested most recently by the "relief explosion" that began during the late 1960s. Piven and Cloward claim that it was due in large part to the threat posed by widespread black violence that started with a wave of riots in 1964, followed by the insurgency of the (mostly black) poor throughout the remainder of the decade.

The role of beneficent control in quelling insurrection is clear enough, but social control theories are often rather vague about the role of coercive control, or repression. Most scholars who address this subject suggest that the state will respond to social unrest by an increase in both beneficient and coercive controls. Piven and Cloward (1971) perceive a mixed strategy, even in the absence of democratic political institutions, in their historical account of European relief systems during the sixteenth century. Since "penalties alone" were not adequate to maintain order, "some localities began to augment punishment with provisions for the relief of the vagrant poor" (p. 8).

In developing a more general model of social control, Quinney (1974) explicitly recognizes the role of both coercive and social welfare bureaucracies in the control of threatening classes. When this model is extended to the case of black insurgency, it suggests that the response may be an expansion of welfare coupled with an increase in coercive control, as reflected by more punitive criminal justice policies. This process is represented graphically in Figure 1.

The Neopluralist Perspective

At the heart of the classic pluralist interpretation of democratic politics are three assumptions. First, the means by which groups may communicate preferences to leaders are effective and accessible to all groups should they wish to use them. Second, sufficient competition among all groups exists so that no single group has a policymaking monopoly. Third, policymakers are open to demands put forth by any group that gains access to the policy arena (e.g., Dahl 1961).

An important corollary of this model with respect to social movements is that the use of unconventional political tactics is thus unnecessary and even pathological. The plausibility of this conclusion, as well as the adequacy of the classic pluralist model more generally, is perhaps most clearly questioned by the emergence and success of the civil rights movement in the United States, which effectively challenged the pluralist assumption that available (legal) means of political participation are available to all groups. This case led to new approaches to explain the use of unconventional political strategies and the rise of social movements. Modifications of the pluralist model concerning the origins of protest have become known as the resource mobilization and political process approaches to the study of social movements (e.g., Jenkins 1985; McAdam 1982; McCarthy and Zald 1973; Tilly 1978). According to these neopluralist models, the infrequent and often unexpected occurrence of insurgency is due

to insufficient resources and opportunities for successful mobilization, which counters the classic pluralist position that insurgency is unnecessary.

The success of the civil rights movement in achieving its goals also led to a revision of the pluralist model regarding the consequences of protest. By the late 1960s, a number of modifications attempted to account for the rationality and effectiveness of unconventional politics but retained many of the model's core assumptions. Among the earliest and most influential of these was Lipsky's (1970) study of protest by the poor in New York. Consistent with neopluralist critiques, Lipsky saw the problem of powerless groups to be a lack of bargaining resources. The role of protest is to "activate third parties to enter the implicit or explicit bargaining arena in ways favorable to the protesters" (p. 2). The key to reaching these third parties, which Lipsky called "reference publics," is the mass media. Backed by the financial and organizational resources of reference publics, powerless groups may then be able to influence policy.

The role of protest, and more specifically that of political violence, has been incorporated into broader theoretical models of policymaking, which is clearly seen in the literature on agenda setting. Cobb, Ross, and Ross (1976) identify several stages of the agenda-building process, along with characteristics of policies and groups that lead to different strategies for achieving access to the formal agenda. One particular strategy of agenda building is likely, they argue, when the relevant group originates from outside the government structure. In their outside initiative model, access to the formal agenda is achieved indirectly through issue expansion and eventual inclusion on the public agenda. At that point, access to the formal agenda is relatively easy, due to the interest aroused in a larger number of voters, and policymakers are likely to be inclined to act. The most critical stage for groups pursuing this strategy is issue expansion: "In order to be successful in getting on the formal agenda, outside groups need to create sufficient pressure or interest to attract the attention of decision makers" (p. 128). A common tactic, particularly among groups that are large in number but have few financial resources, is through "violence and threats of violence" (p. 131).[2]

Similar attempts to specify the potential outcomes of insurgency abound in the more recent literature on social movements. Generally speaking, these theories predict that insurgency may or may not be successful, depending upon a number of movement characteristics and/or environmental factors. Like Lipsky (1970) and Cobb, Ross, and Ross (1976), many of these scholars cite the importance of third-party support (e.g., Jenkins and Perrow 1977; McAdam 1982). Other works cite such factors as the "strength" of insurgent forces (Tilly 1978), the nature of movement goals (e.g., Gamson 1975), and the ability of insurgent groups to combine violence with conventional modes of participation (Gamson 1975).

Despite differences, all these approaches share important features. First, the use of violence by aggrieved groups is assumed to be due to their inability to influence policymaking through conventional forms of participation. Second, success is thought to be contingent upon the nature of the political environment, most notably the level of public support for the insurgent group. Finally, and most important, although each of these models differs with the classic pluralist model regarding the utility of insurgency, all agree with it that the role of the state is to mediate among competing interests. In other words, although protest may be necessary for aggrieved groups to obtain access to the policy arena and the resources to compete effectively, once they reach the arena, the policymaking process is still a pluralist one. In the case of black insurgency, the specific implications are that any success in achieving issue expansion, gaining agenda access, and obtaining sufficient bargaining resources should have resulted in a favorable state response in one or more of the policy areas with which the insurgents were most concerned.

Black violence can be linked to a host of grievances, but two categories dominated the responses of insurgents to surveys conducted in the aftermath of major riots in 1967 (National Advisory Commission on Civil Disorders 1968). These were impoverishment in urban ghettos, including poor housing and unemployment, and harsh treatment by the criminal justice system. These concerns also were emphasized by protest leaders in negotiations with white officials after many riots and were reflected in press coverage of the events. Thus, to the extent that insurgency was successful, neopluralist theory predicts that it should have resulted in an expansion of the welfare state and a less punitive criminal justice system. This model is displayed in Figure 2.

Distinguishing between Models of Response

Despite the extensive empirical literature on the response to insurgency, work thus far has not distinguished between the models described above. This is due to the fact that the vast majority of these studies have only examined the relationship between black insurgency and welfare expansion. Most find that a relationship exists, but both the social control and the neopluralist theories predict such a relationship. Inherently, then, studies that examine a beneficent response alone cannot distinguish between the two models.

This problem does not exist with respect to research that examines the relationship between insurgency and criminal justice policy. Indeed, as can be seen by comparing figures 1 and 2, the competing theories suggest an opposite relationship between insurgency and criminal punitiveness. Very little research falls in this category, however. Welch (1975) examined city-level responses to riots across several local expenditure categories. Based on a sample of all cities with more than 50,000 in population, she found the number of riots to be positively related to expenditures for police and fire protection but unrelated to expenditures for public welfare. The only research on the federal response to insurgency is by Button (1978), who exam-

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[2] Similar models can be found in Cobb and Elder (1983) and especially in Baumgartner and Jones (1993), who specifically address the role of the 1960s riots in moving urban issues to the public agenda.

FIGURE 2.   The Neopluralist Model of State Response to Insurgency

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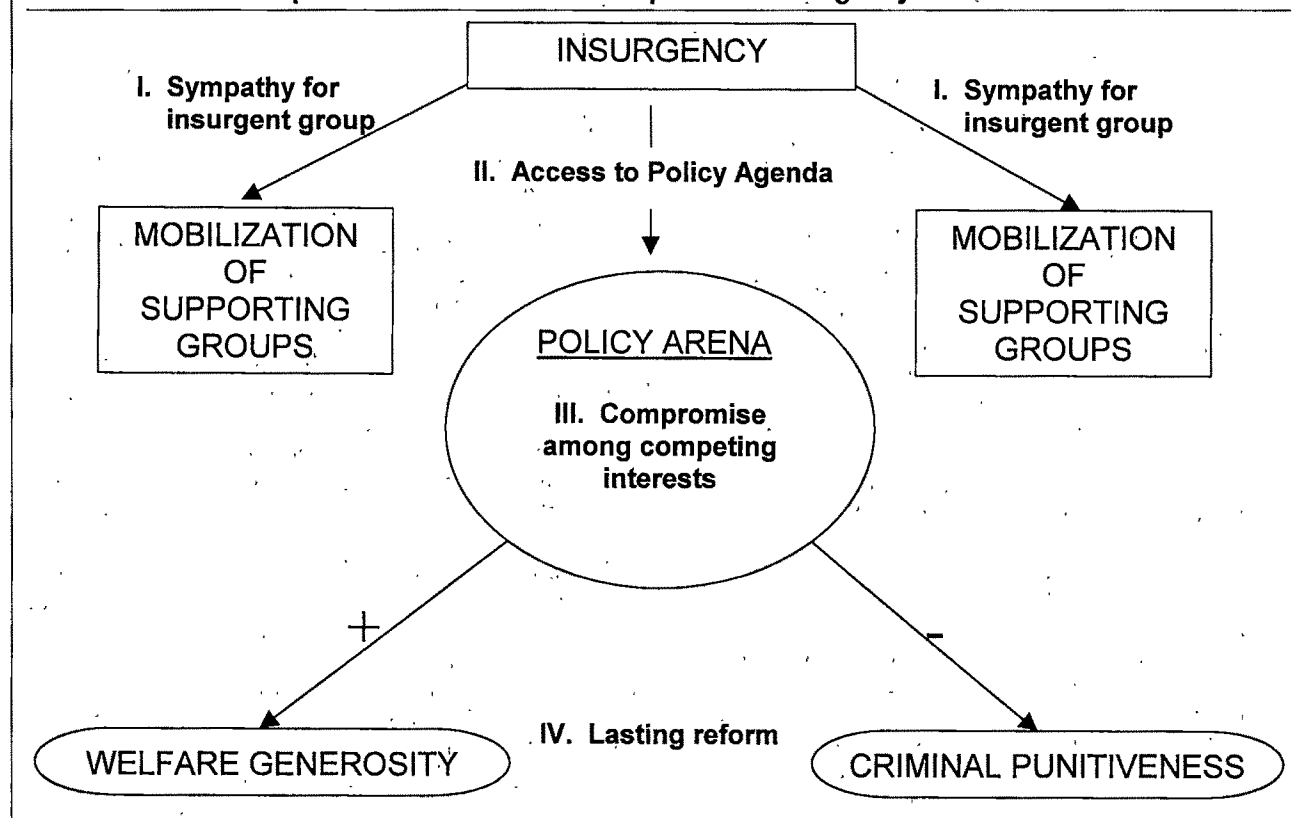
ined how urban riots affected the targeting of a variety of federal programs that represented both beneficent responses (Office of Equal Opportunity; Department of Health, Education, and Welfare; and Department of Housing and Urban Development) and coercive responses (Department of Justice). Based on data from 40 cities, Button found that urban riots were positively related to both types of responses, to which he referred respectively as the "carrot" and the "stick."

The only other study to examine both forms of response (Iris 1983) used interrupted time-series analysis for a sample of 35 large metropolitan areas to examine the effect of urban rioting on both AFDC and police expenditures. Like Button, Iris found that the response to disorder could be characterized as beneficent, because rioting was related to AFDC expansion. Unlike Welch and Button, however, Iris found no evidence of a coercive response, as rioting was unrelated to police expenditures. That finding is supported by two city-level studies that examine the law enforcement response to urban unrest (Jackson and Carroll 1981; Jacobs 1979). In sum, the paucity of research and the inconsistent findings provide little guidance as to which of the competing theories of state response may be correct.

In an effort to distinguish between the two models, I employ several modifications to the studies described above. Although I analyze the response to insurgency with respect to both welfare and criminal justice policy, I examine responses at the state rather than the city level. States are the most important actors in the

American federal system, at least with respect to these two policy areas. States are generally responsible for about half the welfare expenditures in the country. In the case of criminal justice, most arrests are made by local law enforcement agencies, but criminal courts are under state control, and nearly all convicted offenders are sent to state prisons. In addition, annual data for all states are available for nearly all the variables in this study, which is not the case at the city or county level. This permits a pooled cross-section time-series design, which has important inferential advantages over the designs used in previous studies. The period 1962–80 was chosen to include the entire span of black insurgency and yields an N of 912 cases.

Even with these modifications, it is possible that the findings across both types of response will be inconclusive. Consider the following. If a positive relationship were found between insurgency and welfare generosity, as empirical evidence leads us to believe, this would leave us with three possibilities. First, there might also be a positive relationship between insurgency and coercive control (criminal punitiveness), which would support the social control model. Second, there might be a negative relationship between insurgency and criminal punitiveness, which would be consistent with the neopluralist model, as it suggests that blacks were successful with respect to grievances in both policy areas. Third, despite a positive relationship between insurgency and welfare generosity, if insurgency and criminal punitiveness are unrelated,

we would still be unable to distinguish between the two theories. This third combination of findings would be consistent with a social control response and might be expected when elites fear the electoral costs of coercion are too great. Yet, this situation also would be consistent with a neopluralist model, as the lack of a coercive response could be interpreted as a victory for insurgent blacks.

In the third combination of relationships we would be able to distinguish between the two models by examining two additional hypotheses. The first rests on assumptions of the two models regarding the relationship between welfare and criminal justice policy. According to the social control model, both policies perform the same function, that is, social control. Assuming cost constraints and a fixed demand for social control, we should expect investment in these two areas to be negatively and reciprocally related (Inverarity and Grattet 1989; Liska 1997; Spitzer 1975). In other words, there should be a tradeoff between welfare and criminal justice expenditures as policymakers seek to maintain a desired level of social control at the lowest possible cost. In the neopluralist model, however, such a relationship should not exist. Policymakers are expected to approach each policy area separately, responding only to the preferences and resources of the groups in the arena. These alternative relationships are represented in figures 1 and 2.

The second distinguishing hypothesis concerns the expected duration of the various responses to insurgency. According to the neopluralist perspective, a favorable state response represents an exercise of power by the insurgent group in the bargaining arena, and therefore policy changes can be seen as genuine reform. If so, we should expect these changes to be relatively permanent, ceteris paribus. To use the language of Tilly (1978), the successful insurgent group, once a challenger from outside the polity, now becomes a member of the polity and henceforth has more or less direct access to the policy agenda. If the neopluralist model is correct, increases in welfare and decreases in criminal justice punitiveness should continue beyond the period of insurgency. In contrast, the social control model assumes that response to insurgency is driven by the need to reestablish order at the lowest possible cost. We should expect the state to reduce the overall level of social control investment once disorder subsides. Piven and Cloward (1971, 3) conclude with respect to a welfare response: "As turbulence subsides, the relief system contracts, expelling those who are needed to populate the labor market." Similarly, to the extent that the state responds with coercive controls, we should expect decreased investment in this strategy once disorder subsides.

AN EMPIRICAL MODEL OF STATE RESPONSE TO BLACK INSURGENCY

I rely on insurgency data originally reported in Fording (1997), where I defined insurgency as "any act of violence on behalf of blacks or minorities, either spontaneous or planned, which is either framed as or can be construed as politically motivated" (p. 11). The data cover collective acts of political violence (i.e.,

riots) as well as activities that involve fewer people, such as arson and sniping attacks, which were carried out by various black revolutionary groups. Violent acts are defined as rock throwing, vandalism, arson, looting, sniping, or beating of whites. Consistent with past research, insurgency is measured as the number of incidents. Data were collected for 923 events.[3]

A Model of State Welfare Generosity

Consistent with past research, welfare generosity is measured as the annual growth (change) in the number of AFDC recipients per one million state population in each state. This measure, as opposed to benefit levels or expenditures, is chosen due to the emphasis given this dimension of welfare policy by social control explanations (Piven and Cloward 1971). Although the social control approach does not preclude the possibility that insurgency may be related to AFDC benefit levels as well, Piven and Cloward argue that the effect on recipient rates should be strongest, since it is presumed that the primary source of insurgency, and thus the target of state control, is the unruly poor who are not currently receiving government aid.

Exogenous variables for this model are suggested in past research on AFDC growth and on the growth of social welfare programs generally. Broadly conceptualized, several of these approaches to explaining welfare policy comprise what Rochefort (1986) has termed the progressive perspective. Other explanations focus on changes in welfare programs originating at the federal level, the characteristics of state political institutions, and the extension and expansion of political rights for the poor.

The Progressive Perspective. Within this general approach, different theorists have stressed various factors, but all the explanations share the position that welfare policy is primarily driven by the benevolent motives of policymakers and the needs of poor. To the extent that at least some motivation is humanitarian, we should expect change in AFDC rolls to be related to the size of the needy population, as measured by the annual change in the number of female-headed households in poverty and by the annual change in the level of unemployment in a state (e.g., Fording 1997; Isaac and Kelly).

Theorists also have linked welfare expansion to increased capacity to afford social welfare and to the increase in social pathologies caused by urbanization (e.g., Wilensky and Lebeaux 1965). To capture these effects, I include *Per Capita Income* and *Per Capita State Revenue* as measures of economic capacity (each measured as the annual change) as well as the annual change in the level of urbanization in a state *(Urban-*

[3] Another form of insurgency potentially relevant here are the relatively disruptive yet nonviolent protests by various welfare rights groups, many organized under the leadership of the National Welfare Rights Organization. Because these protests were nonviolent, they did not receive much attention from the major news media, and they are not included in this analysis.

ization). Each of these variables is expected to be positively related to AFDC expansion.[4]

Federal Changes. According to some analysts, important changes in welfare policy, particularly the introduction of Medicaid in 1965 and the abolition of residency requirements by the Supreme Court in 1969, have altered the incentives of the poor and caused significant changes in AFDC participation (Durman 1973). To capture the effect of the Medicaid program, I include *Medicaid,* a dummy variable with the value of 1 in the year that the program was introduced in a state, 0 otherwise.[5] The effect of residency requirements is captured by *Residency Requirements,* a dichotomous variable that equals 1 in 1969 for states affected by the Court's decision, 0 for all other states and years.[6] Both variables are expected to be positively related to AFDC growth.[7]

State Political Institutions. Ideological differences between the major political parties as well as the level of competition between them are accounted for by *Democratic Control,* a variable measuring Democratic control of state government (e.g., Dye 1984; Erikson, Wright, and McIver 1993), and *Interparty Competition,* which measures the annual change in interparty competition (Key 1949). The ideology of state electorates (measured as the change in ideology), which is presumed to influence the decisions of state policymakers (Erikson, Wright, and McIver 1993), is captured by *Ideology,* a measure of liberalism constructed by Berry et al. (1998). Each of these variables is hypothesized to affect AFDC growth positively.

Political Citizenship. Some theorists suggest that welfare expansion is associated with periodic expansion of "citizenship," that is, the evolution and extension of social and political rights (e.g., Gronbjerg 1977). One of the most important periods in this regard was the

1960s, and two dimensions of this expansion are incorporated into the analysis.

First, due to significant changes in voting qualifications that affected both blacks and whites (e.g., abolition of poll tax), masses of poor people were effectively enfranchised for the first time during the 1960s, which presumably altered the class composition of the electorate. Therefore, *Class Bias* is adopted from Husted and Kenny (1997), a measure that captures the expansion of the electorate during the 1960s as well as variation in the relative participation of the poor over the period of this analysis. Because it reflects the degree to which the poor are underrepresented in the electorate, class bias (measured as the annual change) is expected to have a negative effect on AFDC expansion.

Second, important court decisions ended decades (in some cases) of extreme malapportionment and gave densely populated poorer districts more power, which may have contributed to welfare expansion. I include *Reapportionment* and model this process as an intervention, that is, the variable has a nonzero value only for the first year after the first significant reapportionment was implemented in a state.[8] The variable is not dichotomous, however, because the intervention value represents the magnitude of malapportionment (alternatively, the extent of reapportionment) that existed previously. Reapportionment is expected to result in greater representation for the urban poor, so it is expected to be positively related to AFDC growth.

A Model of State Criminal Punitiveness

The criminal justice response to insurgency is hypothesized to be reflected by the change in incarceration rates and is measured as the annual change in the number of prisoners in state prisons per one million population. In addition to black insurgency, several other variables are hypothesized to influence growth in state incarceration rates.

Criminal Involvement. Many criminologists cite the stability of punishment thesis, that is, incarceration rates remain relatively stable over time and change in response to society's tolerance for crime, rather than in response to crime itself. Several studies have found crime to be an important determinant of incarceration rates (e.g., Carroll and Cornell 1985; Garofalo 1980), but others have found little if any relationship (e.g., Carrol and Doubet 1983; Joubert, Picou, and McIntosh 1981). Given these mixed findings, I include *Crime,* measured as the annual change in crime rates, which is expected to be positively related to incarceration.

Economic Characteristics. Rusche and Kirchheimer (1939) argue that incarceration rises with unemployment due to the potential threat to the social order from the unemployed. The issue of threat aside, Cappel and Sykes (1991) point out that employment status

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[4] For specific sources and details about how all the variables in this analysis were constructed, see the Appendix.

[5] In an earlier analysis (Fording 1997), I used the annual change in the Medicaid recipient rate to represent the effect of Medicaid on AFDC expansion. Although I found a significant relationship between the two, we might alternatively expect AFDC expansion to cause an increase in the Medicaid recipient rate, which suggests that the causal arrow runs in the opposite direction. To determine which is the case, I conducted Granger causality tests. The results provide strong evidence that increases in the Medicaid recipient rate are caused by AFDC expansion ($F = 21.56, p = .000$ at five lags). I use a dummy variable representing the introduction of the Medicaid program to avoid potential simultaneity bias.

[6] Since the dependent variable is measured as the *change* in AFDC recipient rates, I code this variable (as well as the Medicaid variable) as 1 in the year of the intervention (i.e., when a residency requirement was dropped, or when the Medicaid program was introduced), 0 in all other years. This is equivalent to modeling an intercept change in the level of the dependent variable.

[7] One of the anonymous reviewers suggested that federal court decisions handed down in 1967 that increased access to welfare for the black poor in the South could be an additional source of the "relief explosion." Yet, inclusion of a dummy variable for 1967 to control for this factor failed to reach statistical significance ($t = 1.14$) and did not significantly affect any of the results reported below. As this variable contributes to multicolinearity problems, I do not include it in the models estimated in tables 1 and 2.

[8] Because the dependent variable is measured as the change in AFDC, this is equivalent to modeling an intercept change in the level of the dependent variable.

is an important determinant of sentencing outcomes because it indicates the likelihood that the defendant will continue to commit crime. Based on both of these hypotheses, the annual change in a state's unemployment rate is included in the incarceration model. *Unemployment* is expected to be positively related to incarceration.

In similar vein, many researchers posit a direct relationship between level of poverty and incarceration (e.g., Taggert and Winn 1993). Consistent with the threat hypothesis for unemployment, states with a higher level of inequality might be subject to greater potential threat from the lower classes, which could result in a greater level of incarceration. As with unemployment, however, an alternative explanation can be given for a positive relationship between inequality and incarceration. Defendants with sufficient financial resources can purchase competent defense and can sustain numerous appeals upon conviction, which increases the probability that they will not be incarcerated for very long, if at all. Consistent with other studies, I include *Poverty*, measured as the annual change in state poverty rates, as a measure of inequality.[9]

Finally, as noted earlier, two indicators of a state's economic capacity, per capita state income and per capita state revenue, are included. The direction of their effect on incarceration is not entirely clear. On the one hand, wealthy states may be able to afford more prisons, so economic prosperity should be positively related to incarceration. On the other hand, for the same reasons we expect poverty to be positively related to incarceration, we might expect per capita state income and revenue to be negatively related to incarceration.

State Political Institutions. As with welfare policies, several features of state political institutions and ideology are hypothesized to affect state incarceration rates. Relatively liberal voters, Democratic control of state government, competitive party systems, reapportionment, and extensive political participation by the poor and blacks are all expected to contribute to more lenient sentencing and parole policies, more alternatives to imprisonment, and hence lower incarceration rates, ceteris paribus. Although political variables have not been incorporated into many studies of incarceration rates in the past, recent research finds that political forces are an important determinant (e.g., Taggert and Winn 1993).

Military Mobilization Rates. Many studies have noted a negative correlation between military participation rates and incarceration rates. This is not surprising, given a presumed indirect influence of military participation rates through some of the variables discussed above (e.g., unemployment and poverty). A direct relationship also may exist; Inverarity and Grattet (1989, 357) note the "legal folklore" that, during times

of war, "judges commonly offer young miscreants a choice between jail and enlistment." In any case, a direct relationship is supported to some extent by empirical research that controls for some of the variables (but not all) through which military mobilization might influence incarceration indirectly (e.g., Cappell and Sykes 1991). Based on this possibility, I include *Military*, measured as the annual change in the number of individuals in the state in the armed forces (active duty).

The Conditional Effect of Insurgency

The strength of the relationship between insurgency and welfare depends on two important contextual factors (Fording 1997). First, presumably due to electoral incentives of policymakers, a welfare response only occurs when the insurgent group achieves effective electoral access (defined as both voting right *and* electoral systems that are not malapportioned). Second, as suggested by Keech (1968), during periods of effective electoral access, the influence of insurgency on AFDC growth is strongest in states with a relatively small black population (presumably due to a low level of white resistance) and in states with a relatively large black population (presumably due to black electoral influence). In other words, the insurgency effect is curvilinear (U-shaped) over the black population range of values and is weakest in the middle range (Fording 1997, 21). Based on this finding, a similar interactive specification is applied to the welfare model in this research.[10]

With respect to insurgency and incarceration, two curvilinear interactions are possible in the context of black electoral access. In a neopluralist model, a generally negative relationship would be expected between insurgency and incarceration across the range of black population size, as this is representative of a beneficient response. Consistent with the pattern of response found for AFDC, however, we would expect states in the middle range of black population size to show a relatively weak beneficient response, as blacks in these states simultaneously experience a relatively high level of white resistance and a low level of black electoral strength (Keech 1968). In a social control model, however, a generally positive relationship would be expected between insurgency and incarceration, as this is representative of a coercive response. Moreover, in this case the magnitude of the relationship should be strongest in the middle range, again due to the (presumed) combination of white resistance and weak black electoral influence.

For each dependent variable, the conditional relationship is incorporated by estimating a model in which insurgency is hypothesized to interact with black electoral access and the size of the black population. To capture the effect of black electoral access, I adopt a dichotomous measure from Fording (1997), which I label *Power*. It has a value of 1 for a state in which the following two conditions are satisfied for all years: (1) blacks have effective voting rights (based on implementation of the Voting Rights Act of 1965 in certain

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[9] Although poverty rates are not perfect measures of income inequality, better measures (such as Gini coefficients) are not readily available for the period analyzed here. Recently, Langer (1999) computed state-level Gini coefficients beginning in 1976 and reports sufficient correlation with state poverty rates to provide some confidence that poverty rates are a reasonable surrogate (>.70).

[10] For a thorough derivation of this specification in the context of a welfare model, see Fording 1997, 25–6.

states), and (2) electoral districts are not malapportioned (for states in which more than 90% of blacks resided in heavily populated urban areas). Otherwise, its value is 0. This implicitly assumes that both dimensions of access are essential to black interests being translated through electoral mechanisms.

Additional Hypotheses

The Welfare-Incarceration Tradeoff. As discussed previously, a crucial distinguishing feature of the social control model is the welfare-incarceration tradeoff. Although the model seems to rest heavily on this hypothesis (e.g., Liska 1997; Spitzer 1975), few empirical studies have tested it. In what appears to be the only research of its kind, Inverarity and Grattet (1989) used time-series analysis at the national level to test for a tradeoff between incarceration rates and the number of AFDC recipients, but they found no relationship. Aggregation is a potential source of problems in their analysis, however, and they failed to use an estimation technique that explicitly allows for reciprocal causation. Both problems are avoided in the present research, which posits a direct contemporaneous relationship between AFDC and incarceration. If the social control model is correct, the tradeoff should lead to a negative relationship between the two.

The Durability of Response. Another distinguishing hypothesis necessitates modeling both the short- and long-term effects of insurgency. Here it is important to consider the way in which the dependent variables, AFDC and incarceration, as well as insurgency are measured. The dependent variables are measured as changes (first-differences), whereas insurgency is measured simply as the number of incidents of political violence. Therefore, if insurgency is found to have an effect on either dependent variable for a particular lag length, the effect is assumed to result in a permanent increase in the level of that variable; in the language of time-series analysis, an intercept change in the level of the dependent variable occurs. This is consistent with the notion of lasting reform posited by the neopluralist model but inconsistent with the social control model, which suggests that welfare and incarceration levels will decline once disorder subsides. In order to test both models simultaneously, it is necessary to include multiple lags of insurgency. According to social control theory, a significant positive effect (for either dependent variable) eventually will be followed by a negative effect as the state readjusts the level of assistance/incarceration necessary to achieve social control. To accomplish this, I examine the effect of insurgency for up to seven lags. Lags lengths for other variables are determined by theory or, when theory is ambiguous, empirically (*t*-values). The entire model to be estimated, along with hypothesized relationships for both the welfare model and the incarceration model, is presented in Figure 3.

ESTIMATION AND RESULTS

The framework for this analysis is a pooled cross-sectional time-series design. Due to the hypothesized

reciprocal effect between AFDC and incarceration, I employ two-stage least-squares (2SLS) to generate coefficient estimates. To deal with likely violations of error assumptions, I follow Beck and Katz (1995), who recommend calculating standard errors (panel corrected standard errors, or PCSEs) that are consistent in the presence of heteroskedasticity and spatial autocorrelation. To handle serial correlation, they recommend a lagged dependent variable (LDV) rather than a traditional generalized least squares (GLS) correction. As they argue, the LDV approach explicitly brings the dynamics into the model and, based on Monte Carlo tests, appears to provide more efficient parameter estimates (compared to GLS) for typical pooled data sets. Thus, the final estimation strategy used here integrates 2SLS with PCSEs.[11]

To arrive at the final set of results, I engaged in three rounds of estimation. The first used an iterative process to determine the proper lags for the effect of insurgency, beginning with a strictly additive model that contained lags of up to seven years, and the lags that exhibited the strongest effects were retained.[12] The second round introduced interaction terms for the insurgency variables, dropped any interactions that did not appear significant (based on joint *F*-tests) or theoretically plausible, and reestimated. These results are presented as model 1 in tables 1 and 2 for both AFDC and incarceration. Based on these results, the third round estimated a final parsimonious model, retaining all variables that exhibited both the correct sign and a *t*-value of at least 1.0 in absolute value. These final results are presented as model 2 in tables 1 and 2.

Explaining State Welfare Generosity

Many scholars refer to the late 1960s as the "relief explosion," due to the innovation and expansion that occurred across a variety of public assistance programs, especially AFDC. What was responsible for this massive increase in welfare participation? The answer may be at least partially provided in Table 1, which displays regression results for AFDC. Consistent with my earlier analysis (Fording 1997), unemployment, residency requirements, and state ideology were all significant factors. In addition, urbanization and female poverty are positively related to AFDC growth. My earlier analysis failed to account for urbanization, and the significance of female poverty (which was insignificant earlier) is likely explained by the improved specification of the model.

The influence of insurgency on AFDC growth is strong and appears to be conditional upon electoral access and the size of the black population. This can be seen from examining the coefficients for the first set of insurgency variables in model 2 of Table 1 (i.e., where

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[11] As Figure 2 indicates, the model is clearly overidentified, assuming the hypothesized relationships between the exogenous variables unique to each model and the endogenous variables do in fact exist. Identification is also significantly aided (at least for the AFDC model) by the inclusion of state dummy variables in the incarceration model.

[12] It is possible to include all the interactive terms up to seven lags in one model, but it would be unwieldy and likely suffer crippling multicolinearity problems.

FIGURE 3. Hypothesized Relationships among AFDC, Incarceration, and Independent Variables
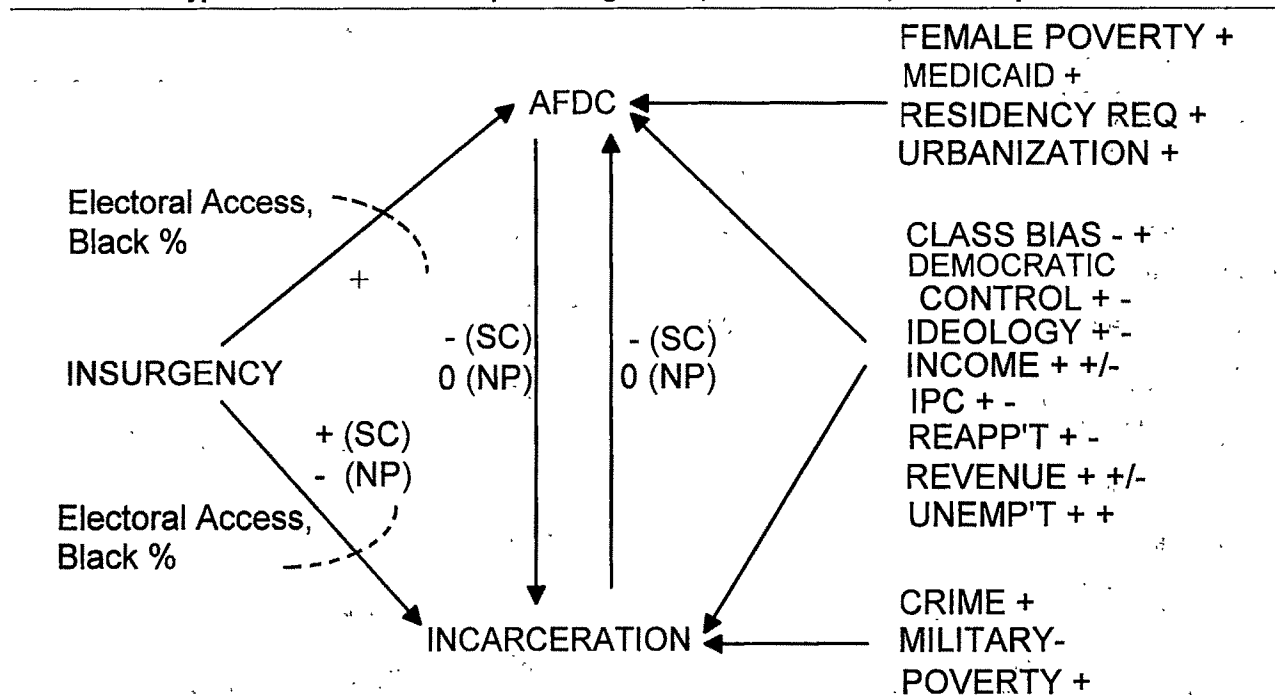
Note: Dashed lines denote conditional relationships. Hypothesized signs of relationships are given for each independent variable. For exogenous variables (other than insurgency) hypothesized to affect both AFDC and incarceration, the two signs listed denote hypothesized effects on AFDC and incarceration, respectively. For relationships serving as critical tests of the social control (SC) and neopluralist (NP) models, hypothesized signs are listed for each of these competing theories.

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insurgency is lagged two years). As in my 1997 analysis, the effect of insurgency is near zero when blacks did not have electoral access. Once they achieved electoral power, however, the effect indeed appears to be strong and varies significantly across the black population range in a curvilinear manner. Using slope coefficient estimates from Table 1, and setting the contextual variables of power and black to the desired values, the effect of insurgency when access has been achieved can be estimated for the range of values of the black population observed throughout the 1962–80 period.

These estimated effects, depicted graphically in Figure 4, generally reflect the curvilinear pattern reported in my earlier analysis (Fording 1997). At a two-year lag and in states where blacks comprise less than 3% of the population, each incident of insurgency is predicted to cause an increase in AFDC growth of 250–300 recipients per million population. When the black population is larger, the effect of insurgency diminishes, although not as significantly as my original results suggest. In states in which at least 30% of the population is black, each episode of insurgency is estimated to produce an increase in AFDC growth of at least 500 recipients per million population.

Explaining State Criminal Punitiveness

The results for the incarceration model are presented in Table 2, which reveals that the strongest effects originate from state economic conditions. Unemployment and poverty both exhibit relatively strong positive relation-

ships with incarceration rates, whereas higher state revenue is associated with a reduction. As with welfare generosity, the influence of political variables is significant but not especially striking. Both Democratic control and interparty competition are negatively related to incarceration, which is consistent with hypotheses that liberal control of political institutions or competitive party systems in the state produces more liberal policies. The other political variables—ideology, reapportionment, and class bias—all proved insignificant.

The effect of insurgency on incarceration was estimated in the same manner as for AFDC, by an iterative procedure that isolated the potentially significant lags and then determined the significance of the various interactions. The results clearly demonstrated that the effect of insurgency is not conditional as hypothesized, so an additive specification was used to generate the final results presented in Table 2. This is not to say that insurgency is unrelated to incarceration, however, as can be seen by examining the coefficient value for the two-year lag of insurgency, which indicates a positive and significant relationship. According to the results, each act of insurgency is predicted to increase incarceration by about 23 prisoners (per million population), holding other variables constant.

These results have two important theoretical implications. First, they provide support for the social control theory of state response, in particular a model in which the state increases both coercive and beneficent types of controls when threatened. Second, the

TABLE 1. Two-Stage Least-Squares Regression Results for AFDC Growth

Independent Variable	Model 1		Model 2		
	β	PCSE	β	PCSE	β^*
Progressive Perspective					
Female poverty$_{i,t}$	471.97**	190.00	491.13**	181.45	.08
Unemployment$_{i,t}$	861.96**	137.82	842.43**	138.87	.28
Revenue$_{i,t-1}$	−3,652.96	8,962.03	—	—	—
Income$_{i,t-1}$	−.04	1.29	—	—	—
Urbanization$_{i,t-1}$	193.30**	54.05	212.19**	56.41	.12
Federal Changes					
Medicaid$_t$	−125.13	385.83	—	—	—
Residency requirement$_{i,t}$	1,639.53*	831.89	1,863.59*	814.39	.09
State Institutions					
Democratic control$_{i,t-1}$	−282.69	462.28	—	—	—
Interparty competition$_{i,t-1}$	8.27	20.71	—	—	—
Ideology$_{i,t-1}$	44.93**	17.99	42.62*	18.90	.08
Political Citizenship					
Reapportionment$_{i,t}$	42.93	109.44	—	—	—
Class bias$_{i,t-3}$	−1,154.66	1,463.94	—	—	—
Insurgency: Lag of Two Years					
Insurgency$_{i,t-2}$	−45.16	240.40	−25.62	241.99	—
Power$_{i,t}$	342.81	531.60	668.80	486.34	—
Insurgency$_{i,t-2}$ × Power$_{i,t}$	352.93	266.53	321.16	267.49	—
Insurgency$_{i,t-2}$ × Power$_{i,t}$ × Black$_{i,t}$	−20.92	14.68	−19.58	15.02	—
Insurgency$_{i,t-2}$ × Power$_{i,t}$ × Black$^2_{i,t}$	.94**	.36	.89*	.38	.29/.21
Insurgency: Lag of Five Years					
Insurgency$_{i,t-5}$	332.87	250.90	356.25	252.60	—
Power$_{i,t-3}$	−1,042.09*	441.32	−1,200.81**	425.15	—
Insurgency$_{i,t-5}$ × Power$_{i,t-3}$	−567.47*	278.18	−743.28**	294.04	—
Insurgency$_{i,t-5}$ × Power$_{i,t-3}$ × Black$_{i,t}$	15.73	14.95	—	—	—
Insurgency$_{i,t-5}$ × Power$_{i,t-3}$ × Black$^2_{i,t}$	−.19	.38	—	—	—
Insurgency$_{i,t-5}$ × ln(Black)$_{i,t}$	—	—	125.73	67.34	−.37/−.06
Welfare-Incarceration Tradeoff					
Incarceration$_{i,t}$	−1.14**	.37	−1.09**	.35	−.29
AFDC$_{i,t-1}$	.30**	.06	0.29**	.06	.30
Constant	750.51*	369.17	668.65*	321.09	—
Number of cases	912		912		
Adjusted R^2	.43		.44		

Note: Column entries are unstandardized slope estimates (β), PCSEs, and for model 2, standardized slope estimates (β^*). The standardized effects reported for insurgency are calculated by setting the variable *Black* (percentage black) at the values represented by the 25th and 75th percentiles (1.44% and 13.78%, respectively, based on the full sample). A joint *F*-test indicated that the inclusion of unit effects (i.e., state dummies) was not warranted. An LM (language modifier) test demonstrates that the inclusion of the lagged dependent variable sufficiently eliminates serial correlation. All estimates were generated by RATS (regression analyses of time series), using a two-stage least-squares PCSE procedure written by Nathaniel Beck. Significance tests are two-tailed for insurgency variables, one-tailed for all other variables. *$p < .05$, **$p < .01$.

insignificance of insurgency for welfare generosity when blacks do not have electoral access suggests that, in the absence of electoral power, insurgency is likely to receive only a coercive response from the state.

Additional Hypotheses

Despite the initial support for the social control model, it is useful to examine hypotheses concerning other aspects of state response that help distinguish between the neopluralist and social control models. The first concerns the durability or permanency of the response. If the direct (positive) effect of insurgency on AFDC growth is relatively permanent, then the lasting reform posited by the neopluralist model is supported, and the findings concerning incarceration are contradicted. An

additive model of AFDC growth was estimated with a series of insurgency variables lagged up to seven years.[13] The values of the coefficients obtained from this regression display a relatively clear pattern over time. The initial response to insurgency is positive, with the first significant coefficient value seen at a lag of two years. Beyond this point, the coefficient value becomes negative, although only significantly so at a lag of five years, that is, three years after the initial positive response.[14] A similar pattern can be observed

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[13] Additive models were used in these diagnostic regressions because seven lags of interaction terms are extremely cumbersome, and the multicolinearity problem would be severe.

[14] For the effect of insurgency on AFDC, the coefficient values for seven lags were −34.3, 247.4*, −21.0, 7.7, −83.1*, 9.3, −53.8 (*$p < .05$). For incarceration, they were 17.9*, 7.0, −13.8, −11.5, 4.2, 5.6,

TABLE 2.   Two-Stage Least-Squares Regression Results for Incarceration

Independent Variable	Model 1 β	Model 1 PCSE	Model 2 β	Model 2 PCSE	β*
Crime$_{i,t-1}$	.20	.14	.26*	.13	.10
Unemployment$_{i,t}$	156.64**	36.73	159.29**	37.35	.20
Poverty$_{i,t}$	483.80**	94.51	491.63**	90.13	.32
Revenue$_{i,t-1}$	−4,071.37*	2,405.72	−4,259.44*	2,410.08	−.06
Income$_{i,t-1}$	−.36	.37	—	—	—
Democratic control$_{i,t-1}$	−234.38*	124.49	−221.33*	126.59	−.07
Interparty competition$_{i,t-1}$	−13.28*	6.00	−13.77**	6.13	−.09
Ideology$_{i,t-1}$	9.09	5.48	—	—	—
Reapportionment$_{i,t}$	−.27	26.49	—	—	—
Class bias$_{i,t-3}$	127.54	359.25	—	—	—
Military$_{i,t}$	−.09	.23	—	—	—
Insurgency$_{i,t-2}$	23.92**	8.69	24.31**	9.20	.10
Insurgency$_{i,t-3}$	−16.03	8.92	−16.85	9.41	−.07
AFDC$_{i,t}$	−.06**	.02	−.06**	.02	−.24
Constant	642.34	427.57	621.35	426.48	—
Number of cases	912		912		
Adjusted R^2	.20		.20		

Note: Column entries are unstandardized slope estimates (β), PCSEs, and for model 2, standardized slope estimates (β*). A joint *F*-test indicated that the inclusion of unit effects (i.e., state dummies) was necessary for both first- and second-stage regressions (results not reported). Diagnostic tests reveal an absence of serial correlation, which is reflected by the insignificance of the coefficient for a lagged dependent variable when included in the model (results not reported). All estimates were generated by RATS, Version 4.2, using a two-stage least-squares PCSE procedure written by Nathaniel Beck. Significance tests are two-tailed for insurgency, one-tailed for all other variables. *$p < .05$, **$p < .01$.

for the effect of insurgency on incarceration. In Table 2, we see that the positive effect of insurgency at a lag of two years is matched by a negative effect at a lag of three years.[15] This temporal pattern across the two dimensions of response provides additional support for the social control model: The levels of social control increase in response to insurgency and then decline when disorder subsides.

Returning to the dynamics of the welfare response, since it was found that AFDC expansion is contingent upon electoral considerations represented by size of the black population, we might expect the extent of AFDC contraction to be conditional upon the electoral context as well. Based on the coefficient estimates in model 2 of Table 1, this appears to be the case to some degree.[16] The dashed line in Figure 4 displays the predicted effect of insurgency at a lag of five years across the range of values for the black population. As we can see, the picture that emerges here is somewhat different from that for AFDC expansion with the two-year lag. At lower levels of black population, the AFDC rolls appear to contract somewhat after order is

restored, which does not appear to be the case at higher population levels. It seems that black electoral strength helps protect concessions made during disorder from being lost over time.

Thus far, the results across the two dimensions of response provide strong support for the social control model. Further support would be gained if it were found that AFDC and incarceration are reciprocally related, indicating that the two policy dimensions at least partly serve the same function of social control. This hypothesis is supported in tables 1 and 2, where coefficient estimates for the effect of incarceration on AFDC and for the effect of AFDC on incarceration are both negative and significant.

The Cumulative Effect of Insurgency

In combination with the direct influence of insurgency and the conditional nature of both the long- and short-term effects, a welfare-incarceration tradeoff would suggest that the overall effect of insurgency may be quite complex. As the cumulative effect of insurgency is not easily discernible from tables 1 and 2, I present Figure 5, which displays predicted levels of AFDC recipient rates and incarceration rates for two hypothetical states. To generate these estimates, predicted values for changes in AFDC and incarceration were first calculated, holding the values of all exogenous noninsurgency variables constant at representative values, which allowed the total effect of insurgency (direct and indirect effects combined) to be isolated. These predicted change values (i.e., first differences) were then converted to levels for ease of interpretation

1.8 (*$p < .05$). Moving to the reduced model (models 1 and 2, as reported in Table 2), however, a lag of two years proved to be significant, whereas a lag of one year did not.

[15] The coefficient estimate for the lag of insurgency at three years is not quite significant at the .05 level ($p = .06$), but given the inherent multicolinearity in distributed lag models, I treat it as such.

[16] In estimating the interactive effect of insurgency in the case of AFDC contraction, the results in Table 1 (model 1) indicate that coefficient values for the interaction terms are not significant. As this result may be due to multicolinearity, and given the shape of the curve in Figure 4 representing a lag of two years, I used the natural log of black population, rather than black population and its square, to model the conditional effect of insurgency in model 2. This specification shows a larger effect ($p = .07$).

FIGURE 4.   Predicted Effect of Insurgency on AFDC Growth, by Percentage of State Population that Is Black, at Two-Year and Five-Year Lags (for States in which Electoral Access Has Been Achieved)
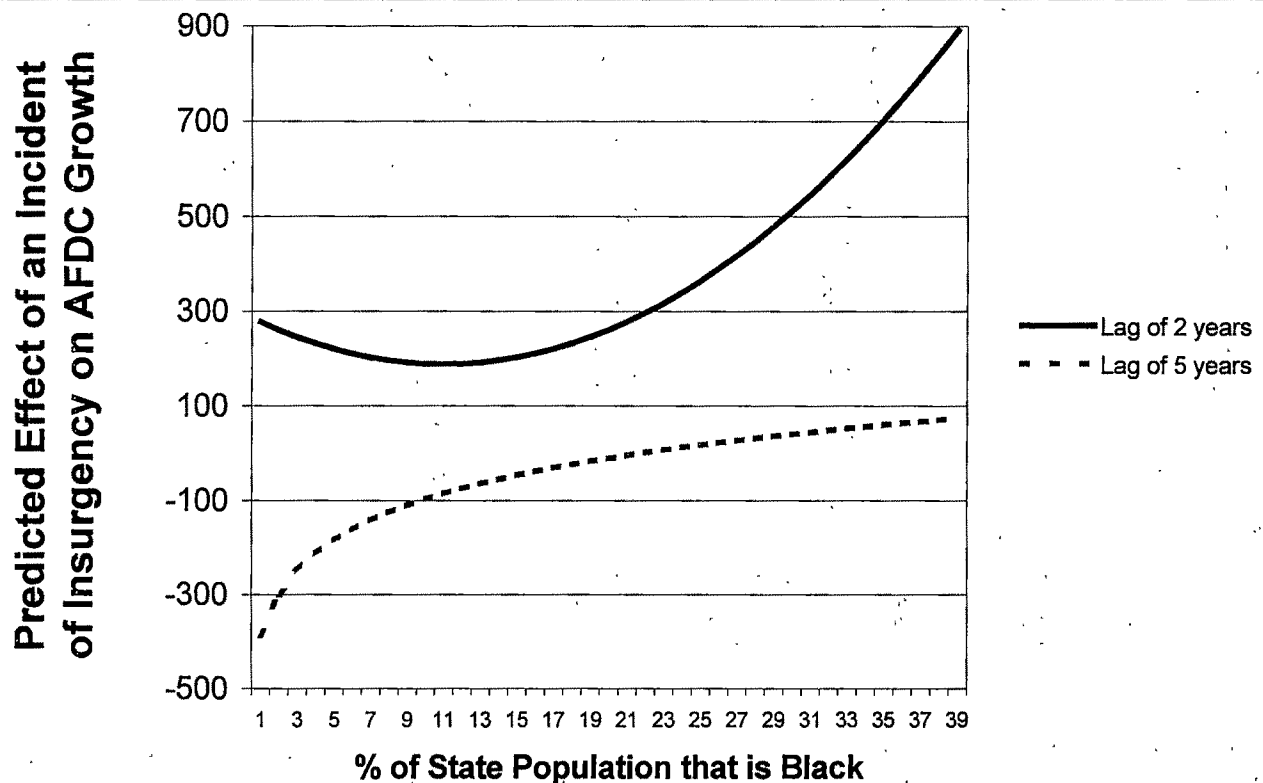
and plotted in Figure 5.[17] The hypothetical state in panel A experienced relatively little black violence (six incidents) during 1967–68, and black population size was rather small (3%). States that fit this description include Colorado, Iowa, Kansas, Massachusetts, and Nebraska. In this case the initial response to insurgency is heavier reliance on beneficent control, but eventually both AFDC and incarceration return to levels close to those before unrest (holding other variables constant).

A much different picture emerges for the hypothetical state in panel B of Figure 5, where there was considerable black insurgency (23 incidents), and black population size was large (30%). In this category are most of the states in the Deep South. Due to the relatively strong expansive effect of insurgency on welfare generosity, coupled with the durability of this policy in the years following unrest, the long-term influence of insurgency is quite significant. There is a relatively permanent increase in AFDC levels, and the predicted tradeoff between social control strategies contributes to a relatively permanent decrease in the level of incarceration.

Figure 5 depicts two very different scenarios, and we are left to wonder whether and how these effects might have combined into a national picture over the same period. This can be seen in Figure 6, which plots observed AFDC recipients rates, incarceration rates, and levels of insurgency during 1962–80. There is

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[17] Predicted AFDC and incarceration changes were converted by assuming representative levels for the initial year of each series.

evidence of the pronounced inverse relationship between AFDC and incarceration found in the state-level analyses presented above. In addition, the national trends resemble the pattern in Figure 5A more than the pattern in Figure 5B. This is not surprising, because the values of black population size and the level of violence used to calculate panel A are more typical across states than those used to generate panel B.

CONCLUSION

Although a relationship between mass unrest and welfare expansion has been established in several contexts, relatively little effort has been applied by social scientists to uncover the causal mechanisms. This research indicates that the social control perspective may provide the most valid explanation for this relationship in the case of black insurgency. Further work is necessary to determine whether these findings are applicable to other democratic systems, but this analysis may help explain important policy developments both past and present within the United States.

A widely debated historical question concerns the motivation behind New Deal legislation during the Great Depression, a period of unprecedented welfare state expansion to address the needs of the poor, the unemployed, labor, and the aged (e.g., Ametha, Dunleavey, and Bernstein 1994; Goldfield 1989; Piven and Cloward 1977; Quadango 1984; Skocpol 1980). A key

FIGURE 5. Predicted Total Effect of Insurgency on AFDC and Incarceration Rates, by Political Context

A. Low Violence, Weak Electoral Influence
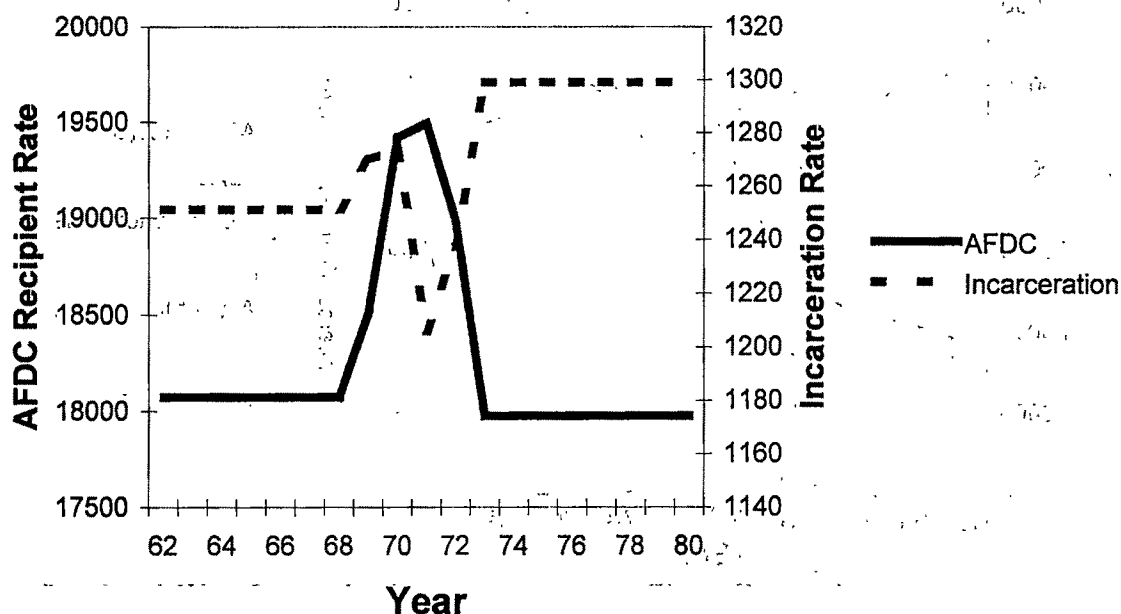
B. High Violence, Strong Electoral Influence
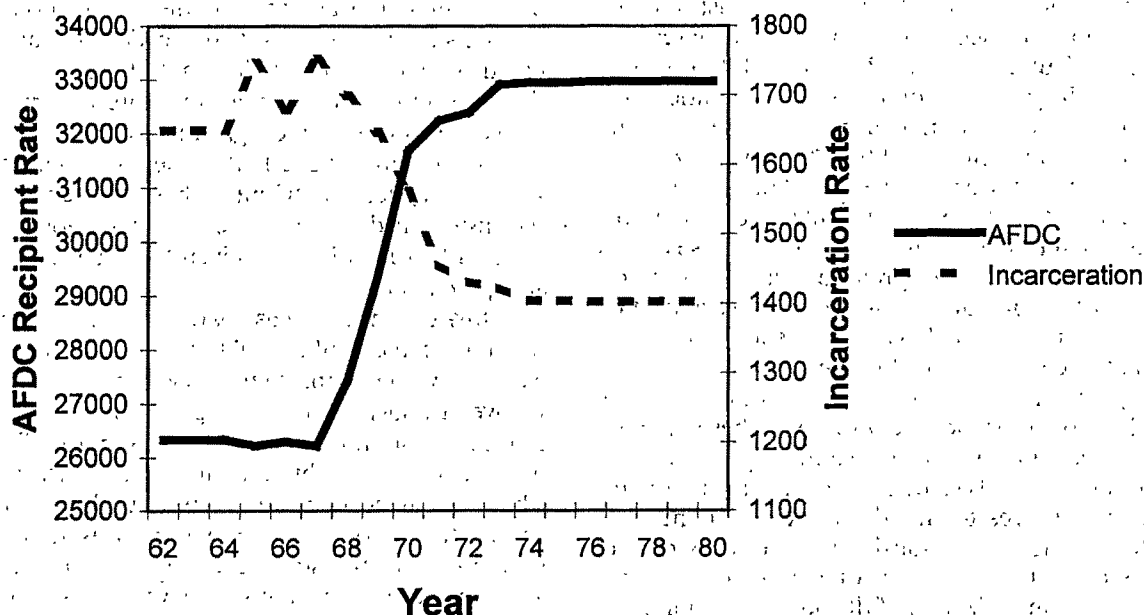
issue in this debate is the role of insurgency versus reform-minded politicians and parties in promoting reform. To the extent that the state functioned similarly during the 1930s and the 1960s, this research provides some support for the view that extrainstitutional politics have been important in the development of the American welfare state.

This conclusion is not without qualification, how-

ever, as conventional electoral channels appear not only to condition the response to insurgency but also can contribute to reform independently. With respect to AFDC, this is evident by the direct influence of state ideology on welfare expansion (Table 1). For incarceration, the effects of party control and interparty competition also provide evidence of the importance of conventional politics in the area of criminal justice

FIGURE 6.   Trends in AFDC Participation and Incarceration Rates at the National Level, 1962–80
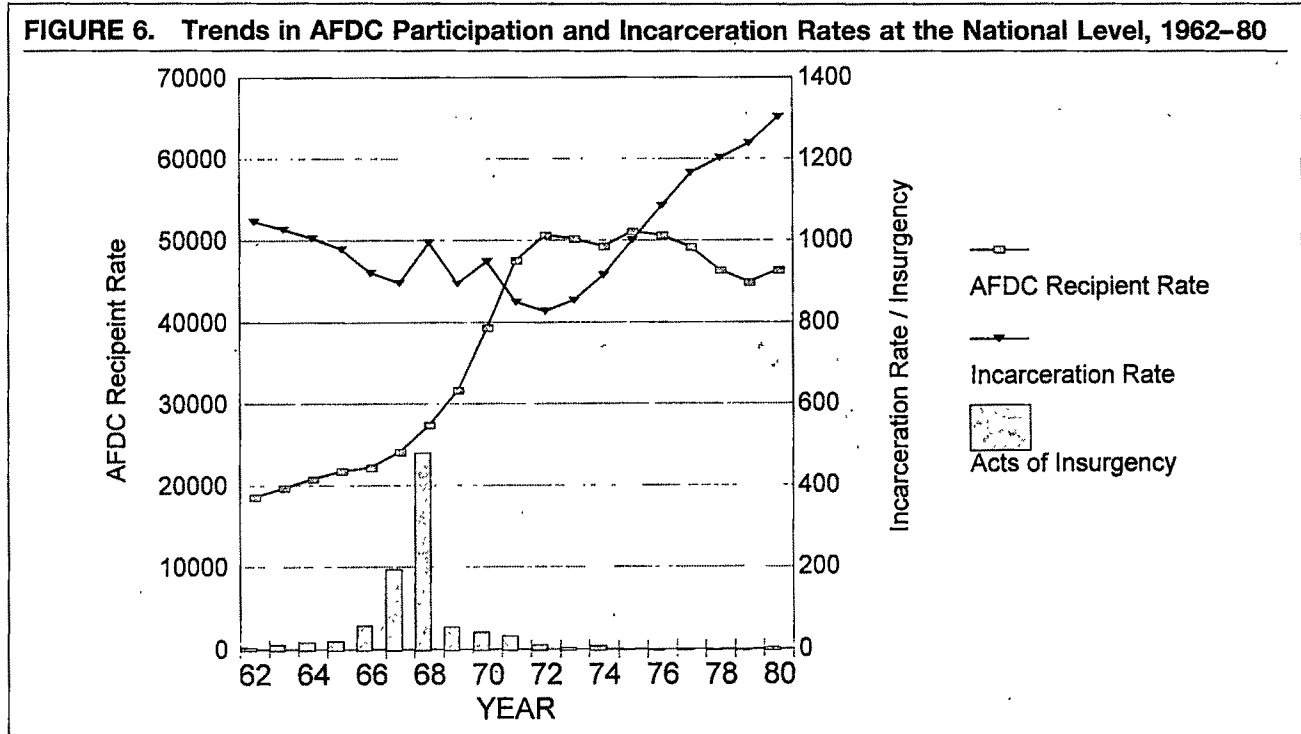
policy (Table 2). Even if Marxists are correct regarding the role of the state, one should not ignore the relevance of other political and social constraints on state behavior. This seems to be the conclusion reached by Ametha, Dunleavey, and Bernstein (1994) in their analysis of Huey Long's followers. Their political mediation theory suggests that evolution of the welfare state is due to, and often conditional upon, a number of traditional economic and conventional political factors, and unconventional politics is only one (albeit the strongest) determinant. The results of my research fit nicely with such an interpretation, as conventional political institutions appear to operate as important constraints on state behavior and sometimes have significant effects in and of themselves.

Perhaps of more interest to students of contemporary welfare policy is the extent to which these findings, and the social control perspective more generally, can explain welfare trends since 1980. It appears that many states were unsuccessful in reducing AFDC in the years immediately after unrest subsided. If the social control model is correct, however, labor market imperatives, coupled with a mobilization of the business community, eventually would motivate policymakers to reduce welfare generosity throughout the 1980s and 1990s. According to Piven and Cloward (1982, 13), this was an important motivation for President Reagan's attack on public assistance programs, as AFDC served to "limit profits by enlarging the bargaining power of workers with employers." This thesis is echoed by Noble (1997, 107) who maintains that "generous public assistance programs made it harder for employers to cut labor costs because they cushioned workers from the shock

of unemployment." He concludes that the political mobilization of the American business community played an important role in the contraction of the welfare state over the last two decades, perhaps culminated by national legislation in 1996. On the surface these developments appear to represent merely an ideological swing to the right that began in the 1980s, but this research suggests that the continued effort by states to reduce AFDC might best be seen as part of the historical dynamic of welfare expansion and contraction first identified by Piven and Cloward in *Regulating the Poor*.

If this dynamic at least partly explains recent contraction of the welfare rolls, then this research may explain a second important policy trend in recent years. If a welfare-incarceration tradeoff exists, then efforts to reduce welfare throughout the 1980s and 1990s should have been matched by some increase in incarceration. This seems to correspond with what we know about the aggregated national level trends, but it is not necessarily the case at the state level. Some evidence is provided, however, by recent state-level analyses of AFDC caseload reductions and AFDC waiver adoptions (Fording 1998), and an analysis of state welfare reform provisions under the Temporary Assistance to Needy Families Program (Soss et al. n.d.). Each of these studies reports that a decline in welfare generosity by states is related to an increase in incarceration levels. More research is needed to determine exactly why this relationship exists, but it appears that the utility of the social control perspective in explaining policymaking may extend beyond the period of black insurgency.

APPENDIX: DEFINITION OF VARIABLES AND DATA SOURCES

Insurgency: From Fording (1997, 11), defined as "any act of violence on behalf of blacks or minorities, either spontaneous or planned, which is either framed as, or can be construed as politically motivated." Data were originally obtained from several sources, including the *New York Times*; the *Report of the National Advisory Commission on Civil Disorders; Riot Data Review*, published by the Lemberg Center for the Study of Violence; Facts on File; and *Congressional Quarterly*.

AFDC: Change in the number of AFDC recipients per one million population, for December of each year. These data are available from *Social Security Bulletin*, various years.

Incarceration: Change in the number of adults incarcerated per one million population. Data are published by the Department of Justice in *Prisoners in State and Federal Institutions on December 31* and are reprinted in *Statistical Abstract of the United States*.

Per Capita State and Local Government Tax Revenue, Per Capita Income (1967 dollars): Measured as the first difference. Published yearly in *Statistical Abstract of the United States*.

Interparty Competition: A yearly measure was computed by averaging the majority vote for governor and the average majority percentage within the state legislature (% of majority in House/2 + % majority in Senate/2). This value was subtracted from 100 to yield a measure of competition that ranges from zero (no competition) to 50 (perfect competition). Interparty competition is measured as the change (first difference) in competition. All component variables are available from *Statistical Abstract of the United States* and the *Book of the States*.

Democratic Control of State Government: Measured as the *change* in Democratic control, so that state-years in which Democratic control replaces Republican or divided control receive a value of 1, state-years in which Republican or divided control replaces Democratic control receive a value of −1, and all other state-years receive a value of 0. Relevant data are available from *Statistical Abstract of the United States* and the *Book of the States*.

Crime: Change in the yearly crime rate (number of offenses per 100,000 population). Crime data are published by the Federal Bureau of Investigation in *Uniform Crime Reports for the United States* (reprinted in *Statistical Abstract of the United States*). Part I and II offenses are included.

Urbanization: Annual change in the percentage of state population that resides in metropolitan areas. Data were obtained for decennial years from the U.S. Census and for intervening years from the Bureau of Census, *Current Population Reports* (P-25).

Poverty Data: Annual change in the number of female-headed families below poverty per 1,000 population (AFDC model) and in the percentage of individuals below poverty (for incarceration). Available for 1959, 1969, 1975, and 1979 (intervening years interpolated) in *State and Metropolitan Area Data Book, 1979, 1982,* and *Statistical Abstract of the United States*.

Unemployment: Annual change in state unemployment rate (yearly average). State data were obtained for every year and are published by the U.S. Department of Labor, *Employment and Training Report of the President*.

Class Bias: From Husted and Kenny (1997), measured as the annual change. This variable is constructed by (1) obtain-

ing yearly income per capita for each state, (2) obtaining an alternative version of state income per capita by computing a weighted average of county-level income per capita, with weights based on turnout in the most recent federal election, and (3) calculating the final measures as the ratio of the turnout-adjusted measure created in step 2 to the statewide average published by the U.S. Department of Commerce.

Reapportionment: Modeled as an intervention; the value represents the magnitude of malapportionment (i.e., the extent of reapportionment (i.e., the extent of reapportionment) that existed before the properly apportioned system was implemented. This value is calculated as follows: (Maximum District Size − Minimum District Size)/(Average District Size). The measure is based on detailed accounts of state reapportionment efforts reported by the National Municipal League in *Apportionment in the Nineteen Sixties* and the *Book of the States*, published by the Council of State Governments.

Black Electoral Access (Power): From Fording (1997), a dummy variable taking on a value of 1 when two conditions are satisfied: (1) blacks have effective voting rights (based on implementation of the Voting Rights Act [VRA] of 1965 in certain states) and (2) districts are properly apportioned (for states in which more than 90% of blacks reside in urban areas); 0 otherwise. Data are from the National Municipal League, *Apportionment in the Nineteen Sixties*, and the *Book of the States* as well as targeting of VRA enforcement.

Black Population: Data are available for black population at the state level for 1960 from the U.S. Census and for 1970–80 from the Bureau of the Census, *Intercensal Estimates of the Population of Counties by Age, Sex, and Race: 1970–80* (ICPSR #8384). Values for intervening years were interpolated between 1960 and 1970.

Medicaid: A dichotomous variable that takes on a value of 1 in the first year a state implemented the Medicaid program, 0 otherwise. Implementation dates for the Medicaid program are reported in Table 7 of National Center for Social Statistics (NCSS) report B-5, *Medicaid Statistics, FY 1971*.

AFDC Residency Requirements: Detailed data for various characteristics of state AFDC programs are published by the U.S. Department of Health, Education, and Welfare in *Characteristics of State Public Assistance Plans under the Social Security Act*.

Military Data: Total number of active-duty military personnel (Army, Navy, Marine Corps, and Air Force). Published by the Department of Defense, available at http://web1.whs.osd.mil/mmid/military/trends.htm.

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More Order with Less Law: On Contract Enforcement, Trust, and Crowding

IRIS BOHNET *Harvard University*
BRUNO S. FREY *University of Zürich*
STEFFEN HUCK *University of London*

*M*ost contracts, whether between voters and politicians or between house owners and contractors, are incomplete. "More law," it typically is assumed, increases the likelihood of contract performance by increasing the probability of enforcement and/or the cost of breach. We examine a contractual relationship in which the first mover has to decide whether she wants to enter a contract without knowing whether the 'second mover will perform. We analyze how contract enforceability affects individual performance for exogenous preferences. Then we apply a dynamic model of preference adaptation and find that economic incentives have a nonmonotonic effect on behavior. Individuals perform a contract when enforcement is strong or weak but not with medium enforcement probabilities: Trustworthiness is "crowded in" with weak and "crowded out" with medium enforcement. In a laboratory experiment we test our model's implications and find support for the crowding prediction. Our finding is in line with the recent work on the role of contract enforcement and trust in formerly Communist countries.

T rust can increase efficiency in the economic and political spheres. Recent studies using aggregate data suggest the existence of an efficiency-enhancing feature of trust for countries and organizations.[1] We attempt to provide a microfoundation for some of these findings by investigating whether trustworthiness can have an economic payoff at the individual level. Important domains for trust and trustworthiness include the relationship between representatives and their constituents, such as between politicians and voters, managers and shareholders, or attorneys and clients. In all these situations, principals have to decide whether they want to enter a contract that they know will be incomplete (i.e., agents might have an incentive

to breach). Offering the contract is a matter of trust, and performing it, a matter of trustworthiness.

The problem of trust is more pronounced in large, anonymous societies than in small groups. In the latter case, participants frequently interact, and reputation matters. Therefore, according to the folk theorem type of argument, cooperation can be sustained even in the absence of genuine trustworthiness.[2] This kind of argument fails in the case of large groups, and institutions such as the law are needed to facilitate efficient outcomes.[3] The law, however, may affect behavior not only by creating incentives but also by influencing preferences. Whereas rational choice theory focuses on the first aspect, we propose a model that integrates both. We analyze how the enforceability of a contract affects individual performance in the short run with given preferences and in the long run when preferences adapt to the new environment. Our analysis builds on an evolutionary approach.[4] We present analytical results and test their implications in a laboratory experiment.

A contractual relationship is represented by a game in which the first mover has to decide whether she wants to enter a contract without knowing whether the second mover will perform. If the second mover breaches, a chance move decides whether he is held liable for the cost of breach. Standard economic analysis of law predicts that the higher the expected cost of breach, the more likely is the second mover to perform.

Iris Bohnet is Assistant Professor of Public Policy, Kennedy School of Government, Harvard University, Cambridge, MA 02138 (Iris_Bohnet@Harvard.edu). Bruno S. Frey is Professor of Economics, Institute for Empirical Economic Research, University of Zürich, Bluemlisalpstreet 10, 8006 Zürich, Switzerland (bsfrey@iew.unizh. ch). Steffen Huck is Senior Lecturer in Economics, Department of Economics, Royal Holloway College, University of London, Egham, Surry TW20 0EX, United Kingdom (s.huck@rhbnc.ac.uk).

[1] See, for example, Fukuyama 1995; Knack and Keefer 1997; La Porta et al. 1997; or Putnam 1993.

[2] Folk theorems say that in infinitely repeated games any feasible payoff combination can be supported by an equilibrium.
[3] See, for example, Greif, Milgrom, and Weingast 1994 and Milgrom, North, and Weingast 1990. For a proof of the folk theorem in the prisoner's dilemma with large groups and anonymous interaction, see Ellison 1994. The behavioral relevance of repetition and anonymity have been studied in many laboratory experiments, e.g., Andreoni 1988 and Bohnet and Frey 1999. For a survey, see Ostrom 1998, who also discusses how experimental results relate to political science.
[4] See, for example, Axelrod 1984; Becker 1976; Bendor and Swistak 1997; Bowles 1998; Boyd and Richerson 1985; or Güth and Kliemt 1999.

We show that, when preferences are subject to change, this need not be the case. More specifically, we find that the probability with which a contract is enforced has a nonmonotonic effect on behavior: Performance rates of second movers are high not only when the expected cost of breach is sufficiently large but also when it is sufficiently small.

We focus on preferences for contract performance and assume that individuals may experience psychological costs when they breach. Such trustworthy people are said to have a preference for honesty.[5] Based on the idea that a specific preference is more likely to be maintained and to flourish if it proves to be economically successful, we study a dynamic process in which preferences can change over time. Legal rules can "crowd in" as well as "crowd out" preferences. We find that intermediate levels of contract enforcement lead to crowding out, but low levels induce crowding in. The intuition for this is rather straightforward. Suppose a first mover must decide whether to enter a contract. If she knows that the legal system is inefficient, that is, contracts are rarely enforced, she will be extremely cautious. Clearly, she would like to enter if she knew the other party could be trusted. If a signal is received about the partner's trustworthiness, it must be "sufficiently good." This caution not only protects first movers from being exploited too often but also makes trustworthy second movers more successful than others, because on average they will get more contracts than others. Hence, honesty will be crowded in.

Contractual relationships with weak enforcement are typical in many organizational settings. Some firms purposely create a low enforcement environment in which interactions are not guided by the expected cost of breach but by intrinsic motivation. At the same time, they heavily invest in screening of potential employees, stressing that character is more important than the possession of specific skills.[6] Similarly, most microfinance institutions (e.g., Grameen Bank or Accion) that lend money to poor clients without physical collateral focus on "character-based lending." In the absence of external enforcement mechanisms, the intrinsic trustworthiness of clients is a key variable that makes the contract between borrower and lender possible (Murdoch 1999).

The same pattern applies to many other domains: The more leeway agents have—whether these are employees, borrowers, legislators, judges, or executives—the more careful are principals when deciding who will be offered a contract. That the leeway for politicians can be considerable becomes clear in Rose-Ackerman's (1999) analysis of corruption. She points out that even in the United States the law is not strict

enough to deter elected officials from being corrupt. "The criminal penalties are 'not more than three times the monetary equivalent of the thing in value (i.e., the bribe) or imprisonment for not more than fifteen years, or both' (18 USC § 201 (a)). This is appropriate for officials who receive bribes except that multiplying by three may be a poor measure of the risk of detection and punishment. The actual probability of catch is likely to be well below one-third" (p. 55). Whether this probability is low enough to induce crowding-in is an empirical question. If the probability of contract enforcement is higher but not high enough to deter all second movers from breaching, then we expect crowding-out.

Under medium enforcement, the expected payoff of entering is higher than the payoff of abstaining, even if the first mover knows that the contract will be breached. Accordingly, she will enter regardless of her beliefs about the partner's trustworthiness. This unconditional trust makes second movers who want to maximize expected monetary payoffs more successful than honest types who forsake profitable opportunities to breach. Therefore, honesty will be crowded out, causing the aforesaid nonmonotonicity. In sum, under high levels of enforcement, all second movers perform because they are deterred regardless of their preferences, and all first movers enter the contract; preferences are irrelevant and outcomes are efficient. At intermediate levels, honesty is crowded out; more second movers breach, and resources are wasted in trials. At low levels, trustworthiness is crowded in; more second movers perform even though they would have an incentive to breach without a preference for honesty, and efficiency increases.

Contract enforcement probabilities that are too small to deter breach may be due to a badly functioning legal system with weak state protection, corrupt governments and judiciaries, or high enforcement costs. So far, the literature has focused mainly on how informal institutions, such as social norms, may substitute for an ineffective legal system and whether shame and ostracism can replace imprisonment and fines.[7] Our model focuses on formal law and intrinsic dispositions and shows how the effectiveness of each depends on the other. By providing a specific legal enforcement regime, the state affects the degree of trust and trustworthiness in a society. By including formal institutions in the analysis we address an aspect rarely examined in the current debate on trust and social capital.[8]

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[5] We are purposely vague here for two reasons. Our model does not depend on the specifics of the psychological costs incurred and our experiment does not test for different kinds of such costs. Our results are compatible with intrinsic motivation (see, e.g., Frey 1997), inequality aversion (see, e.g., Bolton and Ockenfels 2000 or Fehr and Schmidt 1999), reciprocity (see, e.g., Rabin 1993), and psychological contracts (see, e.g., Rousseau 1995).

[6] For a summary of such high-commitment human resource management practices, see Baron and Kreps 1999.

[7] Social norms confine minor crimes, such as trespassing (Ellickson 1991), or the overuse of common pool resources (Ostrom, Gardner, and Walker 1994). For the legal debate about "alternative sanctions" see Kahan 1996, and for a general discussion of how social norms and the law interact, see Cooter 1998 or Sunstein 1996.

[8] Tarrow (1996, 395) asks: "Can we be satisfied interpreting civic capacity as a home-grown product in which the state has played no role?" Schneider et al. (1997) are among the few who discuss the influence of institutions, namely, the extent to which parents can choose a public school, on social capital. Those interested in this influence mainly focus on the relationship between institutions and trust rather than between institutions and trustworthiness (see, e.g., Brehm and Rahn 1997; Norris 1999; and Nye, Zelikow, and King 1997).

Our findings may help understand two tendencies observed in many countries of the former Soviet block. On the one hand, there is a demand for "more law" in order to enforce contracts and secure property rights. When the state cannot provide levels of enforcement high enough to deter breach, the demand for protection is satisfied privately. This is one explanation for the rise of the Mafia in Sicily (Gambetta 1993) and may account for its thriving in Russia (Varese 1994). On the other hand, a reemergence of the demand for and supply of trust-based relationships can be observed in the very same countries. Wintrobe (1995, 46–7) writes: "The absence of enforceability generates a demand for trust. The costs of trust formation are lower, when the two parties share common traits, such as a common language, ethnicity, and so on." His analysis differs from ours, but his conclusions are similar: More order can be achieved by relying on trust-based relationships when each party can predict the other's likelihood of cooperation.

We suggest a rationale for the coexistence of these two tendencies. If trustworthiness has been crowded out, people cannot help asking for "more law," and if it has been crowded in, they can rely on trust-based interactions. According to Simis (1982) and Varese (1994), the former Soviet system was characterized by corruption affecting only specific sections of the population, namely, the "nomenclatura." In the absence of a credible state, these groups are unable to engage in trust-based interactions and, thus, demand private protection. For ordinary people, trust was and remains the basis of their contractual relationships (Wintrobe 1995). Overall, our model predicts a pattern encapsulated in a Latin American quip: For friends everything, for enemies nothing, for the stranger the law.[9] The model's empirical validity is tested in a laboratory experiment that implements the theoretical setup as closely as possible, and the results support the crowding predictions.

The article is organized as follows. We first describe how agents behave with fixed preferences. Next, we analyze the crowding dynamics and discuss the model's main implications. We then present the design and the experimental results. In the final section we offer conclusions.

THEORY

The Contract Game

We model a situation in which two players have the opportunity to produce a joint surplus, but the players are asymmetrical. Player 1 has to enter the contract without knowing whether player 2 will perform. Therefore, player 1's decision to enter is a matter of trust.[10]

We denote her trusting move with T and her nontrusting move with $\bar{T}$. In the case where she trusts, player 2 can perform (move P) or breach ($\bar{P}$). The game ends either if player 1 does not enter, which yields zero payoffs for both parties, or if player 1's trust is rewarded by player 2's cooperation. This yields payoffs of 1 for both players.[11] If player 2 breaches, we assume a final chance move that captures a litigation process, with probability p that 2 will be held liable (move L). The surplus is divided, as in the case of performance, but the loser has to bear the costs of the trial $c > 0$. In other words, we assume that perfect expectation damages place player 1 in the position in which she would have been if player 2 had performed and that all legal fees are paid by player 2, the loser.[12] Thus, the payoffs are 1 for player 1 and $1 - c$ for player 2. If player 2 is not held liable ($\bar{L}$), he profits from breach and receives a payoff of $1 + b$ (with $b > 0$); player 1 bears the legal cost and is not compensated for any investments she made by entering the contract, so her payoff is $-a$ with $a \geq c$.

Breach is never efficient. The benefits from it are not large enough to compensate the first mover, that is, $b < 1 + a$. Figure 1 shows this game in its extensive form.

We assume that all payoffs are monetary, but in order to solve the game we need utilities associated with the various outcomes. To map outcomes into (cardinal) utilities, we assume two possible preferences for players. One type of player (M) is only interested in (expected) monetary payoffs, so for this type the monetary payoffs in Figure 1 represent utilities. The second type of player (H) has a preference for honesty and suffers a psychological cost when breaching a contract. These costs are so high that H types will never betray regardless of the monetary gain b.[13]

With this set of possible preferences $\{M, H\}$, we can transform the "money game" of Figure 1 into a standard game in which the payoffs are von Neumann-Morgenstern utilities. This is done by replacing the payoff of player 2 after path $\overline{TPL}$ by $1 + b - \delta$, where $\delta = 0$ for type M, and $\delta > b$ for type H.

Assuming that player 1 recognizes whether player 2 is M or H, we have, for each possible match of players, a well-specified standard game.[14] We solve this game

[9] "A los amigos todo, a los enimigos nada, al extraño la ley" (Rose-Ackerman 1999, 97).

[10] For related games on trust, see Berg, Dickhaut, and McCabe 1995; Burnham, McCabe, and Smith 1999; Glaeser et al. 2000; Güth and Kliemt 1999; or Van Huyck, Battalio, and Walters 1995.

[11] This specification of the payoffs is a simple normalization without loss of generality.

[12] This corresponds to the English legal cost allocation rule.

[13] This assumption simplifies the analysis without altering the results, all of which would still hold if we allowed different levels of psychological cost. Notice that in the case of a continuous space—possibly ranging from infinite (psychological) costs to infinite gains—all that matters is whether the costs are larger or smaller than the monetary gain b.

[14] If player 2 is type M, the game is identical to that shown in Figure 1. If player 2 is type H, the payoff after path $\overline{TPL}$ is $1 + b - \delta < 1 + b$.

FIGURE 1. The Contract Game with Monetary Payoffs
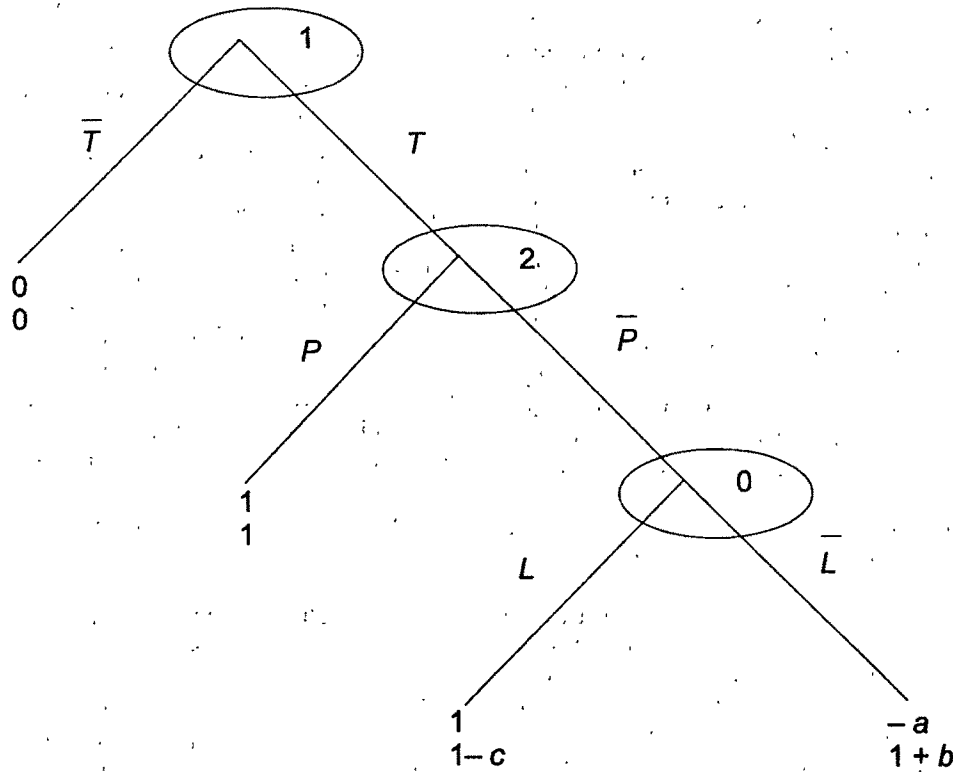
Note: Player 1 chooses between T (her trusting move) and $\overline{T}$ (her nontrusting move). After T, player 2 chooses between P(erformance) and nonperformance $(\overline{P})$. After $\overline{P}$, chance decides whether player 2 is held L(iable) or not $(\overline{L})$. The end nodes show the two players' respective payoffs.

by backward induction.[15] Obviously, player 2 will breach if his expected payoff exceeds 1, that is, if

$$p(1 - c) + (1 - p)(1 + b - \delta) > 1. \qquad (1)$$

For type H this is never fulfilled, because a player 2 with a preference for honesty will always choose P. For type M, we can insert $\delta = 0$ into inequality 1 and rewrite it as

$$p < \frac{b}{b + c}. \qquad (2)$$

Player 2 of type M breaches if the probability of enforcement is smaller than the benefit of breach divided by the benefit plus the legal cost. Next, consider the decision of player 1. If she is confronted with an H type, she will surely trust. The same is true if she is confronted with an M type and

[15] Admittedly, the assumption of perfect type recognition is strong, but the results hold as long as players receive sufficiently informative signals about the opponent's type. This is also discussed later and illustrated in Appendix A. The main effect of assuming perfect signals is that it simplifies notation and analysis significantly. Recent experimental evidence supports the notion that type recognition is possible after preplay communication in a prisoner's dilemma game (Frank, Gilovich, and Regan 1993; Orbell and Dawes 1991). Kikuchi, Watanabe, and Yamagishi (1995) found that trustworthy types are better at predicting others' types than are nontrustworthy types. More generally, see Frank 1988.

$$p > \frac{b}{b + c}$$

holds. But if she is confronted with an M type and (2) is fulfilled, she will enter only if her expected payoff exceeds her outside-option payoff, that is, if $p - a(1 - p) > 0$. This can be rearranged as

$$p > \frac{a}{1 + a}. \qquad (3)$$

From this a proposition follows.

PROPOSITION 1. *The (unique) subgame perfect equilibrium (SPE) is (T,P) if player 2 is of type H or if player 2 is of type M and*

$$p > \frac{b}{b + c};$$

this is the high-probability regime [High p]; (T,$\overline{P}$) if player 2 is of type M and

$$\frac{a}{1 + a} < p < \frac{b}{b + c},$$

called the medium-probability regime [Medium p]; and ($\overline{T}$, $\overline{P}$) if player 2 is of type M and

$$p < \min\left\{\frac{a}{1+a}, \frac{b}{b+c}\right\},$$

the low-probability reigme [Low p].

We shall assume below that $ac < b$. Otherwise $(T,\bar{P})$ would never be an SPE, and the crowding analysis would be less rich and less interesting. Imposing this requirement means, informally speaking, that the loss player 1 incurs from uncompensated betrayal must be relatively small in comparison to the profit of player 2 and the legal costs.

Crowding

We have shown how individuals with given preferences behave under different legal regimes, and we now allow for preferences to adapt to the contractual situation. This enables us to study the implications of "preference crowding" for our model. Economically successful preferences are crowded in, and unsuccessful preferences are crowded out. Formally, the share of types with a certain preference grows faster than another share if and only if the average material earnings of the former exceed the average earnings of the latter. In the context of our model and in the absence of a fully fledged theory of preference formation, this assumption seems reasonable. Contracts are closed to secure material benefits, and the outcomes of our contract game are exclusively characterized by different resource allocations.[16]

The assumption that successful "traits" spread is often associated with models of genetic evolution, although it can be justified differently.[17] One justification is offered in Appendix B, which briefly illustrates a stochastic model of individual preference adaptation.[18] In our model, this implies the following: If honesty leads to forsaking profitable opportunities such that a typical H earns less than a typical M, honesty will be crowded out. If an environment favors honesty, such that, on average, H types earn more than M types, honesty will be crowded in.

In order to calculate the success of the two different

types (of preferences), we assume a random match of players and enough individuals to ensure that the law of large numbers can be applied. This allows us to take expected values as a measure of success.

Proposition 1 shows that what happens when two players interact depends on the value of p, the probability that the contract is enforced. The proposition distinguishes three regimes, and in each case we can show how preferences are crowded in and out.

In the high-probability regime,

$$p > \frac{b}{b+c},$$

all individuals, regardless of type and matching, will play the SPE $(T,\bar{P})$. Players of both types receive the same expected monetary payoff, so there will be no crowding. Regardless of the numbers of H and M types, the high enforcement probability ensures performance.

In the medium-probability regime,

$$\frac{a}{1+a} < p < \frac{b}{b+c},$$

behavior in a match depends on the type of player 2. If player 2 is an H, the SPE is (T,P). If he is an M, the SPE is $(T,\bar{P})$. Since M maximizes expected monetary payoffs but H does not, it follows that average earnings of M types exceed those of H types. In the role of player 2, M types earn on average $p(1-c) + (1-p)(1+b)$, which is strictly greater than 1, the payoff of an H type in the role of player 2. In the role of player 1, both types do equally well on average, since the expected payoff of player 1 is independent of her type. Thus, with medium p, M types always earn more than H types (regardless of their number). Accordingly, honest preferences will be crowded out. This implies, asymptotically for the long run, that H types will completely vanish and that all individuals will play equilibrium $(T,\bar{P})$.

In the low-probability regime,

$$p < \frac{a}{1+a},$$

we take into account that the SPE is $(T,\bar{P})$ if player 2 is an M and (T,P) if player 2 is an H. In this case, the earnings of H types exceed the earnings of M types. In the role of player 2, an H always receives 1, an M always 0. In the role of player 1, the average payoffs of both types are again identical. They do not depend on their own type. Thus, with low p, preferences for honesty are crowded in and, in the long run, type M will vanish, such that all individuals will play the trust-rewarding equilibrium (T,P).

We summarize our results in the following proposition.

PROPOSITION 2. *In the high-probability regime, where*

$$p > \frac{b}{b+c},$$

there is no crowding; in the long run both types may be present in the society, and all individuals play (T,P).

[16] Other studies analyze endogenous preferences in a similar framework (e.g., Bester and Güth 1998; Huck and Oechssler 1999; To 1999), especially Güth and Kliemt 1999, who also deal with the issue of trust. Pioneering studies suggesting that endogenous preferences may result from payoff-driven (evolutionary) dynamics are Becker 1976 and Hirshleifer 1977. We do not wish to generalize too much, however, as different mechanisms may apply if one deals with attitudes toward drugs, religion, or other domains in which outcomes are less tangible. For a survey of alternative approaches, see Bowles 1998.

[17] For two such justifications, see Börgers and Sarin 1997 and Schlag 1998.

[18] We assume that the probabilities with which preferences are adapted depend on two factors, economic success and conformity. The more an individual's environment favors a certain preference and the more common is a certain preference, the more likely is an individual to develop the same preference. (Note, however, that adaptation is not deliberate. People cannot consciously decide about their preferences. For a different approach, see Cooter 1998.) We show that this process behaves, under certain additional regularity assumptions, like a growth-monotonic (evolutionary) process.

In the medium-probability regime, where

$$\frac{a}{1+a} < p < \frac{b}{b+c},$$

trustworthiness is crowded out; in the long run only type M will be present in the society, and all individuals play (T,P̄).

In the low-probability regime, where

$$p < \frac{a}{1+a},$$

trustworthiness is crowded in; in the long run only type H will be present in the society, and all individuals play (T,P).

The long-run stable states also can be derived by analyzing evolutionary games in which the two types compete. For

$$p > \frac{b}{b+c}$$

this game is trivial, because both types behave identically and receive identical payoffs. For the other cases, the two matrix games in tables 1 and 2 emerge. (Note that the payoffs are based on the assumption that both types are equally likely to become player 1 or 2).
  As

$$p < \frac{b}{b+c},$$

it is easy to see that the game has a unique evolutionary stable strategy (see Maynard Smith 1982). There is a unique equilibrium (M,M), and the equilibrium is strict. Hence, the unique evolutionary stable strategy is M. This mirrors the second result of proposition 2: In the long run only M types survive.
  An even simpler matrix emerges in the low-probability regime, where

$$p < \frac{a}{1+a}.$$

Clearly, the unique equilibrium of this game is (H,H); because the equilibrium is strict, the unique evolutionary stable strategy is H. This mirrors the third result of proposition 2.

TABLE 1. The Evolutionary Game with Medium Probability p

$\frac{a}{1+a} < p < \frac{b}{b+c}$	Type M	Type H
Type M	$1 + b - a + p(1 - b + a - c)$	$2 + b - p(b + c)$
Type H	$1 - a + p(1 + a)$	2

TABLE 2. The Evolutionary Game with Low Probability p

$p < \frac{a}{1+a}$	Type M	Type H
Type M	0	1
Type H	1	2

Discussion

There are two main implications of proposition 2. The most fundamental one is that it is impossible to predict behavior in a group of agents playing the contract game without knowing their history. Individual preferences are subject to change, and outcomes in one round affect the distribution of types in the next. And because preferences depend on past regimes, so do actions. The longer a group plays in a low-probability environment, the more agents with a preference for honesty are present, and the less breach is observed. The opposite is true for a medium-probability environment. Also, it may be possible that groups have experienced regime changes in the past, in which case one needs to know not only how long the group has been playing under the current regime but also how long under the preceding one. The total crowding history matters.
  The second significant implication is that the probability of contract enforcement exerts a nonmonotonic effect on behavior, which would not occur in standard models with fixed preferences.[19] The worst legal regime is not one in which contracts cannot be enforced but one with an intermediate level of enforceability. With an intermediate p, first movers do not care with whom they interact, because entering the contract is better than staying outside, even if the contract is breached. This lack of caution makes dishonest second movers economically successful, so the share of dishonest types will grow. There are then two alternatives: more order with more law or more order with less law.
  With less law, first movers have to be extremely cautious. They have to think about their partners' trustworthiness, which makes honesty a successful preference. In our model, first movers receive a perfect signal about their opponents' type, and their decision rule is simple: "Only enter a contract if your opponent is trustworthy." With a stochastic signal the rule would be very similar: "Only enter if the signal is good enough." This illustrates how our results would extend to the more general case of imperfect but informative signals. With perfect signals, M types in the role of player 2 are *never* offered contracts when p is low, while H types *always* get contracts. With imperfect signals, some M types would get contracts, while some H types would not (namely, whenever the signal is wrong). If the signal is sufficiently informative, however, H types will get *more* contracts than M types, which is required

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[19] To the best of our knowledge, the first authors to highlight the possibility of such nonmonotonicities are Akerlof and Dickens (1982). They study a model with cognitive dissonance that may induce players to reevaluate outcomes.

TABLE 3. Experimental Sessions

Session	Matching	Prob. 1-3	Prob. 4-9	Number of Subjects	Univ.
1 (RLLB)	Random	Low (p = .1)	Low (p = .1)	20	B
2 (RMLB)	Random	Medium (p = .5)	Low (p = .1)	28	B
3 (RHLB)	Random	High (p = .9)	Low (p = .1)	28	B
4 (FMLB)	Fixed	Medium (p = .5)	Low (p = .1)	16	B
5 (RLLH)	Random	Low (p = .1)	Low (p = .1)	34	H
6 (RMLH)	Random	Medium (p = .5)	Low (p = .1)	28	H

for crowding in. Appendix A elaborates on the case of imperfect signals further.

Comparing the two alternative policies for replacing a medium probability regime ("more or less law"), two differences can be observed. The first is due to the dynamic nature of our analysis. With "more law," more order is instantly achieved, since performance and entering becomes rational for everyone. This is different in the case of "less law" because after the change of the regime the crowding process needs some time. Though our experimental results indicate that adjustments can be fast in small groups, the behavior does not change instantly. This is an argument in favor of the standard law-and-order approach. "Less law," however, is less costly. In our model, we disregard all fixed costs of legal contract enforcement and variable costs being a function of p. Increasing p costs resources; decreasing p saves resources.

Here we do not wish to make any judgment about what is the better policy. Instead, we test our model's empirical validity in the laboratory.

EXPERIMENT

Design

In the experimental design we tried to implement our model as closely as possible. Subjects played a two-person contract game and were randomly matched. Six sessions with a total of 154 subjects were conducted, and the game was repeated several times. To examine crowding in of trust (more order with less law), we confronted subjects in all sessions with a low contract enforcement probability during the last rounds. In order to create different histories, we varied the legal regime in the first few rounds. If behavior is driven by incentives only, it should be independent of the history created in earlier rounds and should depend only on the current legal regime. In contrast, if preferences adapt to the legal regime, earlier history should affect the likelihood of performance in later rounds.

Experimental subjects were confronted with identical payoffs: 50 cents for each player whenever the first mover chose $\overline{T}$ (corresponds to choosing alternative A in the experimental payoff table in Appendix C); $1.50 for each player in case of TP (corresponds to choosing alternative Y in the experimental payoff table); $1.50 for the first and $1.20 for the second mover in case of $\overline{TPL}$ (corresponds to the realization of state α in the experimental payoff table); and 20 cents for the first

and $2.50 for the second mover in case of $\overline{TPL}$ (corresponds to the realization of state β in the experimental payoff table). The normalized payoff variables were a = .3, b = 1, and c = .3. The probabilities were .1, .5, and .9.[20]

Subjects received payment for each round. Instructions (see Appendix C) were neutrally framed. After each round, aggregate information on outcomes was provided, that is, first and second movers knew how many contracts were offered and performed in the previous round. Providing information on the distribution of types only serves as a conservative test of our model, because individuals' types could not perfectly be detected. The crowding model assumes random matching, so we implemented a stranger treatment in five sessions and used a fixed-pair matching in one control session. Table 3 presents an overview of all sessions.

The experiments were conducted at the University of California, Berkeley, and at Harvard University (labelled B and H). Participation was voluntary, and students were paid a show-up fee of $5. The experiments took approximately 45 minutes. Average earnings were approximately $15.

Subjects were identified by code numbers only, and anonymity was fully preserved. After signing a consent form, participants were randomly assigned to the role of first and second mover; they were given written instructions, along with an envelope containing a code number sheet and nine decision sheets, all marked with the subject's code number. Instructions were repeated orally, which allowed subjects to ask questions and helped ensure that everyone understood the decision task. In all but session 4, they were truthfully assured that they would be randomly matched with a different person after each round. They were told that nine rounds would be played and that they would publicly be informed about the aggregate outcome after each round. Individual results were private information. Subjects could not anticipate the regime change before round 4,[21] but when informed about the new conditions, they were also told that they would play under the new regime for the remaining six rounds.

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[20] Subjects carried out the chance moves themselves. After each round a randomly chosen participant picked a card from a pile of red and black cards.

[21] The instructions told them neither that the environment would remain constant nor that it would change.

TABLE 4. Data

Session	Behavior	1	2	3	4	Round 5	6	7	8	9
1 (RLLB)	No trust ($\bar{T}$)	4	8	10	8	3	2	3	1	5
	Trust and breach ($T,\bar{P}$)	5	2	0	0	2	2	1	2	0
	Trust and perf. (T,P)	1	0	0	2	5	6	6	7	5
2 (RMLB)	No trust ($\bar{T}$)	0	0	4	10	10	10	8	4	8
	Trust and breach ($T,\bar{P}$)	10	12	8	4	4	2	3	6	4
	Trust and perf. (T,P)	4	2	2	0	0	2	3	4	2
3 (RHLB)	No trust ($\bar{T}$)	0	0	0	5	8	2	7	6	4
	Trust and breach ($T,\bar{P}$)	2	2	2	7	2	8	4	2	2
	Trust and perf. (T,P)	12	12	12	2	4	4	3	6	8
4 (FMLB)	No trust ($\bar{T}$)	0	1	0	2	0	0	4	0	8
	Trust and breach ($T,\bar{P}$)	1	0	0	0	0	2	0	0	0
	Trust and perf. (T,P)	7	7	8	6	8	6	4	8	0
5 (RLLH)	No trust ($\bar{T}$)	6	3	9	6	7	7	8	9	10
	Trust and breach ($T,\bar{P}$)	5	6	2	5	3	3	3	3	2
	Trust and perf. (T,P)	6	8	6	6	7	7	6	5	5
6 (RMLH)	No trust ($\bar{T}$)	0	1	1	11	10	8	8	7	8
	Trust and breach ($T,\bar{P}$)	8	7	8	1	1	1	0	1	0
	Trust and perf. (T,P)	6	6	5	2	3	5	6	6	6

Note: The table summarizes behavior for all sessions over time. In RLLB, R stands for random matching, the first L for low probability in the first few rounds, the second L for low probability in the remaining six rounds, and B for Berkeley; analogously, for the other sessions.

Results

Our theory predicts history-dependent behavior. The longer individuals are confronted with a low-p environment, the more trustworthiness should be crowded in, and the higher performance rates should be. High enforcement probabilities are expected to be neutral with respect to crowding, and medium probabilities should crowd out trustworthiness.[22]

Table 4 presents the results for all sessions in all rounds. We briefly examine session 4 with fixed pairs, which is a simple control session because the requirements for crowding are not fulfilled. There is no random matching and therefore no interaction on the group level. Instead, subjects play a finitely repeated game. Experiments on games with cooperative gains (e.g., repeated public goods games or gift exchange games) reveal that cooperation rates are typically higher with this kind of matching than standard theory expects. Furthermore, cooperation rates seem to be relatively stable over time but break down toward the end of the game when the "shadow of the future" loses its power and reputation no longer plays a role. This strong drop in the last rounds has been called an "end game effect" (Selten and Stoecker 1983).[23] If the contract game is comparable to these games, we should observe a similar pattern. Table 4 confirms this expectation: We find that in the fixed-pairs session cooperation drops from 100% to 0% in the last round.

In contrast, our crowding theory predicts increasing cooperation over time and rules out an end-game

effect. It predicts that trustworthy second movers perform because they receive less utility from breaching than from performing, even though breaching leads to a higher monetary payoff. Table 5 shows aggregate data for rounds 4 to 9 in all random-matching sessions. It suggests a trend toward more cooperation that does not break down. On the contrary, the performance rate (the number of contracts performed divided by the number of contracts offered) reaches its maximum in the last round, which is in line with the crowding prediction. We summarize by the following.

RESULT 1. *In the low probability environment with random matching, performance rates increase over time and there is no end-game effect.*

Our model assumes that it is most efficient if all second movers perform. We expect high efficiency rates instantly when enforcement is strong, and only slow increases in efficiency rates over time with low enforcement probabilities. Figure 2 presents the efficiency rates (the number of contracts performed divided by the number of all possible contracts), for

[22] These predictions cannot be viewed as deterministic, since the law of large numbers does not apply in the laboratory (see also Appendix B).
[23] For additional evidence, see Andreoni 1988 and Croson 1996, who differentiate between cooperation based on reputation and reciprocity.

TABLE 5. Aggregate Random-Matching Data for Rounds 4 to 9

Aggregate Data	4	5	Round 6	7	8	9
No trust ($\bar{T}$)	40	38	29	34	27	35
Trust and breach ($T,\bar{P}$)	17	12	16	11	14	8
Trust and perf. (T,P)	12	19	24	24	28	26
Performance rate	.41	.61	.63	.69	.67	.77

Note: Numbers in the first three rows are absolute numbers; in the last row they are relative frequencies.

FIGURE 2. Efficiency Rates over Time (Berkeley Only)
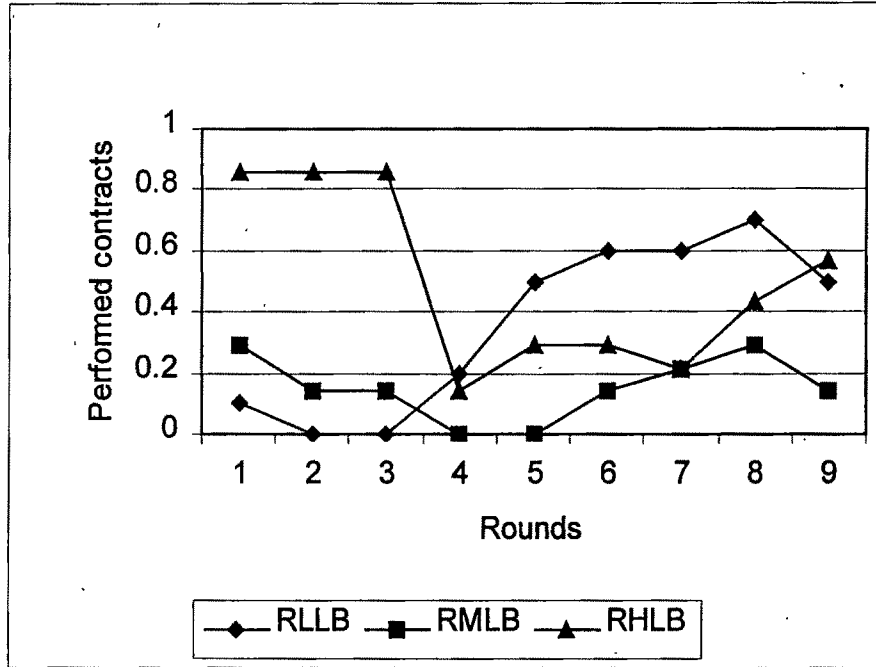
the sessions with random matching. In the short run, high enforcement probabilities lead to the most efficient outcomes. In the medium term, when the crowding dynamics start to become relevant, the low-p environment is most efficient. The longer subjects were confronted with a low-p environment, the less applicable were the differential effects of their respective crowding histories.

RESULT 2. *In the short run, efficiency rates are highest when enforcement is strong; in the medium term, they are highest in the low-probability environment; in the long run, the differential effects of enforcement and crowding tend to vanish.*

In order to analyze the data more thoroughly, we next estimated binary choice models for first movers' propensity to enter and second movers' propensity to perform, and we controlled for the relevance of crowding compared to economic incentives and for fixed effects of the university group.

In order to measure crowding, let

$$\gamma_j^t \begin{cases} 1 \text{ if } p \text{ is small in group } j \text{ in round } t; \\ 0 \text{ if } p \text{ is high in group } j \text{ in round } t; \text{ and} \\ -1 \text{ if } p \text{ is medium in group } j \text{ in round } t; \end{cases}$$

and let

$$CROWD_j^t = \sum_{h=1}^{t-1} \gamma_j^h.$$

The variable γ_j^t indicates whether our theory predicts crowding in $(+1)$, crowding out (-1), or no crowding (0), and the variable $CROWD_j^t$ summarizes the "crowding history" of group j up to round t. If the theory is

relevant, we would expect $CROWD_j^t$ to help explain the propensity of second movers to perform.[24]

In addition to $CROWD$, we include a number of variables as covariates.

- $PERFORM_j^{t-1}$ is the performance rate in group j in round $t - 1$, measured as the number of contracts performed divided by the number of contracts offered.

- $ENTER_j^{t-1}$ is the rate of entering in group j in round $t - 1$, measured as the number of contracts entered divided by the number of first movers in group j.

- $INCENT_j^t$ is a dummy variable indicating whether the first mover has an incentive to enter the contract if only monetary payoffs play a role, that is, $INCENT_j^t = 1$ if p is medium or high, 0 otherwise.

- $INCPERF_j^t$ is a dummy variable indicating whether the second mover has an incentive to perform the contract if only monetary payoffs play a role, that is, $INCPERF_j^t = 1$ if p is high, 0 otherwise.

- $UNIV_j$ is a dummy variable indicating the university group to which j belongs; $UNIV_j = 1$ for university H and 0 for university B.

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[24] It can be argued that this definition is somewhat arbitrary, and we agree. By no means do we claim that $CROWD$ captures the "true story," and certainly we do not claim that crowding is linear. Probably it is not. We think, however, that this a "Bayesian approach." In the absence of any specification that is a priori more rational than another, the above definition is the most simple and can be justified by taking expected values over equally probable alternatives. Furthermore, if we find a significant effect of $CROWD$ in its current form, any "optimal" definition would increase its significance.

TABLE 6. Propensity to Enter (First Movers) and to Perform (Second Movers) a Contract (Logistic Regression)

	Without Subject Dummies				With Subject Dummies			
	First Movers							
	Coef.	S.E.	p	R	Coef.	S.E.	p	R
$CROWD_j^t$	.048	.035	.175	.000	−.103	.078	.189	.000
$PERFORM_j^{t-1}$	2.062	.447	.000	.160	5.441	.884	.000	.218
$ENTER_j^{t-1}$	.530	.458	.247	.000	−.263	.759	.729	.000
$INCENT_j^t$	2.553	.515	.000	.173	7.955	1.508	.000	.185
$INCPERF_j^t$	4.649	4.432	.684	.000	5.914	32.663	.856	.000
$UNIV_j$	−.518	.213	.015	−.072	−1.838	5.206	.724	.000
Constant	−1.296	.325	.000		−1.999	3.063	.514	
	Second Movers							
$CROWD_j^t$	.125	.058	.030	.077	.792	.198	.000	.183
$PERFORM_j^t$	1.493	.704	.034	.080	−.959	1.669	.566	.000
$ENTER_j^{t-1}$	−.864	.709	.223	.000	−1.731	1.738	.319	.000
$INCENT_j^t$	−.177	.538	.743	.000	−1.454	1.412	.303	.000
$INCPERF_j^t$	2.057	.749	.006	.115	15.225	40.668	.708	.000
$UNIV_j$	.594	.280	.034	.077	2.588	24.336	.915	.000
Constant	−.505	.557	.365		−.484	13.967	.972	

Note: $N = 552$ for first movers, $N = 312$ for second movers. R is the partial contribution measuring the relative importance of each variable.

Furthermore, we also ran (logistic) regressions including subject dummies S_{ji}.[25] The estimated model without subject dummies is

$$\ln\frac{q_{rji}^t}{1 - q_{rji}^t} = \alpha + \beta_0 CROWD_j^t$$
$$+ \beta_1 PERFORM_j^{t-1} + \beta_2 ENTER_j^{t-1}$$
$$+ \beta_3 INCENT_j^t + \beta_4 INCPERF_j^t$$
$$+ \beta_5 UNIV_{j} + \varepsilon_{ji},$$

with q_{rji}^t indicating either first mover i's propensity to enter or second mover i's to perform. Table 6 presents the results for first movers and second movers. All sessions with random matching are included.

We find that first movers' behavior is mainly driven by the economic incentives they face and by the performance rate of the last round. In the estimation without subject dummies we also find a significant effect of the university dummy: On average, H students enter less often than B students. Since this dummy loses its significance when subject dummies are introduced, we are confident that the difference between H and B is not due to some differences in the experimental procedures (of which we were unaware). Rather, the difference is on the individual level.

In the case of second movers, we find that H students are more trustworthy than B students. Again, this effect disappears when we control for differences between individuals. In the estimation without subject dummies, three additional variables help explain second movers'

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[25] We use nested effect coding (see Königstein 1998) because the subject dummies are nested in the university dummy.

decisions: economic incentives (less breaching if breach is deterred), last round's performance rate (such that there is some inertia), and crowding history (the longer subjects interact in a low-p environment, the more likely they are to be trustworthy). Only the last variable remains significant when we include subject dummies. We summarize as follows.

RESULT 3. *First movers' propensity to enter a contract (i.e., to select alternative B in the experimental payoff table) mainly depends on their monetary incentives and on second movers' previous performance rate. Second movers' propensity to perform mainly depends on their crowding history.*

The second part of result 3 is the key finding of this study because it confirms the qualitative predictions of the crowding theory. It is a result on the aggregate level, however, so it seems worthwhile to investigate whether the theory also predicts individual behavior. The main prediction of our model on the individual level is that, in the low-p environment, subjects who breach once are much more likely to switch to honest behavior than are honest subjects to switch to breaching. To analyze this hypothesis we simply count how many second movers switch from breaching to performing once p is low. We also count how many second movers switch in unpredicted directions and how many never switch once p is low. Table 7 presents the results. The picture is clear. In the low-enforcement regime 53.6% of all subjects changed their behavior, and 89.2% of these switched in the predicted direction. In other words, 33 second movers breached in the early phases of the experiment and then performed later, even though a money-maximizing strategy favored the

TABLE 7. Individual Behavior in the Low-Probability Environment				
Session	Breach→ Perform	Unpredicted Switches	Behavior Fixed	N
1 (RLLB)	7	—	3	10
2 (RMLB)	6	1	7	14
3 (RHLB)	9	—	5	14
4 (RLLH)	6	3	8	17
5 (RMLH)	5	—	9	14
Total	33	4	32	69

old behavior. None of these 33 subjects switched back to breaching. They became trustworthy, even though they started by breaching. There were very few unpredicted switches.[26] The null hypothesis that behavior switching is random can be rejected at a significance level of .01%. Thus, the crowding theory's predictions are confirmed by individual data.

CONCLUSION

In our model the (legal) rules of a game have not only short-run but also long-run effects on behavior because they affect preferences. In a contractual relationship, economic incentives have a nonmonotonic influence on contract performance. Our model complements recent work on the interaction of rules and preferences (e.g., Fehr and Schmidt 1999), which only allows for differences in preferences, whereas our model permits preferences to change. It suggests that the rules of the game determine which preferences dominate. More specifically, it predicts that low levels of legal contract enforcement increase trustworthiness. Because first movers cannot trust the legal system, they enter a contract only if they can trust the second mover. They are careful about the decision, which makes trustworthiness a successful trait.

Arguing from a different perspective, others come to very similar conclusions. Mansbridge (1999, 305), who discusses various ways of encouraging trustworthiness and trust, concludes:

When the trustworthiness of a population is too low to sustain a general stance of initial trust, and when geographic and social mobility make reputational, kin and local sanctions less viable, the trustworthy members of a given population will benefit from finding ways of distinguishing themselves and other trustworthy individuals from the untrustworthy ... the trustworthy would find it useful to train themselves to recognize subtle signs of trustworthiness in others and also to develop in themselves signs that could not easily be mimicked.

Differential incentives to learn about others' dispositions may account for some of the cross-cultural variation in behavior found in laboratory experiments. For example, Yamagishi, Cook, and Watanabe (1998) argue that Japanese subjects are less trusting and trustworthy than American subjects because contract enforcement mechanisms and assurance structures are

more prevalent in Japan than in the United States. This corresponds to our high-p setting and our finding that when contracts are completely specified, interpersonal trust is replaced by institutional trust in the legal system. First movers enter a contract because second movers are deterred from breaching.

Previous work on crowding focuses on the relevance of preferences when contracts are complete.[27] We show that trustworthiness is crowded out not by complete contracts but by semispecified contracts. At intermediate levels of enforcement, second movers are not yet deterred from breaching, and first movers find entering a contract financially more attractive than remaining outside. Interpersonal trust is replaced by institutional trust in the legal system, and genuine trustworthiness is crowded out. Semispecified contracts cause nonmonotonic behavior: More order can result from less law, which yields a "motivation-compatible" environment (Bohnet and Frey 1997), and from more law, which yields an incentive-compatible environment.

Closely related to this finding are results by Huang and Wu (1994) and Huck (1998). Using psychological game theory,[28] Huang and Wu (1994) model games very similar to ours and show that if payoffs also depend on beliefs, then different levels of order may result from the same level of law; that is, there is a multiplicity of equilibria. Simply speaking, there is one equilibrium in which everyone believes society is functioning well and trust is rewarded, and this becomes self-fulfilling. If everyone believes the opposite, that becomes self-fulfilling. A crucial difference between the Huang and Wu approach and ours is that preferences (regarding payoffs and beliefs) are fixed in their model. Thus, no obvious dynamics lead from one state to another, and there is no straightforward link between institutional design and behavior. In contrast, Huck (1998) shows in the context of criminal law that if preferences are allowed to change, socially desirable behavior can be induced with lower levels of (monetary) punishments than one would conclude assuming fixed preferences.

In our experiment we tried to map the theoretical assumptions into a laboratory environment as precisely as possible, and the results support our qualitative predictions. Similar to Huck, we find that if there is enough time for the crowding dynamics to unfold, environments with low contract enforcement can produce outcomes as efficient as high levels of enforcement.

To the best of our knowledge, we provide the first empirical evidence of long-run effects of legal rules on behavior. Although experiments simplify reality, our contract game is informed by real-life institutions; it represents a situation in which legal enforcement leads

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[26] Notice that, as the theory is stochastic, these instances are not entirely unpredicted.

[27] See Titmuss 1970 for policy examples; Frey 1997 for the crowding out of tax morale; Frey and Oberholzer-Gee 1997 for the crowding out of civic duty in a siting context; and Gneezy and Rustichini 1999 for the crowding out of parental discipline in a daycare center. For experimental evidence, see the extensive survey of psychology studies by Deci, Koestner, and Ryan 1999 as well as Falk, Gächter, and Kovacs 1999. For the disruption of "implicit agreements" in the organizational context, see Arrow 1974.

[28] See Geneakoplos, Pearce, and Stacchetti 1989.

to perfect expectation damages, and transaction costs are allocated according to the English legal cost allocation rule, where the loser pays all legal costs. The "long run" in our experiment is nine rounds (or 45 minutes), which we interpret as a conservative test of crowding. In fact, we were surprised that the dynamics unfolded so quickly and that subjects' inclination to trust and to be trustworthy changed in such a short time. The results support the view that institutional changes affect behavior, but they also reveal that, by affecting behavior, institutions affect preferences.

APPENDIX A. A SIMPLE CASE WITH IMPERFECT SIGNALS

Suppose first movers receive, before making their decision to enter a contract, a signal $s \in \mathbf{R}$. The signal technology is the following. If player 2's true type is H, then the signal is $1 + \varepsilon$; ε is normally distributed, with mean zero and variance σ^2. If player 2's true type is M, the signal is $0 + \varepsilon$, and ε comes from the same normal distribution.

How will player 1 decide whether to enter the contract? Nothing changes in the case of

$$p > \frac{a}{1 + a}.$$

She will always enter. (This follows from the fact that she enters even when she knows for sure that player 2 is of type M.) In case of

$$p < \frac{a}{1 + a},$$

player 1 has to update her beliefs about the type of player 2 by using the Bayes rule. She will enter if and only if the probability for player 2 being of type H is large enough. Hence, there will be a critical value $\bar{s}$, such that player 1 enters if $s \geq \bar{s}$ and stays out otherwise.

Given $\bar{s}$ and the signal technology, we can now compute the probabilities with which the two different types are offered contracts. Using these probabilities we can calculate the expected payoffs for both types or, in a population model, the average payoff for both types. Obviously, H types will always get more contracts than M types. (It is more likely that signal s exceeds $\bar{s}$ for H types.) Yet, M types benefit from profitable breach. The dominance of one of these effects depends critically on how noisy the signal is, that is, on variance σ^2.

There are two boundary cases. (i) $\sigma \to 0$ is the case of a perfect signal, and the first effect is stronger than the second (trustworthiness is crowded in). (ii) $\sigma \to \infty$ is the case without a signal, and the second effect is stronger (trustworthiness is crowded out). Obviously, we can now find a critical standard deviation, σ, that induces identical expected monetary payoffs for both types. If the standard deviation is above this level, M types will earn more than H types, and the third result of proposition 2 will be reversed. If the standard deviation is below this level, the original result is resurrected.

APPENDIX B. A STOCHASTIC MODEL OF INDIVIDUAL PREFERENCE ADAPTATION

Consider a large population of individuals who may be heterogeneous with respect to their preferences. Let Ω be the finite set of possible preferences, and let ω be a typical element of this set. When individuals interact, their behavior and earnings depend on the exact situation they face and on

their type. For a given situation (e.g., a game that specifies only monetary payoffs) we can denote the monetary payoff of individual i as $\pi_i(\omega_i, \omega_{-i})$, where ω_{-i} denotes the types of all individuals with whom i is interacting. (In situations with multiple equilibria, this implies that a ready selection criterion is at hand.) Individuals are matched by a matching scheme S, that is, S maps the set of individuals itself. The average payoff earned by individuals of a certain type ω', depends on ω', S, and the current profile of types, denoted by the cumulative distribution function $F(\omega)$, which can be written as $\Pi(\omega',S,F(\omega))$ or as $\Pi(\omega')$. For convenience, we restrict the model to discrete time, t being the time index. Accordingly, let $f^t(\omega)$ denote the share of individuals of type ω at time t. The expression

$$\frac{f^{t+1}(\omega) - f^t(\omega)}{f^t(\omega)} = g(\omega)$$

reflects the growth rate of type ω.

We assume that preference changes occur stochastically and within individuals. Let $q(\omega',\omega'')$ be the probability that an individual's preference ω' changes to ω''. Obviously,

$$\sum_{\omega''} q(\omega',\omega'') = 1.$$

Furthermore, we assume that these probabilities depend on two factors, economic success and conformity.[29] To capture the role of conformity we assume the following.

ASSUMPTION 1 (conformity). *Ceteris paribus*, $q(\omega',\omega'')$ *is proportional to* $f(\omega'')$.

Assumption 1 implies that $q(\omega',\omega'')$ can be written as $f(\omega'')$ times some other function $Q(\omega',\omega'')$. In order to embed the role of economic success, we assume the following.

ASSUMPTION 2 (economic success). $Q(\omega',\omega'')$ *only depends on* $\Pi(\omega'')$ *and is strictly increasing in it.*

This gives rise to a stochastic process in which $F(\omega)$ develops over time. Below we show that, under certain additional (regularity) assumptions, such a process behaves like a growth-monotonic evolutionary process, that is, like a process assumed in our text: Shares of types grow according to their relative economic success.

ASSUMPTION 3 (regularity). *(a) The matching scheme S specifies random matching. (b) The population is large enough for the law of large numbers to apply.*

THEOREM 1. *Under assumptions 1 to 3 the dynamic process of individual preference adaptation behaves like a growth-monotonic evolutionary process, that is,* $g(\omega') > g(\omega'') \Leftrightarrow \Pi(\omega') > \Pi(\omega'')$.

Proof. With assumptions 1 and 3 we can write:

$$f^{t+1}(\omega') = \sum_{\omega} f^t(\omega) Q(\omega,\omega') f^t(\omega').$$

Therefore,

$$g(\omega') > g(\omega'') \Leftrightarrow \sum_{\omega} f^t(\omega) Q(\omega,\omega') > \sum_{\omega} f^t(\omega) Q(\omega,\omega'').$$

Due to assumption 2 $Q(\omega,\omega') > Q(\omega,\omega'') \Leftrightarrow \Pi(\omega') > \Pi(\omega'')$. Hence, the claim follows.   Q.E.D.

The widely used replicator dynamics belong to the class of growth-monotonic evolutionary processes, and it is easy to see when a process of individual preference adaptation behaves like the replicator dynamics.

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[29] For theories of conformity, see Akerlof 1997; Bernheim 1994; Bowles 1999; Boyd and Richerson 1985.

COROLLARY 1. *If*

$$q(\omega',\omega'') = \frac{\Pi(\omega'')}{\sum_{\omega} f(\omega)\Pi(\omega)} f(\omega''),$$

then the process of individual preference adaptation behaves like the replicator dynamics.

Proof. The proof is straightforward.   Q.E.D.

APPENDIX C. SAMPLE INSTRUCTIONS

For the condition of random matching of subjects, medium probability, player 1, and the first phase of the experiment, instructions were as follows.

Welcome to this research project! You are participating in a study in which you have the opportunity to earn cash. The actual amount of cash you will earn depends on your choices and the choices of other persons. At the end of the study, the amount of cash earned will be added to your show-up fee and paid to you in cash.

What the study is about: The study is on how people decide. You are randomly matched with another person present in this room. You and the other person have to choose between two alternatives. The payoff table tells you how much money you earn depending on what you choose and what the other person chooses.

How the study is conducted: The study is conducted anonymously, without communication between the participants, and repeated 9 rounds. Participants are only identified by a letter or a number called "code number." Neither the other participants nor the researcher will ever know how you decide. You are randomly matched with another person after each round. You will never interact with the same person again.

You are person 1.
Start of the study.
Round 1: The payoff table reads as follows.
First you have to choose between A and B.
If you choose A, you and the other person receive 50 cents each.
If you choose B, person 2 gets to choose between Y and Z.
If person 2 chooses Y, you and person 2 receive 150 cents each.
If person 2 chooses Z, chance decides about your earnings.
You earn 150 cents with probability 0.5 (α) and 20 cents with probability 0.5 (β), that is, your expected earnings after a chance move are 85 cents.
The other person earns 120 cents with probability 0.5 (α) and 250 cents with probability 0.5 (β), that is, his or her expected earnings after a chance move are 185 cents.

Payoff Table

Who Decides	Alternatives	Earnings for 1	Earnings for 2
Person 1	A	50	50
	B	$\rightarrow$	$\rightarrow$
Person 2	Y	150	150
	Z	$\rightarrow$	$\rightarrow$
Chance	α (prob = .5)	150	120
	β (prob = .5)	20	250

Now, please open your envelope. It contains 9 decision sheets and a code number sheet. Please take everything out of the envelope. Keep the code number sheet. Then choose

between A and B. Indicate your choice on the decision sheet marked "Round 1," put this decision sheet back into the envelope, and put it into the box which we will pass around. Keep all other decision sheets.

Persons 2 are randomly allocated an envelope and asked to look at your decision and—if they get to make a choice—indicate their choice of either Y or Z on the decision sheet. Decision sheets will be put back into the envelope and into the box.

We collect all decision sheets and count how many people in this room chose A, B, Y, and Z, respectively, and inform all of you of the aggregate outcome of the first round. We then give you the envelope back. Please take the decision sheet out. The information on the decision sheet is private. Please do not share it with anyone else.

Chance now decides whether α or β will be realized in this round. For this purpose we draw a card from a pile with five red and five black cards. Red implies α, black implies β.

We determine your earnings according to your choice and the choice of the other person after the study is over. Your earnings will be paid to you in cash.

End of round 1.

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Process Preferences and American Politics:
What the People Want Government to Be
JOHN R. HIBBING and
ELIZABETH THEISS-MORSE *University of Nebraska-Lincoln*

*W*e present evidence of the kind of governmental processes Americans would like to see in Washington. People believe they have been excluded from current processes, but they do not want direct democracy. The extent to which individuals believe actual processes are inconsistent with their own process preferences is an important variable in understanding the current public mood. Moreover, individual-level differences in level of dissatisfaction with democratic processes help explain variations in public approval of government and in willingness to comply with the outputs of government. Of course, many political attitudes and behaviors are influenced by fondness for the policies that government produces, but it is also the case that sentiments and actions are affected by the way government produces those policies. Far from being merely a means to a policy end, governmental process is important in its own right.

How should democratic government work? This question has been debated by political philosophers since the birth of democracy in Athens 2,500 years ago, but we do not know much about the ordinary American's answer. Understanding public desires concerning the processes and structures of government is crucial for several reasons. Negative attitudes may discourage prospective politicians from serving, sitting politicians (or whole institutions) from tackling controversial policy issues, and ordinary people from participating in politics. They also may embolden some people to view government as less legitimate and possibly even to take lightly their obligation to comply with the outputs of government (see, e.g., Hetherington 1998; Tyler 1990).

Standard investigations tend to be less interested in how the people want their government to run than in which candidates, parties, and policies are preferred. We know people are displeased with interest groups, the campaign finance system, and politicians who are accorded a large staff and salary; we know they dislike Congress more than the president and certainly more than the Supreme Court, and the government in Washington more than state governments; we know they generally view people in government as dishonest and untrustworthy; and we even know something about their attitudes toward selected reform proposals. As useful as all this information has been, it does not tell us the kind of government people want. We know the things that upset the American people, but we do not know much about the larger issue of how they want government to go about its business, to be structured, and to locate power.

This article focuses on public preferences for governmental procedures. Many aspects of modern American politics do not make sense unless consideration is

given to process. Are people satisfied with the procedures they perceive to be employed by government? If not, toward which processes do they want the government to move? What are the consequences when people believe they are not getting the type of government they want? To come to grips with public desires for (and perceptions of) the workings of government, we administered a specially designed national survey to 1,266 randomly selected adults, and we conducted eight lengthy focus groups at locations around the nation. We report primarily on the survey items that measured the processes people prefer and the processes they perceive they are getting.

POLICY-BASED EXPLANATIONS FOR PEOPLE'S ORIENTATION TO GOVERNMENT

What influences people's relationship with their government? According to many scholars, broad societal conditions, which we call policy *outcomes*, are central to this relationship. For example, Nye (1997, 8) explains that feelings about government may go sour if people are "properly unhappy with poor social outcomes." Numerous scholars have followed Lane (1965, 877) in expecting that the public will be pleased with government if the economy is prosperous. Others focus on the match between the policy desires of citizens and the perceived policy decisions of the government, which we call policy *outputs*. In this view, ideological conservatives who are convinced the government is producing liberal policies will be dissatisfied, as will liberals with conservative policies.

The gap between policy preferences and the perceived policy offerings of various parties, institutions, and governments has been a workhorse variable in political research. Scholars argue that it influences the policy choices of institutions and parties (Downs 1957) as well as individuals' party identification (D. King 1997), vote choice (Enelow and Hinich 1984), modes of political participation (Muller 1972), desire for divided government (Fiorina 1996), and general support for the government (Citrin 1974; Miller 1974). In their famous exchange on the meaning of public mistrust, for

John R. Hibbing (jhibbing1@unl.edu) is Professor of Political Science and Elizabeth Theiss-Morse (etheissmorse1@unl.edu) is Associate Professor of Political Science, University of Nebraska-Lincoln, Lincoln, NE 68588-0328.

example, Citrin and Miller disagree on many things, but Citrin (1974, 974) "accepts Miller's main conclusion that policy-related discontent is a source of political cynicism." Citrin (p. 973) describes policy-related reactions thusly: "Political elites produce policies; in exchange, they receive trust from citizens satisfied with these policies and cynicism from those who are disappointed."

Whether the focus is on outputs or outcomes, these policy-based explanations directly challenge our contention that people are concerned not only with government policies but also with how they are produced. Policy-centered explanations have dominated the literature and assume that people will tolerate virtually any procedures as long as these help them obtain favorable policies and conditions. Popkin (1991, 99), drawing on Fiorina (1981) and others, expresses this view well: People "judge government by the results and are generally ignorant of or indifferent about the methods by which the results are achieved."

Empirical evidence is not particularly supportive of policy-based expectations. At the aggregate level, confidence in government dropped most dramatically in the late 1960s, when the economy was doing quite well, and shortly after Lane (1965, 877) declared that the new "age of affluence" would lead to "a rapprochement between men and their government and a decline of political alienation." More recently, Seelye (1998, A15) notes with surprise that "most Americans still deeply distrust the Federal Government despite the end of the cold war, the robust economy, and the highest level of satisfaction in their own lives in 30 years." There is even less support at the individual level. Cross-sectional analyses find no or only a modest relationship between policy satisfaction and institutional approval (Caldeira 1986; Mueller 1973; Patterson and Caldeira 1990). Tellingly, Tyler (1990) finds people's satisfaction with substantive decisions to be unrelated to their tendency to view them as legitimate and, most important, to comply with them. We are convinced that policy satisfaction is an important factor in the public's relationship with government, but previous research suggests that a great deal of variation in this relationship remains to be explained.

In spring 1998,[1] we asked a large random sample of voting-age U.S. residents to identify their general policy or ideological preferences as well as their perception of overall federal policies and those championed by the Democratic and Republican parties. As is the

FIGURE 1. Policy Space

Source: Democratic Processes Survey, Gallup Organization, 1998.
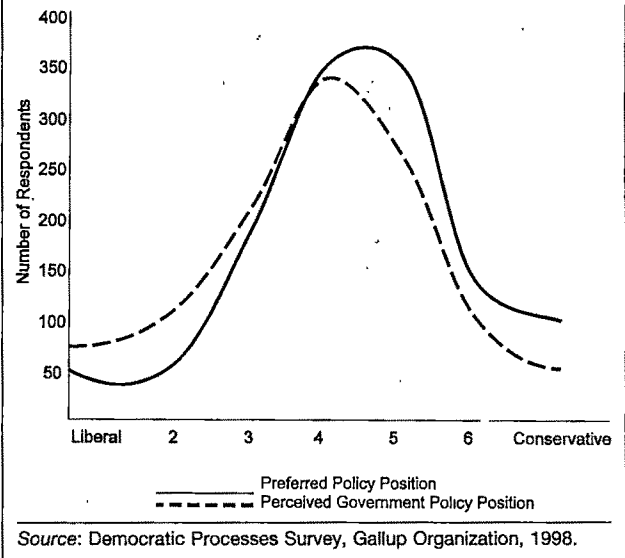
case with parallel items used regularly by the National Election Studies (NES), respondents were presented with the following task:

> Some people hold extremely liberal political views. Think of them as a 1 on a seven-point scale. Other people hold extremely conservative political views. Think of them as a 7. And, of course, there are people in between at 2, 3, 4, 5, or 6. Using the one-to-seven scale, how do you rate [yourself] [the recent policies of the national government in Washington] [the Republican Party] [the Democratic Party].[2]

We begin with a simple comparison of policy preferences and perceptions of recent government policies, as shown in Figure 1. The solid line, which represents public policy preferences, is a pattern familiar to students of modern politics. Americans tend to place themselves in the ideological middle, with a slight tilt toward the conservative side of the spectrum. Fully 72% of respondents located themselves at 3, 4, or 5 on the seven-point scale. Few claim to be policy extremists. The dotted line represents public perceptions of the policy output of the federal government, and these results are somewhat surprising. Most notable is the similarity in pattern between the policies Americans want and what they believe they are getting. In the aggregate the perceived output is a little more liberal than people prefer, but the difference is remarkably small. In terms of mean location, public preferences are at 4.4, and government policies are at dead center,

a difference that is statistically ($p < .01$) but not substantively significant.

The similarity between preferences and perceptions is especially puzzling in view of people's proclivity to complain that the government is out of touch with their needs, concerns, and wants. The participants in our focus groups made such complaints regularly.[3] One person said: "The vast majority of Congress's members have no idea really what the people's wishes are." Another stated that government seemed "very removed from the people." A third commented: "I don't think [elected officials] have any idea about what anyone wants." Participants in the Kettering Foundation focus groups made similar remarks (Matthews 1994, 11–48). The belief that government is out of touch with ordinary Americans is extremely common, but Figure 1 gives no indication that, on the whole, the people see government policies as out of line with their own preferences. In what sense, then, does the public think the government is out of touch?

INTRODUCING PROCESS SPACE

Explanations of public attitudes toward government that rely solely on policy satisfaction are likely to be incomplete. Our main theoretical premise is that attitudes toward the *processes* of government, as apart from the *policies*, constitute an important, free-standing variable that has serious implications for the health of democracy. This idea is not entirely new. The literature increasingly acknowledges that reactions to the way government works are important elements of public opinion. Weatherford (1992, 149) notes that citizen complaints about government could result from "problems with representational linkages (access and responsiveness) or with the elite policy making process (procedural regularity)." Kimball and Patterson (1997) find that disappointment with government is concentrated among those who expect elected officials to be honest, caring, and altruistic but perceive them to be otherwise. Gibson and Caldeira (1995) acknowledge the role of process in explaining public attitudes toward courts in the United States and elsewhere. Hibbing and Theiss-Morse (1995) and Durr, Gilmour, and Wolbrecht (1997) see the always open and often bruising process in the U.S. Congress as a major reason for its consistent lack of popularity with the public (also see Rosenthal 1998, chap. 10, on state legislatures). Tyler (1990) has persuasively demonstrated that public perceptions of "procedural justice" have important consequences, especially for governmental institutions and compliance with those institutions.

Comments made in the focus groups reinforce our conviction that process space deserves to be analyzed alongside policy space. Participants rarely spoke in terms of the Left-Right political spectrum or about policies of any kind. Instead, the discussions were peppered with remarks about the way the government is working or, more frequently, not working. We heard about the people's voice being shut out by combative political parties, self-serving politicians, and demonic special interests. We heard about gridlock across political institutions and sloth and incompetence within them. Perhaps the many references to process should not have surprised us. After all, policy solutions to such perennial societal problems as education, crime, and pollution control are incredibly challenging. People tend to speak more directly and with more confidence about the flawed processes of government than they do about intractable policy dilemmas. The more we listened to them describe their perceptions of government, the more we were taken with the fact that people care deeply about the procedures by which policies are produced. If process perceptions and preferences are as central as they appear to be to the participants in the focus groups, then these attitudes deserve to be studied more thoroughly.

Previous research provides the theoretical justification for studying process, but scholars have paid less attention to measuring public opinion of current processes and, especially, to identifying people's preferred procedures. Indeed, survey instruments rarely include questions about what government processes respondents would like to see. For example, every two years NES asks: "How much attention do you feel the government pays to what people like you think?" It does not ask: "How much attention *should* government pay to what people like you think?" In short, we agree with Weatherford's (1992, 149) conclusion that process attitudes have not been ignored in earlier work, but "their measurement is scattered and unsystematic." We believe these measurement issues must be addressed before process theories and concepts can advance.

Accordingly, our national survey placed respondents on a seven-point scale for process space that is quite different from that delineating policy space. The question used to place respondents on process space is:

> Some people say what we need in this country is for ordinary people like you and me to decide for ourselves what needs to be done and how. Others say ordinary people are too busy and should instead allow elected officials and bureaucrats to make all political decisions. Still others say a combination would be best. Imagine a seven-point scale with 1 being ordinary people making all decisions on their own and 7 being elected officials and bureaucrats making all the decisions on their own, while 2, 3, 4, 5, or 6 indicate in-between opinions on the two extremes. Which number from 1 to 7 best represents ... how you think government should work?

The item is a bit more involved than the traditional policy/ideology question simply because the terms liberal and conservative are familiar phrases, but it is still quite straightforward and boils down to that most basic of all political matters: Who should govern? We also

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[3] The eight focus groups were conducted in late fall 1997: two each in Nebraska, Maine, California, and Georgia. Each consisted of six to twelve participants, recruited either by professional recruiters (in California, the Social and Behavioral Research Institute at California State University, San Marcos; in Georgia, TDM Research of Birmingham, Alabama) or by advertisements, flyers, random telephone calls, and announcements at various civic and social meetings (Nebraska and Maine). Sessions lasted approximately two hours, and participants were paid from $20 to $50 for their time. The sessions were tape recorded.

asked respondents their perceptions of the location in process space of the way the American government currently functions as well as of the process preferences of the Democratic and Republican parties.

One pole of the spectrum is labeled "direct democrat." That is, perhaps through town meetings or electronic coaxial cable connections, people make policy decisions themselves. The other pole represents the notion that policy decisions should be made by elected officials, and these may not (and do not need to) bear much resemblance to decisions that would have been made by the people themselves. The idea is that the people should have some say in the membership composition of political institutions, but once the membership is established, elected officials are left alone to do what they think is best. In this Burkean trustee style of government, public opinion is not directly influential in the determination of public policy (see also Schumpeter 1950). We label this position "institutional democrat." Between these extremes are various options. The influence on decisions by the people (not by political institutions, such as political parties, interest groups, courts, executives, and legislatures) becomes stronger the farther we move away from the institutional democrat pole and toward the direct democrat pole.

This spectrum is unavoidably crude. It ignores many of the important subtleties of ongoing democratic debate. A preferred location close to the direct democrat pole could indicate support for deliberative democracy (see Gutmann and Thompson 1996; Habermas 1995), teledemocracy (Grossman 1995), deliberative opinion polls (Fishkin 1995), or ballot initiatives (Bowler, Donovan, and Tolbert 1998). Institutional democrats are equally diverse. Although people may care about process, they do not have sophisticated conceptions of these variations or of how minority rights should be protected, how interests should be articulated and aggregated, and what specific institutions are needed to approximate given process preferences. Because we are interested in the process ideas of ordinary people and not theorists, a basic process spectrum seems the appropriate place to start.

PROCESS EXPECTATIONS AND RESULTS

Our sense is that Americans typically do not desire direct democracy. This interpretation is consistent with comments made by focus group participants, who often reacted warmly to the idea that the people would have more voice in a direct democracy but quickly raised concerns about its feasibility, the willingness of people to be involved in politics, and the civic abilities of the American public. One focus group exchange expands on this last point.

Sandy: It would be great if everybody got [to express] their, you know, whatever they felt about whatever the issue was. It would be great to know, "Hey, . . . I got my two-cents worth in." But the feasibility of it. . . .

Joan: Well, and I think the public, too, a lot of times is very shortsighted. Those are the problems they want an immediate solution to and they aren't looking 10–20 years down the road. And I mean you could end up with a whole bunch of

FIGURE 2. Process Space

Preferred Process Position
— — — — Perceived Government Process Position
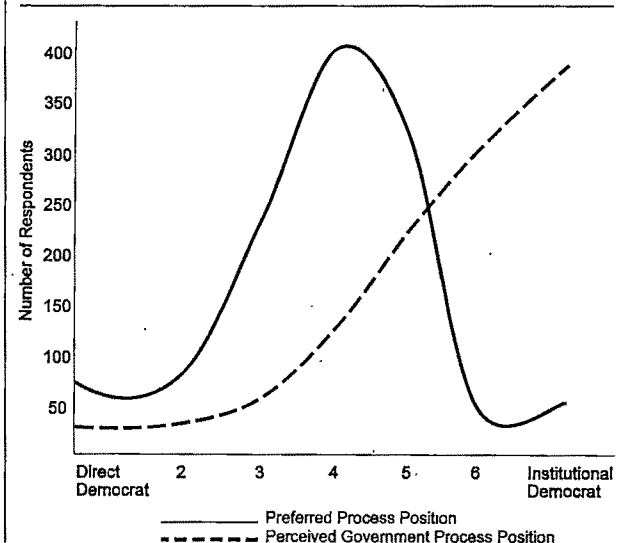
Source: Democratic Processes Survey, Gallup Organization, 1998.

stuff that, that looks great now, boy that's going to be good for four or five years..... But needs change. And problems change. And so elected officials are at least in a position where they can find out more about [it]. . . . They have to look long term and, and try and come up with something that's going to last, . . . hopefully going to be a good solution, . . . for a longer period than just for the immediate.

Our expectation that people do not yearn for direct democracy is certainly at odds with the impression frequently conveyed in media presentations and elsewhere. Americans are often made out to be wide-eyed democrats, willing to adopt any reform—from term limits to expanded initiative possibilities—that would give them more political power. Anthony King (1997, 52) calls Americans "hyperdemocrats" (on various populist sentiments of the public, see Bowler, Donovan, and Tolbert 1998; Citrin 1996; Cronin 1989). Much modern public discourse certainly encourages this conclusion, but we believe that when presented with the stark possibility of making many political decisions on their own, few people respond enthusiastically.

Our more important prediction, however, is that people's process preferences, even though not at the direct democrat pole, will be far to the left of what they perceive as the processes employed by the federal government. Most Americans, we expect, believe governmental processes are inappropriately dominated by elected officials (and the institutions they inhabit) and are insufficiently sensitive to the views of ordinary people. Therefore, they will place the actual workings of government closer to the institutional democrat pole than they place their own procedural preferences. If so, process space may help explain why people think the government is out of touch, even though government tends to provide policies closely approximating the people's desires, as indicated in Figure 1.

In Figure 2, the solid line represents the desires of respondents for processes dominated by the public or by elected officials and institutions. The dotted line

represents perceptions of the process they are getting. The contrast between figures 1 and 2 could hardly be more jarring. As anticipated, the preferred processes balance the input from ordinary people and elected officials. Of the 1,253 usable responses, 975 (78%) register a preference at 3, 4, or 5 on the seven-point scale. Only 119 want officials to have largely unfettered decision-making power once elected (a preference at 6 or 7). More surprisingly perhaps, only 170 want ordinary people to be in charge (a preference at 1 or 2). People definitely want elected officials to be involved in decision making and do not want governmental institutions to go away.

In Figure 1, we observed that people both desire and believe they are receiving centrist policies. The solid line in Figure 2 indicates they also want centrist processes, but the dotted line reveals they believe they are not getting them. More than 55% perceive current processes as dominated by elected officials and institutions (6 or 7 on the scale). Only 5% feel the public has a great deal of influence (1 or 2). The mean placement of respondents in process space is 4, compared to a 5.4 for perceived processes of government, a highly significant and substantive difference ($p < .01$). The lines in Figure 2 support the conclusion that many people are not getting the governmental processes they want. When combined with Figure 1, these results invite the interpretation that public beliefs about an out-of-touch government have more to do with processes than with policies; that is, with the way decisions are made rather than their specific content.

WHICH PROCEDURES DO THE PEOPLE BELIEVE THE TWO PARTIES ESPOUSE?

Another puzzle left unsolved by the policy space explanation involves public views of American political parties. No less common than the view that government is out of touch with ordinary people are such comments as "the parties are both the same," "there is not a dime's worth of difference between them," and the parties are as similar as "Tweedledee and Tweedledum." Grave implications attend such beliefs because modern democracy hinges on the competition between distinct parties. If they are not seen as distinct, any competition will be perceived as a farce (Ginsberg and Stone 1986).

A belief in the similarity of the parties is not merely the stuff of hackneyed phrases. It is consistent with the findings of previous survey research and with the comments we heard in the focus groups. For example, Wattenberg (1981, 943–4) reports that approximately half the public disagrees with the statement that there are important differences between the two parties (for more evidence, see Margolis 1977; Pomper 1972, 419). Although such sentiments may have diminished in recent years, they are still much in evidence among our focus group participants. One stated: "But what's bad is even the two parties are so dag-blasted confused anymore.... If you look up what was [once] a true Democrat or what was a true Republican, we don't have that." Another said: "I've heard the current parties referred to as the Republicrats. And to me that

FIGURE 3. People's Perceptions of the Policies and Processes Preferred by the Two Major Parties
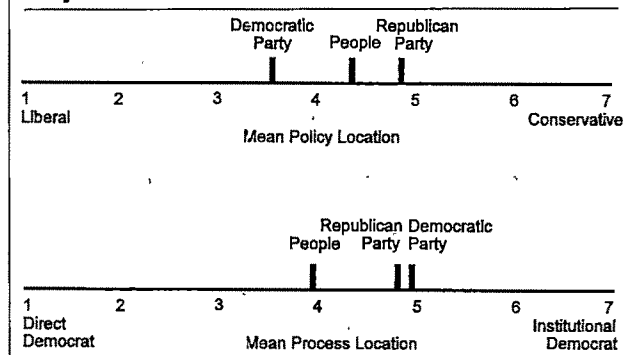
Source: Democratic Processes Survey, Gallup Organization, 1998.

really seems to describe what goes on. Whether we elect Democrats or Republicans, the only difference is chocolate cake with vanilla frosting or vanilla cake with chocolate frosting." Millions of people are convinced the parties are the same.

Contrast this attitude with the placement of the parties in policy space, as shown in Figure 3. For purposes of clarity, we represent only the mean location, not the full distribution of responses (see the top portion of Figure 3). The people place themselves at 4.4 on the policy scale, with 7 being conservative. They place the Democratic Party at 3.6, to the left of the public mean, and they place the Republican Party at 4.9, to the right of the public mean. This difference of 1.3 between the two parties in policy space is highly significant, both substantively and statistically ($p < .01$). On the whole, people see the parties as distinct regarding the policies they advocate, which does not square with the popular belief that the parties are the same.

Again, process perceptions may explain the anomaly. Whereas people seem to believe the parties espouse different policies, they may view them as nearly identical in terms of processes. After all, both parties are heavily involved in financing candidates and in scoring debating points. Both run nasty campaigns and make promises they often cannot keep. Both are heavily intertwined with special interests and the cocktail-party circuit. These are the features that most upset people, and these are likely the features people have in mind when they bemoan the similarity of the political parties.

Empirical results support this logic (the bottom portion of Figure 3). In policy space, most people (64%) believe themselves bracketed by the two parties (usually with the Democrats to the left of them and the Republicans to the right), but in process space, most people (64%) see both parties as being to their right. In other words, both parties support a process in which elected officials and institutions are influential in decision making. Respondents placed the Democratic Party at 4.94 and the Republican Party at 4.85, a difference that is not significant either substantively or statistically ($p = .17$). Thus, in contrast to policy space, both parties are to the right of the public's preferred process position, both are perceived nearly a

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full point away from the public, and both are seen as espousing similar process ideas. In the eyes of ordinary people, the parties are separated by more than one point on their policy positions but by less than one-tenth of a point on their process positions. People believe that neither party is eager to loosen its grip and give ordinary people more say.

Compared to policy perceptions, people are less likely to view the process preferences of their own party much more favorably than those of the opposing party. That is, the partisan bias in assessments of process preferences is much less pronounced than in assessments of policy preferences. Excluding independents, the mean gap between respondents' own policy preferences and those of their own party is 1.15, compared to a mean gap of 2.64 with the opposing party, a substantial difference of 1.49. When it comes to process space, however, the mean gap between self and own party is 1.35, compared to 2.22 for the opposing party, a difference of only .87. Partisan bias does not seem to have as much influence in process space.

THE CONSEQUENCES OF PROCESS DISSATISFACTION

Our use of the vague phrase "the relationship between people and their government" conveys our interest in a set of variables broader than voting behavior and party identification. Indeed, the results just presented indicate why it would be erroneous to expect process perceptions to help people decide whether they are Democrat or Republican or whether to support candidate A or candidate B. The public does not believe the Republicans and Democrats are very different in terms of process, so process factors are of little use in such tasks as voting decisions. (We would maintain, however, that support for the presidential candidacy of H. Ross Perot was driven primarily by process rather than policy preferences, just as dissatisfaction with "process as usual" may on occasion work against certain incumbents; see also Hetherington 1999.) Assessments of individual officeholders also are not likely to be affected by process concerns because people believe virtually all politicians are accomplices in promoting flawed democratic processes. We expect process concerns to play a much larger part in such broad variables as whether people approve of government and whether they view it as legitimate and therefore are willing to comply with the laws it produces.

We first test the hypothesis that the more people believe their process preferences are embodied in the actual workings of government, the more they will approve of government, even controlling policy outcome and policy output satisfaction. Those people who are generally pleased with policy outcomes (an improving financial situation) and with policy outputs (those believing their own overall policy preferences are reflected in recent governmental actions) should be more approving of government. But we maintain that people are often motivated not just by policies but also by their perceptions of how these policies were made.

To test this hypothesis, we developed a model consisting of the usual demographic and political variables

and perceptions of policy outcomes, policy outputs, and governmental processes. (See the Appendix for definitions and question wording for independent variables.) Each respondent's satisfaction with policy outcomes was measured by standard questions on whether their own financial situation and that of the nation had improved, stayed the same, or declined during the previous year (*Personal Financial Condition* and *Country's Financial Condition*). These questions do not cover every aspect of outcome satisfaction, but they measure a central element of overall conditions—perceptions of the economy. To measure each respondent's location in policy space, we computed the absolute value of the gap between policy preferences and perceived policy realities (*Perceived Policy Gap*). These gaps could range from 6 (a person who claims to be extremely liberal but who sees federal policies as extremely conservative, or vice versa) to 0 (a person who believes government policy outputs perfectly match his or her preferences). A parallel gap measure was constructed for process space, indicating the extent to which each person sees preferred and actual processes as being the same (0) or different (up to 6) (*Perceived Process Gap*). *Approval* of government was measured by the standard approval question: "Please tell me if you strongly approve, approve, disapprove, or strongly disapprove of the way different entities have been handling their job lately. How about ... the federal government?" Approval could range from strongly disapprove (1) to strongly approve (4).

We see from Table 1 that the demographic variables are not particularly useful in specifying the kind of person likely to be dissatisfied with government. Among the political variables, the politically knowledgeable are more likely to approve of the federal government as are those who identify with the Democratic rather than Republican Party. The real questions of interest pertain to the policy and process variables. With all the control variables entered, are those who are pleased with policy more likely to approve of government than those who are displeased? Yes. Both policy outcome variables—personal and sociotropic financial conditions—are related to approval of government, although the sociotropic formulation is clearly the more powerful, as previous research would lead us to expect (Kinder and Kiewiet 1979). The policy gap variable has the expected negative sign and is highly significant. The farther governmental policies are from a person's own preferences, the less that person is satisfied with government. But do people only care about policies or do they also want these policies to be produced in a fashion they find acceptable? Table 1 offers a clear verdict. Process matters. Even with all the other controls included, particularly those for policy outcomes and policy outputs, a process that matches a person's preferences for how the process should work increases approval of government. Apparently, people's approval of government is driven by more than just policy concerns. It is also driven by perceptions of the extent to which processes match what people desire processes to be.

We next concentrate on willingness to comply with

TABLE 1. Explaining Variation in Government Approval and Compliance with the Law

Variable	Approval b	s.e.	Compliance b	s.e.
Age	−.029	.034	.198**	.050
Income	−.030	.030	.091**	.035
Race	.037	.020	.097**	.022
Gender	−.005	.014	.023	.016
Education	.017	.026	−.021	.029
External Political Efficacy	.049	.028	−.014	.027
Political Knowledge	.083**	.019	−.025	.039
Party Identification	−.071**	.021	.009	.024
Personal Financial Condition	.045*	.022	.032	.025
Country's Financial Condition	.090**	.021	−.001	.023
Perceived Policy Gap	−.160**	.029	−.034	.034
Perceived Process Gap	−.095**	.030	−.117**	.034
Constant	.485**	.034	.546**	.039
F	15.12**		4.96**	
Adj. R^2	.19		.06	
N	726		732	

Source: Democratic Processes Survey, Gallup Organization, 1998.
Note: *p < .05; **p < .01. All variables have been transformed to range from 0 to 1.

the law (*Compliance*), which in many respects is the most important aspect of people's relationship with their government. Tyler (1990) correctly notes that compliance is central to the viability of authority; if noncompliance is widespread, the system of government created to manage problems will not be able to do so. If a disjuncture between desired and perceived processes hampers compliance, then it is powerful evidence that process perceptions are anything but innocuous.

We measure the tendency to comply with the law with a straightforward survey question: "People should obey the law even if it goes against what they think is right." The response options for this item ranged from 1 (strongly disagree) to 4 (strongly agree). When compliance tendencies are regressed on the independent variables described above, we anticipate that, even after controlling for dissatisfaction with policies, process concerns will still exert an independent influence on compliance. Turning again to the results displayed in Table 1, we find that only four of the twelve variables are related to compliance. Increasing age and increasing income bring a tendency to comply as does being a person of color. Notably, policy gap is not related to

compliance tendencies. Even with the other variables in the model controlled, however, the size of the difference between perceived and desired governmental processes (process gap) is strongly and negatively related to compliance. As hypothesized, regardless of their level of satisfaction with policies, people displeased with the process by which those policies are made are less likely to feel the need to comply with the law. People appear to be motivated less by the extent to which they get what they want than by the way in which decisions are made.

It is always possible that the causal direction is not the one theorized. In this case, perhaps process preferences result from a tendency to comply with the law rather than compliance being the result of process dissatisfaction. Such a reverse relationship can seldom be ruled out completely, but the theoretical and logical bases for expecting it here seem tenuous. The contention would have to be that willingness to comply with the law causes a person to prefer and to perceive certain governmental processes. This means that, because a person thinks it is not necessary to comply with laws, she will then conclude that people should be more influential in making laws than she believes them to be now. We think it much more compelling to argue that process preferences cause compliance. If a person does not think people are given enough influence in the formulation of laws, she will be less likely to comply. Not coincidentally, this is the formulation favored by previous work on compliance with the law (see especially Tyler 1990).[4]

CONCLUSION

Process matters. This does not mean people's policy concerns are unimportant, only that we cannot fully understand their orientation toward government without taking into account how the public thinks government ought to work and how it thinks government works in practice. Just what processes do they want, and what processes do they think they are getting? People are much less pleased with the government's

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[4] These theoretical points are important because empirical refutations of reverse causation are almost never conclusive, particularly with one-shot survey data. Such tests require variables that are strongly related to the key hypothesized independent variable (in this case, process gap) but are completely unrelated to the dependent variable (in this case, compliance with the law). If these are available, the predicted values of process gap can serve as an instrumental variable, but in most cases such variables do not exist.

A few items in our survey are marginally suitable for the task at hand. Not surprisingly, items such as "if the American people were just given a chance, they could figure out how to solve this country's problems," "people should be allowed to vote directly on policies through ballot initiatives and the like much more often than they do now," and "members of Congress should do what they think is best regardless of what the people in their districts want" are all related to the process gap variable but are not related to compliance tendencies. The results obtained when using a "purged" variable created from these items generally support our contention that process preferences affect compliance and not the other way around (i.e., parameters are not markedly weakened when substituting predicted process gap for actual process gap), but we are reluctant to make much of this fact since the predicted variable only correlates with the actual at .32, which renders it a questionable instrument at best.

processes than its polices. The mean gap between desired and perceived process on the seven-point scale is 2.1, compared to 1.7 on the policy scale (the difference is significant; $p < .01$). More important, in process space the discrepancy between preference and perception follows a definite pattern. Whereas in policy space approximately equal portions believe government policies are too liberal (55% of those perceiving any bias at all) or too conservative (45%), in process space a far larger portion believes elected officials have too much influence in decision making (80% of those seeing any imbalance) than believes ordinary people have too much influence (20%). People want policies to be a balance of liberal and conservative ideas, and in the aggregate that is what they think they are getting, more or less. People want decision making to be a balance between elected officials and ordinary people, but they think they are getting a process dominated by officeholders. From their vantage point, public enthusiasm for populist reforms is designed only to reestablish the balance, and until processes more closely approximate that balance, the ostensibly flawed governmental procedures will reduce the inclination of a surprising number of people to approve of government and to fulfill that most minimal of civic responsibilities: compliance with the law.

APPENDIX

We measured the demographic and political variables as follows:

Age: Coded as reported age.

Income: Total household income in 1997 before taxes coded from 0 = under $5,000 to 12 = $100,000 and over.

Race: Coded 0 = white, 1 = nonwhite.

Education: Coded 0 = less than high school to 7 = postgraduate or professional degree.

External Political Efficacy: A scale created by summing agree or disagree responses to the two standard questions—"People like you have a say about what the government does" and "Public officials care a lot about what people like you think"—and coded so that higher scores signify more efficacious responses (alpha = .42).

Political Knowledge Index: The sum of correct responses to four factual questions on the job or political office held by Al Gore and by Tony Blair, on who has the final responsibility to decide if a law is constitutional, and on which party currently has the most members in the U.S. Senate.

Party Identification: Coded 0 = strong Democrat to 6 = strong Republican.

Personal Financial Condition: Coded 1 = worse off, 2 = same, and 3 = better off for the question: "We are interested in how people are getting along financially these days. Would you say that you are better off financially, worse off, or just about the same as you were a year ago?"

Country's Financial Condition: Coded 1 = worse off, 2 = same, and 3 = better off for the question: "We would like to know your views about the nation's financial well-being. Is the nation better off, worse off, or about the same financially as it was a year ago?"

Perceived Policy Gap: Measured by taking the absolute value of respondents' self-placement on the ideology scale minus their perceptions of the recent policies of the national government—"We hear a lot of talk these days about liberals and conservatives. Some people hold extremely liberal political views. Think of them as a 1 on a seven-point scale. Other people hold extremely conservative political views. Think of them as a 7 on a seven-point scale. And, of course, there are people in between at 2, 3, 4, 5, or 6. Using the 1–7 scale, with 1 defined as extremely liberal and 7 as extremely conservative, how do you rate ... yourself? ... the recent policies of the national government in Washington?"

Perceived Process Gap: Measured by taking the absolute value of respondents' self-placement on the process scale minus their perception of the national government—"Some people say what we need in this country is for ordinary people like you and me to decide for ourselves what needs to be done and how. Others say ordinary people are too busy and should instead allow elected officials and bureaucrats to make all political decisions. Still others say a combination would be best. Imagine a seven-point scale with 1 being ordinary people making all decisions on their own and 7 being elected officials and bureaucrats making all the decisions on their own, while 2, 3, 4, 5, or 6 indicate in-between opinions on the two extremes. Which number from 1 to 7 best represents ... how you think government should work? ... how you think the national government in Washington *actually* works?"

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Convergence and Restricted Preference Maximizing under Simple Majority Rule: Results from a Computer Simulation of Committee Choice in Two-Dimensional Space
DAVID H. KOEHLER *American University*

*R*ecent analyses of collective choice predict convergence among the outcomes of simple-majority decisions. I estimate the extent of convergence under restricted preference maximizing through a computer simulation of majority choice by committees in which individual decisions on proposal location and voting are constrained. The simulation generates distributions of majority-adopted proposals in two-dimensional space: nondeterministic outcomes of simple-majority choice. The proposal distributions provide data for a quantitative evaluation of the effects on convergence of relaxing conventional preference-maximizing assumptions. I find convergence of majority-adopted proposals in all cases, and that convergence increases under restricted proposal location. Moreover, under some voting restrictions, experiments yield stable outcomes that demonstrate remarkable convergence. I conclude that restricted preference maximizing generally increases the probability that simple-majority outcomes reflect the central tendency of member preference distributions. Since committees and legislatures are important formal procedures for democratic collective choice, this conclusion applies to a large class of political decisions.

Spatial models of simple-majority decision making by committees demonstrate that stable outcomes exist only under very restrictive conditions. In the unlikely event that those conditions are realized, a stable equilibrium proposal can be identified; it would be preferred by a majority to every possible alternative. That stable outcome, the multidimensional median located at the center of the preference space, would be the unique, best possible representation of the individual preferences of all committee members.

Under normal circumstances, absent the restrictive conditions, the multidimensional median does not exist as a single point but as a relatively small subset of points in the center of the preference space. Furthermore, there is no stable proposal; any proposal adopted by a committee can be defeated on a simple majority vote by some other proposal. In these circumstances the question of the location of unstable, majority-adopted proposals is critical. Specifically, to what extent do unstable proposals converge on the median set, the center of the preference space, and how is convergence affected by constraints on the structural conditions that define majority choice by committees? In other words, how well do unstable, majority-adopted proposals represent the preferences of committee members?

To research this question a computer model was developed to simulate majority decision making by committees. Simulations were run under conventional preference-maximizing assumptions to estimate the extent of convergence attributable to simple-majority

voting. Restrictions were then imposed on committee members that limited their options with respect to both location of proposals in the preference space and their voting decisions once a proposal was under consideration. The resulting data were used to estimate the extent of convergence under conventional preference maximizing and to see how convergence is affected by structural constraints on proposal location and voting. The object is to evaluate the representation of member preferences when majority choice by committees is unstable and when preference maximizing is restricted.

In response to the global-cycling results (Cohen 1979; McKelvey 1976, 1979; Schofield 1978), several related approaches to predicting outcomes of committee choice with endogenous (internal) agenda formation have been investigated. First, a number of game-theoretic solutions demonstrate reasons for members to submit proposals for consideration by a committee that imply convergence. Convergence is indicated when majority-adopted outcomes are contained in a proper subset of the Pareto set formed by committee members' ideal points. The results are in the form of predictions that committees will choose outcomes in the uncovered set (Miller 1980), the Banks set (Banks 1985), the minmax set (Kramer 1977), or the competitive solution set (McKelvey, Ordeshook, and Winer 1978), and so on. It is possible to show that these solution sets generally are subsets of the Pareto set, but they predict unique outcomes only under very restrictive conditions. Furthermore, the location and size of the solution sets is not obvious, given arbitrary sets of member preferences (ideal points).

A second line of attack includes development of the generalized median set, the yolk, as an alternative to the typically nonexistent "median in all directions" (Davis, DeGroot, and Hinich 1972). A number of geometric constructions have been formulated that describe the uncovered set, the Banks set, and other predictions (e.g., the strong point) and that indicate how these respond to differences in committee mem-

David H. Koehler is Professor Emeritus of Political Science, American University, Washington, DC 20016.

bers' preferences as expressed by the yolk (Feld et al. 1987; Feld, Grofman, and Miller 1988; Feld, Grofman, and Miller 1989; McKelvey 1986; Miller, Grofman, and Feld 1989). Feld et al. (1987) show that in Euclidean space the uncovered set is contained within 3.7 radii from the center of the yolk; that the strong point is in the uncovered set; that the Banks set is a subset of the uncovered set; and that the Schattschneider set is a subset of the Banks set. These constructions confirm predictions of convergence except for very small committees, but they improve only marginally on the predictive capabilities of the original solution sets. Furthermore, for committees of even modest size (>9) the geometric procedures are extremely cumbersome.

A third approach concedes the impossibility of making specific predictions with respect to outcomes and instead relies on nondeterministic models of committee choice (Ferejohn, Fiorina, and Packel 1980; Ferejohn, McKelvey, and Packel 1984; Packel 1981). Using a number of different Markov voting models, researchers established the existence of stationary outcome distributions. In the case of majority voting, upper bounds were put on the limiting probability distribution around the yolk: More than 95% of the outcome probability lies within 9 radii of the center (Ferejohn, McKelvey, and Packel 1984).

Although this brief review scarcely does justice to a very impressive body of work, it reveals what is known about the parameters of committee choice and the questions that have stymied further development of the models. Both scientifically and aesthetically it would be preferable to go forward using analytic rather than computational models, but the issues that remain primarily involve measurement and estimation, and simulation can produce answers that are presently impossible to reach using analytic models. Simulation results reported in the literature extended analytic conclusions to estimate the parameters (rather than bounds) of outcome distributions for committee choice under a variety of constraints on status-quo location, agenda length, and the decision rule (Koehler 1996, n.d.). In brief, it was shown that location of the status quo has little effect on outcome convergence, that very short agendas can inhibit convergence, and that supramajority rule can induce convergence and/or stability.[1]

At present, there is agreement that outcome convergence follows from simple-majority rule by committees with endogenous agenda formation and that unstable outcomes of committee choice are confined to a relatively small and centrally located subset of the Pareto set (Ferejohn, McKelvey, and Packel 1984; McKelvey 1986; Miller, Grofman, and Feld 1989). The question

addressed by this research is how large in Euclidean space is the relatively small, centrally located subset of outcomes, and how is the size of that subset affected by restrictions on the behavior of committee members. In short, this study concerns the effects on convergence of restricted preference maximizing.

Questions about preference-maximizing assumptions are not new. In the discussion of his global-cycling results McKelvey (1976) called attention to the requirement that an agenda setter must have the information necessary to determine precise locations for alternatives in the preference space, and that voters must be "able to make fine distinctions between alternatives without becoming indifferent" (p. 481). He went on to note: "If voters cannot make such fine distinctions, this could impose some limits on the space of intransitivities" (p. 481). The stringent requirements on proposal location and fine voting choices were noted subsequently by Feld, Grofman, and Miller (1989) in their demonstration of limits on agenda control imposed by the length of the agenda, the size of the yolk, and the location of the status quo. Although Feld, Grofman, and Miller did not analyze the effects of relaxing the maximizing assumptions, they argue that if the conditions were not realized, "limits on agenda control would be significantly greater" (p. 407). In both works, the conjecture was that proposal location and voting choices that fall short of strict preference maximizing are likely to mitigate the effects of instability and manipulability that follow from simple-majority choice by committees.

More recently, Tovey (1991) reported findings that result from relaxing a number of assumptions in the conventional, Downsian, spatial voting model.[2] Of particular interest, Tovey shows that if committee members fail to make fine voting choices, the probability of stable committee choice is 1.0 as the size of the committee approaches infinity.[3] Tovey's conclusions are surprising, given the instability results that characterize the spatial analysis of simple-majority rule. In one sense, however, the analysis yields too much of a good thing; the "size" requirements that drive his theorems limit their application. The question that

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[1] Simulation also has been applied fruitfully to elections (e.g., Demarchi 1999; Kollman 1992). Although outcome instability is a problem for both committees and elections, the structure or procedures for collective decision making are considerably different. It makes sense to model electoral parties as adaptive agents; they set the agenda and are free to adjust it in the face of new information. Yet, committee members are not adaptive agents. Individuals submit proposals for consideration by the committee, each proposal is voted up or down, and a new member is recognized to submit the next proposal. There is no game tree, and the ability to look ahead is limited because one does not know who will be recognized next.

[2] The conventional model entails an odd-numbered set of members with simple Euclidean preferences defined over an m-dimensional preference space, although many analyses are based on $m = 2$. Agenda formation is endogenous, and agendas are forward moving (Wilson 1986). Each proposal to amend a pending proposal is disposed of on a simple majority vote, and each member votes for the alternative in the preference space that is closer to her or his ideal point.

[3] Other assumptions investigated by Tovey that deviate from the conventional model include an advantage to the incumbent or the status quo. This was operationalized by assuming the probability of abstention by supporters of the status quo is less than the probability of abstention by supporters of change. In a related model it was assumed that a certain number of committee members always vote for the status quo regardless of their spatial location. Both assumptions are equivalent to increasing the decision rule for majority choice. The effects of that modification have been reported elsewhere (Koehler 1996). Tovey's last two models concern descretization: Utilities or proposal locations are measured only in whole numbers. These assumptions are similar to perceptual thresholds and the prohibition on trivial changes that are the basis of the last models tested here.

remains is, under assumptions of *finite* committee size, what is the effect on majority-choice convergence of relaxing the strict preference-maximizing assumptions.

I report on a computer simulation of majority decisions by committees of size $n(21 \leq n \leq 101)$ over a two-dimensional Euclidean choice space.[4] The first model was designed to generate distributions of win-set maximizing proposals using an endogenous, forward-moving agenda. These conditions are consistent with the conventional preference-maximizing spatial model. Win-set maximizing proposal distributions provide data for comparison to the results from experiments under restricted choice conditions.

Subsequent models explored restrictions on proposal location. These include random location of proposals, location of proposals at committee members' ideal points, and constrained maximizing (committee members locate proposals in the win sets of pending proposals at points other than their maximum preference points). Voting also is susceptible to restrictions on preference maximizing. Individual voting restrictions were modeled by requiring a proposal to amend a pending proposal to be at least a finite distance, ε, closer to a member's ideal point in order to gain her or his vote. Collective voting restrictions were modeled by requiring that a majority-adopted proposal change the pending proposal by some minimal amount, δ. Under relaxed preference-maximizing assumptions, simulated committee choice generates distributions of majority-adopted proposals and, in some cases, stable outcomes. These are compared to ideal-point distributions and to data from the initial, win-set maximizing experiments.

The findings are, first, that restrictions on proposal location generally improve the convergence of majority-adopted proposals on the central tendency of ideal-point distributions, that is, the center of the preference space.[5] Second, individual and collective voting restrictions have little effect on the convergence of unstable outcomes. Third, in a number of simulations stable outcomes were reached; no proposal submitted under the extant constraints on preference maximizing was preferred to the pending proposal by a majority of the

committee. Stable outcomes consistently demonstrate impressive convergence on the center of the preference space. Compared to results from the conventional spatial model of committee choice under simple-majority rule, the conclusion is that enhanced convergence is the norm for restricted committee choice, and highly convergent stable outcomes frequently are reached.

These results are not intended to be hypotheses about outcomes of committee voting in the real world. In the following section a case is made for a variety of proposal-location and voting strategies that could appeal to committee members in different circumstances. Unless it is possible to specify a choice among strategies, there are insufficient grounds for predicting the extent of outcome convergence. What the results do reveal are characteristics of the choice process: the effects on majority-choice convergence of the set of structural conditions used to transform preferences, expressed by distributions of ideal points, into distributions of outcomes. Different approaches to that choice process, among which strict preference maximizing is but one possibility, yield different estimates of convergence. Increased outcome convergence implies more efficient collective choice procedures, and that is of great importance to the analysis and practice of democratic decision making.

In the sections that follow, a model of majority choice is described, and simulation models are then developed for committee choice under restricted preference maximizing. Next, the results are presented, followed by a discussion of the conclusions.

A MODEL OF MAJORITY CHOICE BY COMMITTEES

Committee choice occurs on the question of agreeing to a measure (a bill or resolution) as opposed to a status quo, ϕ. Measures are subject to motions to strike, to insert, or to strike and insert (amendments). In short, measures and motions are proposals to alter the language that specifies the status quo on an issue.

To model majority choice, assume that committees consist of odd-numbered sets of members, $N = \{1, 2, \ldots, n\}$, with preferences defined over two-dimensional Euclidean space. Member i is assumed to have an ideal point, x^i ($i = 1, \ldots, n$), and preference, P_i, is a decreasing function of the distance from a proposal to her ideal point. Preferences in two-dimensional space are modeled by circular indifference curves centered on committee members' ideal points. Proposals are denoted $x, y, \ldots \in X$, where X is the set of feasible proposals, the choice space, and $X \subseteq R^2$. A proposal y is preferred by a committee to the pending proposal x, yPx, if and only if

$$|yP_ix| > |xP_iy|,$$

where $|\cdot|$ is the number of members who agree with the enclosed preference relation.

The win set of proposal x is the set of proposals preferred by a committee to x:

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[4] There are several reasons to limit the simulation to two dimensions. First, it has been demonstrated convincingly that the votes of legislators in the real world can be explained on two dimensions, and that additional dimensions increase the explained variation very little (Poole and Rosenthal 1985). Second, it has been shown that variance is additive with increases in dimensionality of a preference space (Koehler 1996), so little new insight is likely to be gained from running the simulation in, say, three dimensions. Third, virtually all analyses of spatial models of committee voting are limited to two dimensions. This makes geometric representations possible, and it usually is the case that the results apply to higher dimension (Miller, Grofman, and Feld 1989).

[5] The optimal location of majority-adopted proposals is assumed to be the central tendency of the ideal-point distributions, or the two-dimensional mean or median of voter preferences. Given uniform distributions of ideal points, the mean and median as well as the center of the yolk are expected to coincide at the center of the 10 × 10 frame. Furthermore, since there is no reason to assume that proposal distributions will deviate systematically in any direction from the location of the ideal-point distribution, the expected location of proposal distributions, the two-dimensional mean and median, also is at the center of the frame. That point is referred to as the center of the preference space.

FIGURE 1.   Win Sets for a Seven-Member Committee

A. *W(x)*

B. *W(y)*

C. *W₌(x), ε > 0.0*

D. *W₌(y), ε > 0.0*
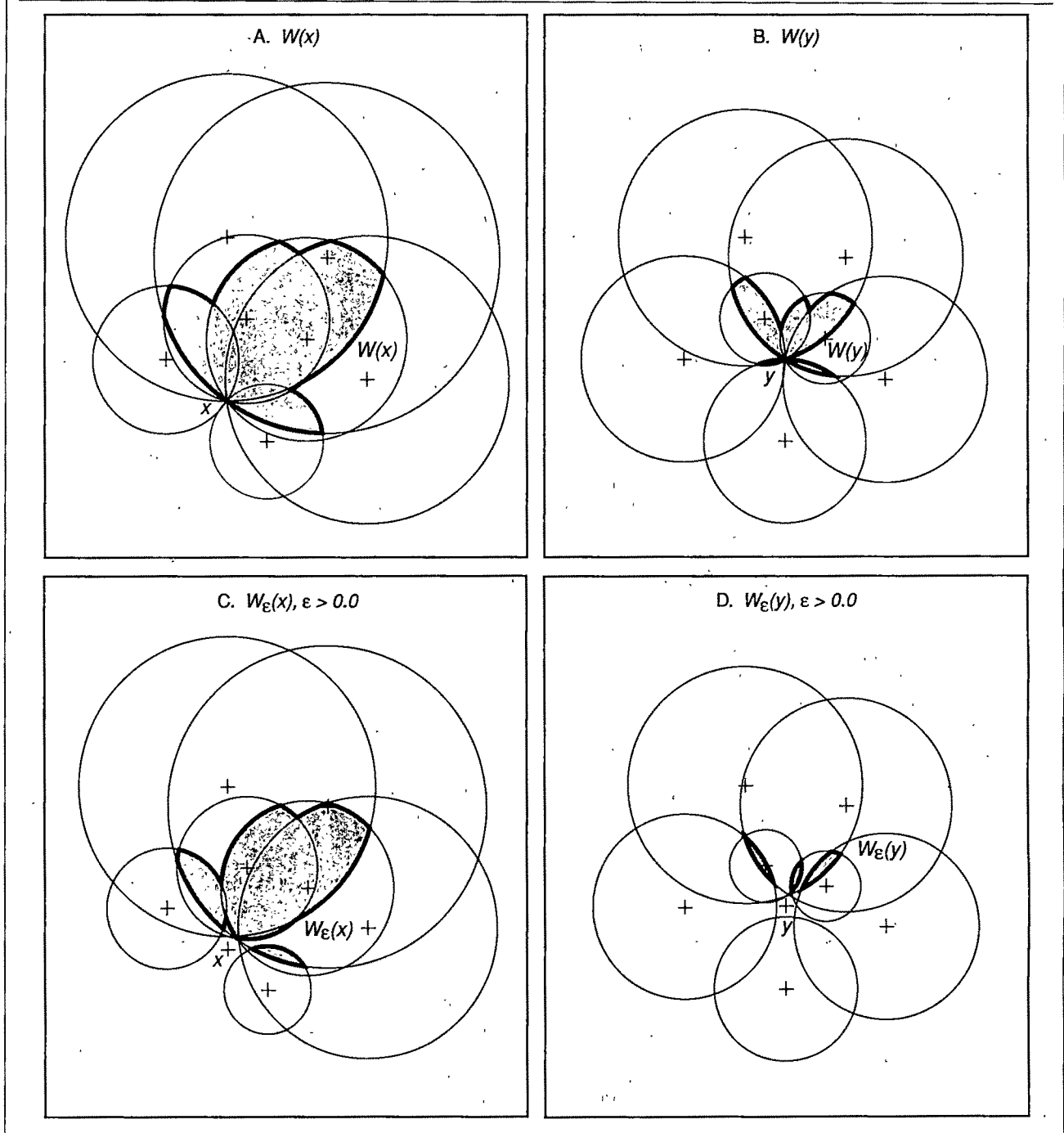
$$W(x) = \{y \in X \mid yPx\}.$$

If x is the pending proposal, the win set of x is determined in two-dimensional Euclidean space by constructing circular indifference curves, centered on committee members' ideal points, x^i, and that intersect x. The indifference contours indicate that member i prefers every point inside the circle to x, prefers x to every point outside the circle, and is indifferent between x and every point on the circle. The set of points inside the indifference contour through x, $P_i(x)$, is called the preferred set of i. The win set of x, $W(x)$ is

formed by the intersection of the preferred sets of a majority of committee members. For example, Figure 1A shows a set of ideal points in X for seven committee members, their indifference curves through pending proposal x, and $W(x)$. Every point in $W(x)$ is preferred by the committee to x, and x is preferred to every point outside $W(x)$. A proposal in the tie set, the boundary of $W(x)$, loses to the pending proposal.

In this analysis, collective or committee preference is expressed by the generalized median set (Ferejohn, McKelvey, and Packel 1984). The *yolk*, as it is com-

monly called (McKelvey 1986), is the smallest ball that intersects every median hyperplane. Median hyperplanes partition an m-dimensional preference space such that at least $n/2$ ideal points are on and to each side of the surface. In two-dimensional space, the yolk is the smallest circle that intersects every median line. The radius of the yolk indicates the extent to which "the voter distribution deviates from one that would generate a majority-rule equilibrium" (Feld, Grofman, and Miller 1989, 405). The radius of the yolk, r, determined from majority partitions of odd-numbered distributions of ideal points, measures the extent to which committee preference converges on the center of the preference space; r decreases with increased preference convergence.

THE SIMULATION OF MAJORITY CHOICE

Model 1, which assumes endogenous formation of a forward-moving agenda and strict preference maximizing, generates distributions of majority-adopted proposals. The model 1 procedure, simple win-set maximizing, is consistent with the conventional spatial model of simple-majority choice by committees. To set up a committee decision, n ideal points ($21 \leq n \leq 101$) and a status quo, ϕ, are selected randomly from a uniform distribution over a 10×10 "frame" in the choice space, X. Next, a mover is recognized, selected randomly from the committee, to submit a measure to replace ϕ or, subsequently, to make a motion to amend the pending proposal, x.[6] In a model 1 experiment, the mover offers the proposal that maximizes her preference with respect to $W(\phi)$ or $W(x)$. Specifically, y is a win-set maximizing proposal if it is preferred to a pending proposal x by mover i, yP_ix, and by a majority of the committee, yPx. Furthermore, in the spatial model, win-set maximizing means that mover i's proposal is located at the point in $W(\phi)$ or $W(x)$ closest to x^i, her ideal point.[7]

The model 1 simulation proceeds from ϕ when a mover submits a win-set maximizing proposal, $x \in W(\phi)$, and it is adopted. A second randomly selected mover then introduces a win-set maximizing proposal, y, to amend the pending proposal, x. Proposal $y \in W(x)$ is adopted; x is replaced by y. Then another member is recognized to submit a further proposal to amend pending proposal y, and so forth. Determination of a win-set maximizing proposal completes one stage of the simulation. A simulation experiment consists of the number of stages required to determine 100 win-set maximizing proposals. A model 1 experiment generates a majority trajectory; each proposal is preferred by

a majority of the committee to the proposal it replaces but not necessarily to any other proposal. The trajectory, a distribution of 100 majority-adopted proposals in X, is taken as a nondeterministic outcome of committee choice under simple win-set maximizing.

Given a forward-moving agenda, win-set maximizing is a unique, short-term, utility maximizing, proposal location strategy. That is, absent further information about subsequent recognition of movers, their preferences, and proposal location strategies, it is assumed that the best a mover can do is maximize his preference with respect to the pending proposal. Proposal distributions from model 1, simple win-set maximizing, provide a benchmark for comparison with distributions of proposals adopted under restricted proposal location and voting strategies.

Restricted Proposal Location

Compared to win-set maximizing, the set of possible restrictions on proposal location is virtually limitless. Just about any consistent procedure for identifying proposals in the choice space qualifies. The object is not to explore the effects of deliberately perverse strategies, however. Instead, I begin by specifying a random proposal location strategy and then develop a number of alternative strategies that are consistent with goal-directed behavior but that fall short of win-set maximizing. In an attempt to proceed systematically, several restricted location strategies are described in order of increasing information requirements on the part of the mover.

Model 2 provides for random location of proposals in the choice space to amend the status quo or the pending proposal. This implies that a mover is recognized who has no interest in the substance of the proposal and, likewise, whether it wins or loses. This member could be taking advantage of the legislative process simply to advertise her presence, perhaps to develop name recognition. If x is pending and a randomly located proposal, y, is in the win set of the pending proposal, $y \in W(x)$, y is adopted, and another mover is selected to introduce the next randomly located proposal to amend y. If $y \notin W(x)$, y is not adopted, and another mover is selected to introduce the next randomly located proposal to amend x.

Model 2 is similar to model 1 except that random proposal location replaces win-set maximizing. Determination of a randomly selected proposal that is or is not preferred to the pending proposal completes one stage of the simulation. An experiment is composed of the number of stages required to determine 100 majority-adopted proposals. Model 2 experiments also generate majority trajectories; each proposal is preferred by a majority of the committee to the proposal it replaces but not necessarily to any other proposal. The trajectory, a distribution of 100 majority-adopted proposals in X, is a nondeterministic outcome of committee choice under random proposal location.

Model 3, the ideal point proposal location strategy, provides for an elementary expression of individual preference. Each randomly selected mover offers a

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[6] The mover is not to be confused with the dictatorial agenda setter who appears in a number of spatial models. Unlike the setter, the mover is a member of the committee; he submits only one proposal and, on so doing, loses the floor. Furthermore, it is not in order for the same mover to be recognized twice in succession.

[7] For simulation purposes, win-set maximizing is achieved by locating proposals on the boundary of the win set. This is simply more tractable than locating them an arbitrarily small distance inside the boundary, and results from model 6 show that it does not affect the conclusions.

proposal located exactly at her ideal point. With x pending and a proposal y located at the mover's ideal point, if $y \in W(x)$, it is adopted, and another mover is selected to submit a proposal to amend y. If $y \notin W(x)$, y is not adopted, and another mover is selected to submit a proposal to amend x, and so forth, until a member is recognized whose ideal point is in $W(x)$ or it is found that there is no ideal point in $W(x)$.

Again, a trajectory of 100 majority-adopted proposals completes a model 3 experiment. It also is possible, however, that the trajectory cannot be completed because a stable outcome is reached. That occurs if x is the pending proposal and no committee member's ideal point is located in $W(x)$. This does not imply there is no point in X preferred by a majority of the committee to x, only that there is no committee member with an ideal point preferred to x. If a stable outcome is reached, that point is taken as the committee choice in a model 3 experiment. Thus, under the ideal point strategy, there are two possible experimental outcomes: a nondeterministic distribution of majority-adopted proposals in X or a unique, stable outcome.

Another version of restricted proposal location follows from the assumption that each randomly selected mover has a limited perception of the choice space. Specifically, the mover views the space as a single dimension defined by the location of the pending proposal, x, and his ideal point, x^i. That single dimension is called the proposal line, and every point on the proposal line between x and x^i is preferred by member i to the pending proposal. Two proposal location strategies are defined in respect to the proposal line.

Model 4 is the proposal-line median strategy. With x the pending proposal, the mover considers the ideal points of the committee members and locates proposal $y \in W(x)$ on the proposal line at the projection of the ideal point of the median member. This strategy requires information about the location of committee members' ideal points but not their indifference curves. Once $y \in W(x)$ is located at the proposal-line median, if y is closer to x^i than x, it is adopted, and another mover is randomly selected to offer a proposal to amend y. If y is farther from x^i than x, it is dropped, and another mover is recognized to amend x. This procedure continues until a trajectory of 100 majority-adopted proposals has been generated, at which point the experiment ends.

The model 4 strategy reflects Black's median voter solution to simple-majority rule over a single dimension. The median proposal defeats any other alternative on the proposal line and yields win sets of minimal size. The proposal-line median emphasizes winning over consideration of proposal content. The value of the strategy lies in limiting the opportunity for extreme responses, that is, proposals located a substantial distance from the center of the preference space.

Model 5, the proposal-line maximizing strategy, also is based on the perception of the choice space as a single dimension. In this case it is assumed that the mover maximizes her preference on the proposal line. This strategy requires information on members' indifference curves as well as their ideal points. The mover

locates the proposal $y \in W(x)$ at the point on the proposal line that is closest to her ideal point. If this maximal point is closer to x^i than pending proposal x, proposal y is adopted. Another mover is then recognized to submit the maximal proposal on his proposal line to amend y, and so forth. If the maximal point is farther from x^i than x, the proposal is dropped, and another mover is recognized to submit a proposal to amend x. When 100 proposals have been adopted, the experiment is complete. Both models 4 and 5, the proposal-line strategies, yield majority trajectories; there is virtually no chance of reaching a stable outcome.

Proposal-line maximizing is not the same as win-set maximizing. Generally, it is possible to find proposals that are closer to a mover's ideal point than the proposal-line maximum and that are preferred by a majority of the committee to the pending proposal. For the mover there is not much to recommend proposal-line maximizing beyond reduced information and computation requirements. Experimental data on outcome convergence under proposal-line maximizing are useful for comparison with results from the proposal-line median strategy and simple win-set maximizing.

For each of the restricted proposal location strategies, mover preferences have a different influence on location. Figure 2, based on the same seven-member committee as in Figure 1, illustrates the alternative proposal locations for models 1–5. In models 3–5 movers increasingly express their individual preferences as compared to random proposal location, but in no case do they maximize their preferences as in simple win-set maximizing. The results from simulated committee choice under these conditions provide quantitative estimates of majority-choice convergence with increasingly informed decisions on proposal location.

Restricted Voting

Under the conventional spatial model, it is assumed that each member votes for any proposal closer to his ideal point than the pending proposal. This assumption does not admit the possibility of thresholds on members' perceptions or preferences. The next phase of the study examines the effects of relaxing this assumption, that is, the effects on committee choice of voting restrictions.

Support thresholds may influence both individual and collective choices by committees. An individual preference threshold requires the mover to submit a proposal that is at least a measurable distance, ε ($\varepsilon > 0.0$), closer to her ideal point than the pending proposal. Furthermore, such a proposal attracts a member's vote only if it is ε closer to her ideal point than the pending proposal. Epsilon is the individual-preference threshold, and model 6 is called ε-maximizing committee choice. If ε is large, members vote only for proposals that are substantial "improvements" on the status quo or the pending proposal. Alternatively, a small value of ε indicates members are willing to support changes that they consider minor alterations of the pending proposal. As $\varepsilon \to 0.0$, committee choice ap-

FIGURE 2. Alternative Proposal Locations

Note: Seven committee members are located by ideal points x^i, i = 1, ..., 7. The pending proposal is y, the win set of y is $W(y)$, and the mover is x^2. Proposal locations z_j, j = 1, ..., 5 refer to model 1 (win-set maximizing); model 2 (random location with z_2 located at the center of the preference space); model 3 (ideal point); model 4 (proposal-line median); and model 5 (proposal-line maximizing). All proposal alternatives except z_3 would be adopted by a majority of the committee.

proaches the conditions for win-set maximizing and the conventional spatial model.

Collectively, a committee may operate under a preference threshold where it is not in order to adopt amendments that alter the pending proposal by an amount less than δ ($\delta > 0.0$). Delta is the committee-preference threshold, and model 7 is called δ-maximizing committee choice. Large values of δ rule out incremental changes, whereas small values of δ result in committee choice that approximates the conventional spatial model.

Experiments under models 6 and 7 are similar to win-set maximizing as described above. A set of ideal points and the status quo, ϕ, are selected from a uniform distribution over a 10 × 10 frame in $X \subseteq R^2$. Then a mover is selected randomly from the committee to submit a preference-maximizing proposal, $x \in W(\phi)$. For model 6, instead of indifference curves, circular ε-preference curves are centered on x^i and intersect a point closer to x^i than ϕ by an amount ε. In this case member i votes for every point inside the ε-preferred set as opposed to ϕ. The ε-win set of ϕ, $W_\varepsilon(\phi)$, is formed by the intersection of the ε-preferred sets of a majority of the committee members. Once an ε-preference maximizing proposal is located, $x \in W_\varepsilon(\phi)$, it is adopted, and a new mover is randomly selected from the committee to submit an ε-preference maximizing proposal, $y \in W_\varepsilon(x)$. Figure 1C illustrates the ε-win set for the seven-member committee shown in Figure 1A. For the same set of ideal points, pending proposal, and simple majority rule, observe that $W_\varepsilon(x)$ is smaller than $W(x)$. This effect is more pronounced for larger values

of ε, and if it happens that $W_\varepsilon(x) = \varnothing$, a stable outcome has been reached. Where ε is small and $\forall x$, $W_\varepsilon(x) \neq \varnothing$, the simulation experiment generates a trajectory of 100 majority-adopted proposals.

Under model 7, δ-maximizing committee choice, a mover is selected randomly from the committee and submits a win-set maximizing proposal, $x \in W_\delta(\phi)$. That proposal is a distance δ or more from ϕ, the proposal is adopted, and another mover is selected to submit a proposal, $y \in W_\delta(x)$. That proposal is farther than δ from x, it is adopted, and so forth. Under model 7 large values of δ may yield a proposal x, such that $W_\delta(x) = \varnothing$; no mover can locate a win-set maximizing proposal farther than δ from x. In these circumstances, proposal x is a stable outcome; the experiment comes to a halt. Model 8 jointly includes ε and δ-maximizing restrictions on committee choice.

THE MEASUREMENT OF COMMITTEE CHOICE CONVERGENCE

The object of this study is to estimate the effects on outcome convergence of restrictions imposed on preference-maximizing committee members. From a set of committee members' ideal points in a preference space, the computer simulation generates a set of proposals adopted by majorities of the committee in a choice space. Quantitative estimates of the dispersion of ideal points and majority-adopted proposals are given by the two-dimensional variance of ideal points and proposals. For m observations (proposals or ideal points), the variance is computed as follows:

$$V = \sum_{j=1}^{m} \frac{((X_j - \bar{X})^2 + (Y_j - \bar{Y})^2)}{m},$$

where (X_j, Y_j) are the coordinates in the two-dimensional space of individual observations, j, and $(\bar{X}, \bar{Y})$ are means of the distributions of observations.

Convergence refers to the proclivity of outcomes of majority decisions to cluster around the center of the preference space. The extent of convergence is indicated by the reduction of ideal-point variance as a result of committee choice.[8] Thus, convergence is measured by comparing the variance of a set of majority-adopted proposals, V_p, to the variance of the ideal points of the committee members who adopted the proposals, v.

A second measure of convergence used in this study is r^*, the radius of the equivalent variance circle. For any distribution of ideal points or proposals with a known variance, V, the equivalent variance is generated by an even number of points located at equal

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[8] The simulation of majority choice by committees takes place over a 10 × 10 frame in two-dimensional Euclidean space. Elsewhere (Koehler 1996) it has been demonstrated that the expected variance of a uniform distribution over a square frame in X is $V = s^2(n - 1)/6n$, where s^2 is the area of the frame, and n is the number of observations in the distribution. Thus, for 100 ideal points selected randomly from a uniform distribution over a 10 × 10 frame in X, the expected variance is $v = 16.500$. For a continuous distribution over the same frame, $v = 16.667$.

FIGURE 3.   Committee Ideal Points and Majority-Adopted Proposals

A: Ideal Points for a 61-member Committee
    $\overline{X}$ = 5.404, $\overline{Y}$ = 3.937, v = 18.754, r^* = 4.331

B. Win-Set Maximizing Proposals (Model 1)
    for the 61-member Committee
    $\overline{X}$ = 5.377, $\overline{Y}$ = 3.649, V = 6.091, r^* = 2.468

C. Proposal-Line Maximizing Proposals (Model 5)
    for the 61-member Committee
    $\overline{X}$ = 5.296, $\overline{Y}$ = 3.681, V = 2.036, r^* = 1.427

D. Random Proposal Location (Model 2)
    for the 61-member Committee
    $\overline{X}$ = 5.350, $\overline{Y}$ = 3.563, V = 1.039, r^* = 1.020
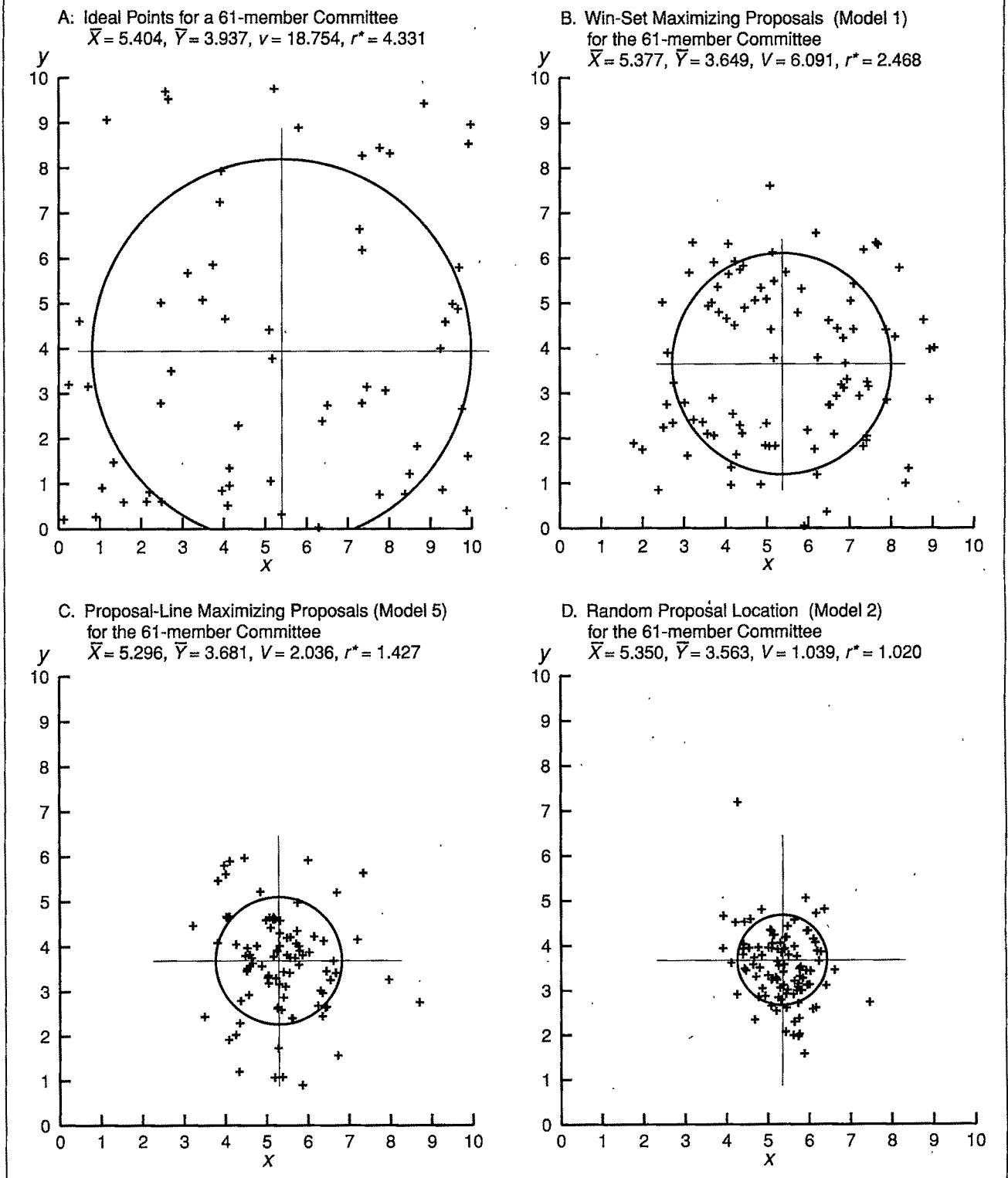
intervals on a circle centered on the two-dimensional mean of the distribution with a radius $r^* = \sqrt{V}$. The two-dimensional variance is an indicator of the area occupied by a distribution of observations, whereas r^* gives the average distance of observations from the center of the preference space.

Figure 3 illustrates these statistics applied to distributions of ideal points and proposals. The first panel shows the distribution of a set of ideal points, randomly selected from a uniform distribution, and the equivalent variance circle for the 61-member committee. This particular committee was chosen because it generates

TABLE 1. Simulation Results

Model	Convergence Measures	Predictor
1: Win-set Maximizing	$\bar{v} = 16.351$	
	$\bar{V}_1 = 6.053\ (63.0\%)$	$v^{***},\ r^{***}$
2: Random Location	$\bar{v} = 16.351$	
	$\bar{V}_2 = 0.842\ (94.9\%)$	r^{***}
3: Ideal-Point Max	$\bar{v}_u = 16.562\ (N = 37)$	
	$\bar{V}_3 = 1.774\ (89.3\%)$	r^{***}
	$\bar{v}_s = 16.304\ (N = 164)$	
	$\bar{D}_3 = 0.713\ (82.3\%)$	$v^*,\ r^{**},\ (-)n^{***}$
4: Proposal-Line Md	$\bar{v} = 16.351$	
	$\bar{V}_4 = 0.186\ (98.9\%)$	r^{***}
5: Proposal-Line Max	$\bar{v} = 16.351$	
	$\bar{V}_5 = 1.962\ (88.0\%)$	$v^{**},\ r^{***},\ (-)i^{***}$
6: ε-Maximizing	$\bar{v}_u = 16.407\ (N = 156)$	
	$\bar{V}_6 = 5.396\ (67.1\%)$	$v^{***},\ r^{***},\ (-)\varepsilon^{***}$
	$\bar{v}_s = 16.159\ (N = 45)$	
	$\bar{D}_6 = 0.399\ (90.1\%)$	ε^{**}
7: δ-Maximizing	$\bar{v}_u = 16.516\ (N = 147)$	
	$\bar{V}_7 = 6.599\ (60.0\%)$	$v^{***},\ r^{***},\ \delta^{**}$
	$\bar{v}_s = 15.903\ (N = 54)$	
	$\bar{D}_7 = 0.576\ (85.6\%)$	$v^*,\ \delta^{***}$
8: $\delta,\ \varepsilon$-Maximizing	$\bar{v}_u = 16.519\ (N = 120)$	
	$\bar{V}_8 = 5.823\ (64.7\%)$	$v^{***},\ r^{***},\ \delta^{**},\ (-)\varepsilon^{***}$
	$\bar{v}_s = 16.104\ (N = 81)$	
	$\bar{D}_8 = 0.577\ (85.6\%)$	$\delta^{**},\ \varepsilon^{***}$

Note: Convergence measures are $\bar{V}_p$, the average proposal variance, and $\bar{D}_p$, the average distance of stable outcomes from the means of their preference distributions. Predictors of convergence are v, the ideal-point variance; r, the yolk radius; n, the committee size; i, the number of stages; ε, the individual preference threshold; and δ, the collective preference threshold. N is the number of unstable or stable experiments. Significance levels are indicated by $^*p < .05,\ ^{**}p < .01,\ ^{***}p < .001$.

proposal variances that are typical of the experimental results. Furthermore, the vertical mean of the ideal-point distribution is somewhat off center, $\bar{Y} = 3.937$, as opposed to the expectation, $\bar{Y} = 5.0$. Note that each proposal distribution reproduces this skewed preference pattern. Figure 3B shows a set of 100 proposals adopted by the 61 committee members under win-set maximizing proposal location (model 1). The reduction in the radius of the equivalent variance circle compared to the ideal points indicates the extent of outcome convergence attributable to majority rule with short-term, utility maximizing proposal location. Examples of preference-restricted proposal location are shown in the last two panels. Figure 3C is the result of proposal-line maximizing (model 5), and Figure 3D is the result of random proposal location (model 2). Convergence is indicated by reductions in the radii of the equivalent variance circles, in this case compared to the distribution of win-set maximizing proposals as well as the ideal points.

RESULTS FROM THE SIMULATION OF RESTRICTED PREFERENCE MAXIMIZING

For each of the models described above, 201 experiments were run, each generating 100 majority-adopted proposals or, in some cases, stable outcomes. The large number of experiments provided data for a statistical analysis of outcome convergence with respect to a number of factors in committee choice beyond the restrictions on proposal location and voting.

Results from the simulations are presented in two tables. Table 1 shows the effect on convergence of restricted proposal location and voting. The first statistic given for model 1 is the average ideal-point variance, $\bar{v} = 16.351$ for the 201 experiments. Ideal-point variances are reported separately for models in which some experiments produced stable outcomes. Thus, under model 3, 37 experiments produced unstable outcomes, and the ideal-point variance for those committees was $\bar{v}_u = 16.562$. The remaining

164 committees reached stable outcomes, and $\bar{v}_s$ = 16.304.

The principal indicators of convergence are $\bar{V}_p$, p = 1, ..., 8, the proposal variance, and for models 3, 6, 7, and 8, when stable outcomes are reached, $\bar{D}_p$, p = 3, 6, 7, 8, is the average distance from the two-dimensional means of committee members' ideal points to the stable outcomes. In Table 1, the numbers in parentheses following V_p give the percentage reduction in the ideal-point variance. For model 1, $\bar{V}_1$ = 6.053 and indicates a 63% reduction in $\bar{v}$=16.351, the ideal-point variance. The numbers in parentheses following $\bar{D}_p$ give the percent reduction in r^*, the radius of the equivalent variance circle for the ideal points. For model 3, $\bar{D}_3$ = 0.713 represents a reduction of 82.3% in r^* = $\sqrt{16.304}$ = 4.038.

The third column of Table 1 gives results from an ordinary least squares (OLS) regression of the convergence indicators, V_p and D_p, on a number of factors in the experimental procedure that encourage or discourage convergence: v, the ideal-point variance; r, the radius of the yolk; n, the committee size; i, the number of stages required to complete an experiment; ε, the individual-preference threshold; and δ, the committee-preference threshold. The asterisks indicate 0.05, 0.01, and 0.001 levels of significance.

Model 1 shows that both indicators of committee members' preferences, v and r, are statistically significant. Elsewhere (Koehler n.d.) it has been argued that the ideal-point variance, v, is a measure of individual preference, expressed by committee members' proposal location decisions. Alternatively, the yolk radius, r, based on the location of median lines, indicates the majority or collective preference of committee members. The OLS results show that individual preference, v, and collective preference, r, have separate, independent effects on win-set maximizing proposal variance.[9] Following model 1, subsequent experiments were run using the same 201 committees (ideal-point sets) in order to facilitate comparison. It follows that differences in convergence are strictly attributable to the behavioral restrictions on committee members as opposed to differences in committee size and member preferences.

The experiments featuring restrictions on proposal location, models 2–5, all demonstrate greater convergence than win-set maximizing. Furthermore, the reduction in proposal variance reflects the information content of different proposal location strategies. Proceeding from win-set maximizing under model 1, proposal-line maximizing, ideal-point maximizing, and random proposal location follow in order of decreasing proposal variance and increasing convergence. The exception is model 4. The proposal-line median is a short-run, defensive strategy whereby movers use information about members' ideal points to minimize the size of win sets of successful proposals. This limits the distance from the center of the preference space where

the next mover can locate a winning proposal. The result is even more intense convergence than is found under random proposal location. Under model 4, information is used by movers to reinforce convergence.

Both individual and collective preferences, v and r, respectively, are factors in the convergence of committee choice. Predictably, r is significant in every case and is positively correlated with proposal variance; a smaller yolk yields greater convergence. Table 1 also shows that V_p is related to v when the movers' preferences influence proposal location decisions. When v is significant, it is positively correlated with proposal variance; a smaller ideal-point variance yields greater convergence. Exceptions are models 2–4: random proposal location, ideal-point maximizing, and the proposal-line median. In these location strategies, a mover's options are quite limited; individual preferences are not reflected in proposal locations. This may seem counterintuitive for ideal-point maximizing, but once a proposal is adopted that is close to the center of the preference space, its relatively small win set implies that most members' ideal points that are more distant from the center are, effectively, no longer in play.

The only other significant factors are the committee size, n, for the stable outcomes under model 3 and the number of stages, i, under model 5. The former indicates that large committees are more likely to generate stable outcomes near the center of the preference space. The number of stages under proposal-line maximizing is highly significant. Thus, for model 5 experiments, when convergence is intense (and win sets are accordingly small), large numbers of stages are required to identify 100 proposals preferred by movers to the pending proposals.

Models 6–8 give the results from restricted voting. Values of ε and δ, the individual and collective preference thresholds, respectively, were chosen randomly from ranges of values that were found to produce both unstable and stable outcomes ($0.0 \le \varepsilon \le 0.15$; $0.0 \le \delta \le 2.5$). Consider the unstable outcomes first. Using model 1 as the baseline, the unstable outcomes from model 6 show that the individual preference threshold reduces proposal variance by 10.9% ($\bar{V}_1$ = 6.053; $\bar{V}_6$ = 5.396, p < 0.001); the committee preference threshold increases proposal variance by 9.0% ($\bar{V}_7$ = 6.599, p < 0.01); and together they reduce proposal variance by 3.8% ($\bar{V}_8$ = 5.823, p < 0.250). When the two thresholds are run together, the difference between $\bar{V}_1$ and $\bar{V}_8$ is not significant. Compared to simple win-set maximizing, it is evident that preference thresholds have little effect on the convergence of unstable outcomes. Considering the OLS results, v and r are inversely related to convergence, as was true of the experiments reported in model 1. Furthermore, ε is inversely correlated with proposal variance; convergence is greater for larger values of ε. The opposite is true for δ; V_p increases with increases in δ.

When stable outcomes are reached, $\bar{D}_p$, the average distance from the two-dimensional means of the preference distributions to such outcomes, is very small. This is particularly true for ε maximizing; $\bar{D}_6$ = 0.399

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[9] These variables are significantly correlated over the 201 experiments, r_{vr} = 0.315 (p < 0.0001), but less than 10% of the variation is explained.

TABLE 2. Committee Characteristics with Unstable and Stable Committee Choice				
Committee Characteristics	Model 3	Model 6	Model 7	Model 8
Unstable $\bar{v}_u$	16.562	16.407	16.516	16.519
Stable $\bar{v}_s$	16.304	16.159	15.903	16.104
t			2.74**	2.01*
Unstable $\bar{r}$	0.590	0.547	0.554	0.582
Stable $\bar{r}$	0.498	0.405	0.409	0.417
t	2.72**	7.27***	7.48***	8.65***
Unstable $\bar{n}$	57.919	58.654	57.585	53.933
Stable $\bar{n}$	62.683	72.733	73.296	73.469
t		3.84***	4.80***	6.51***
Unstable $\bar{\varepsilon}$		0.0597		0.0644
Stable $\bar{\varepsilon}$		0.113		0.0825
t		10.52***		2.86**
Unstable $\bar{\delta}$			0.918	0.930
Stable $\bar{\delta}$			1.998	1.620
t			14.82***	7.12***

Note: Committee characteristics are v, the ideal point variance; r, the yolk radius; n, the committee size; ε, the individual preference threshold; and δ the collective preference threshold. For all 201 experiments, the means of the characteristics are $\bar{v}$ = 16.351, $\bar{r}$ = 0.515, $\bar{n}$ = 61.806, $\bar{\varepsilon}$ = 0.0716, and $\bar{\delta}$ = 1.208. Significance levels for the difference of means tests are indicated by *$p < .05$, **$p < .01$, ***$p < .001$.

is less than the average radius of the yolk for the same subset of committees, as shown in Table 2. For stable outcomes the average size of the individual threshold is $\bar{\varepsilon} = 0.113$, and the collective threshold is $\bar{\delta} = 1.998$. To see how small ε is, consider that the expected distance between adjacent ideal points, $\bar{n} = 61.806$ (Table 2), distributed uniformly over a 10 × 10 frame, is 1.272, or 11.26 times greater than $\bar{\varepsilon}$. Alternatively, the collective preference threshold, $\bar{\delta} = 1.998$ in the 10 × 10 frame, is too large to be a plausible constraint on committee choice.

These results indicate that very low individual threshold requirements can generate stable outcomes. It is important to note that this holds for committees of relatively modest size. The coefficients for ε and δ are significant and inversely related to convergence when they are included in the experiments. For ε this is a reversal from the unstable cases, in which it is directly related to convergence. An important finding is the absence of influence on D_p of voter preferences in models 6–8. The radius of the yolk, an indicator of collective or committee preference, does not appear in any of the OLS estimates. The indicator of individual preferences, v, is barely significant in one case and absent in the two other.

Table 2 summarizes the differences between committees that yield unstable as opposed to stable committee choices. Compared to unstable outcomes, committees that reach stable outcomes have significantly smaller ideal-point variances (models 7 and 8), have smaller yolk radii (models 3, 6, 7, and 8), and are larger

in size (models 6, 7, and 8). With respect to voting restrictions, both ε (models 6 and 8) and δ (models 7 and 8) are significantly larger when stable outcomes are reached.

CONCLUSION

The point of departure for this study is the size of the small, centrally located subset of preference space that is held to contain the outcomes of majority choice by committees. An estimate of that size is provided by the proposal distribution under simple win-set maximizing. The proposal variance, $\bar{V}_1 = 6.053$, indicates a 63% reduction in the ideal-point variance, $\bar{v} = 16.351$. That not only demonstrates convergence but also shows that outcomes are distributed over roughly 37% of the 10 × 10 subset of the preference space. Model 1 also indicates that the size of the subsets of outcomes depends on the radius of the yolk and the variance of committee members' ideal points. From there the effects on convergence of restricted committee choice are positive; compared to win-set maximizing, restrictions on proposal location and voting thresholds generally enhance the convergence of majority-adopted outcomes. They work in markedly different ways, however.

Restricted Proposal Location

Preference restrictions on proposal location can be interpreted as limits on agenda control. They induce convergence by limiting the options available to the mover. Thus, the results presented here can be placed in a context that proceeds from the global-cycling theorems. Those theorems prove that agendas can be formed which lead a committee through a series of majority votes to any point in a choice space. It is assumed that there is a dictatorial agenda setter who is not a member of the committee and who can locate proposals wherever she sees fit. It follows that such a procedure imposes virtually no limit on proposal variance. Specifically, it can exceed the committee members' ideal-point variance.

The first plausible restriction on agenda control is to require the agenda setter to be a member of the committee. She still exerts complete control over the agenda, but committee membership implies that committee choice will be limited to the Pareto set of ideal points. Then, if dictatorial agenda setters are selected randomly from a committee, proposals will be located at members' ideal points, and over time the proposal variance will be equal to the ideal-point variance. That is estimated to be $v = 16.351$ over a 10 × 10 frame for the committee sizes considered here. A second plausible restriction is to eliminate the agenda setter's dictatorial power; no member is allowed to submit more than one proposal before he is required to relinquish the floor. In effect the agenda setter is replaced by the member referred to here as a mover. This procedure is realized by model 1, win-set maximizing, and the proposal variance is estimated to be $\bar{V}_1 = 6.053$.

Further constraints on agenda control go directly to restrictions on the ability of the mover to express his

preferences. If preference maximizing is limited to the proposal line, proposal variance is reduced to $V_5 = 1.962$, and limiting proposals to the mover's ideal point yields $\bar{V}_6 = 1.774$. The two remaining models virtually eliminate agenda control and generate very small proposal variances. These results indicate that the principal impediment to convergence is preference-maximizing agenda formation; if it is subject to increasing restrictions, convergence improves accordingly.

The factors that influence convergence shed further light on the process. Considering unstable outcomes, Table 1 shows that proposal variance, V_p, is always related to r, the indicator of a committee's collective (majority) preference. V_p also is related to v, the indicator of individual preferences, when the mover is able to exercise some degree of preference maximizing. Overall, these results show that when committee choice yields a majority trajectory of indefinite length it is primarily the extent of agenda control, along with individual and collective voter preferences, that influences proposal variance.

Restricted Voting

Models 6–8 demonstrate a different approach to convergence. Committee members vote for proposals only if these represent a finite improvement on the pending proposal. From the statistics in Table 1, it is evident that voting thresholds have little effect on the variance of unstable outcomes. The effects of preference maximizing are not systematically constrained by individual and collective thresholds on voting. The threshold conditions affect convergence only when they are of sufficient size to induce a stable committee choice.

Perhaps the most important characteristic of stable outcomes, regardless of whether they result from individual or collective voting thresholds, is that they are not scattered widely in the choice space. To the contrary, there is dramatic evidence of convergence compared to both ideal-point variance and win-set maximizing proposal variance. This convergence follows from the characteristics of win sets; specifically, proposals near the center of the preference space have smaller win sets than those located near the boundary of the Pareto set. For example, compare Figure 1B to Figure 1A: For the same set of ideal points, proposal y is closer than proposal x to the center of the preference space. The win set for y clearly is smaller than the win set for x. Next, consider a committee proceeding under an individual preference threshold, $\varepsilon > 0.0$. That threshold also implies that win sets generally are reduced in size, as in Figure 1C compared to Figure 1A. The combined effects of proposal movement from x to y and $\varepsilon > 0.0$ appears in Figure 1D. From there it is evident that either an increase in ε or further movement of y toward the center of the preference space could yield a proposal that has an empty win set. Thus, if an individual preference threshold is of sufficient size, and the trajectory proceeds to a point close to the center of the space, the win set disappears, and there is no escape. The disappearance of the win set is the cause of stable outcomes under individual and

collective preference thresholds and explains why they are found in very close proximity to the center of the preference space.

Another issue concerns the ineffectiveness of δ, the collective preference threshold, as opposed to ε, the individual threshold. Figures 1B and 1D show that an increase in ε reduces the size of the win set in all directions because the threshold applies to all members. Alternatively, δ reduces the size of a win set by eliminating part of the set in the immediate vicinity of the pending proposal, but the extremities of the set are unchanged. Thus, for δ to produce an empty win set, it must be large enough to enclose the farthest extremities of the set. That is accomplished most easily when a pending proposal is near the center of the preference space, and the result is the very small values of $\bar{D}_p$ noted above. In order to contain the extremities of even the smallest win sets, however, δ must be impractically large. The different effects of ε and δ on win sets for unstable proposals also explain the decrease in V_p with increases in ε and the opposite effect with increases in δ.

These findings are important. If individual or committee choice thresholds are raised to high levels, it is obvious that a majority trajectory can be brought to a halt. What is not so obvious is that the resulting outcomes generally demonstrate remarkable convergence, and stability can be achieved at relatively low *individual* preference thresholds.

The distance of stable outcomes from the central tendency of member preferences is primarily related to the size of ε and δ, as opposed to member preferences. In other words, if committee choice is limited to a very small subset of the choice space, it is precisely because the expression of individual and collective voter preferences has been suppressed. In that event, the preference restrictions are better predictors of D_p than are committee members' preferences per se.

An additional issue concerns what has been learned from this study beyond the results reported by Tovey (1991) in his consideration of the tradeoffs between noncenteredness of ideal-point distributions (yolk radius $r > 0.0$) and voting thresholds. He proved that equilibria will exist for $\varepsilon > r$ or $\delta > 2r$ as n approaches infinity. The simulation illustrates the tradeoffs, generating stable outcomes when, on average, $\bar{\varepsilon} = 0.279r$ and $\bar{n} = 72.733$, or $\bar{\delta} = 4.885r$ and $\bar{n} = 73.296$. These results are consistent with Tovey's theorems. They also show that stable outcomes can occur for committees of considerably less than infinite size, and ε is a much more effective constraint on instability than δ. Furthermore, Tovey states that "the yolk center is an undominated point" (p. 20), but the simulation shows that stable outcomes are not limited to a single point, although they are found near the center of the preference space.

Two further questions may be susceptible to simulation modeling in the future. First, under the conventional spatial model, including a forward-moving agenda, where should members locate proposals to maximize utility beyond the short run? Second, what is

the effect on convergence of preferences correlated over the two spatial dimensions.

Critics of simulation have stated that results in the form of proposal distributions are not particularly satisfying or informative. There is no argument that a positive analytic model capable of generating specific predictions of unique outcomes would be preferable to predictions based on sets of potential outcomes. Yet, in defense of the simulation results, for majority choice, which generally does not yield unique outcomes, the principal remaining question regards convergence. To what extent do outcomes of committee choice converge on the center of the preference space, and what factors influence the extent of convergence? Simulation analysis indicates convergence in all cases; enhanced convergence follows from restricted proposal location; and stability with maximal convergence is a possibility, given thresholds on voting. The latter conclusion is reinforced when preference thresholds and committee size are relatively large and when ideal-point variance and the yolk are relatively small. If outcomes converge, small deviations from the center of the preference space only serve to remind us that there is an irreducible minimum in majority-preference specificity. That minimum, indicated by the radius of the yolk, precludes more precise outcome predictions when committee members have some latitude to pursue their individual preferences with respect to proposal location. If the object of a collective-choice process is to replace a status quo near the boundary of the Pareto set with a committee choice near the center of the preference space, that is, an improved representation of member preferences, then the simulation reveals conditions under which simple-majority rule has a good chance of producing that outcome.

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Recycling the Garbage Can: An Assessment of the Research Program

JONATHAN BENDOR *Stanford University*
TERRY M. MOE *Stanford University*
KENNETH W. SHOTTS *Northwestern University*

The garbage can theory of organizational choice is one of the best-known innovations in modern organization theory. It also has significantly shaped a major branch of the new institutionalism. Yet, the theory has not received the systematic assessment that it both deserves and needs. We evaluate the early verbal theory and argue that it fails to create an adequate foundation for scientific progress. We then analyze and rerun Cohen, March, and Olsen's computer model and discover that its agents move in lockstep patterns that are strikingly different from the spirit of the theory. Indeed, the simulation and the theory are incompatible. Next, we examine how the authors have built upon these incompatible formulations in developing the theory further. We assess this larger program, which includes the March-Olsen version of the new institutionalism, and find that many of the problems that attended the original article have intensified over time. We conclude that a fundamental overhaul is required if the theory is to realize its early promise.

The garbage can theory of organizations (Cohen, March, and Olsen 1972) has led a charmed life—to its great disadvantage. Although proponents have effectively advanced their ideas, potential critics have done little to bring about a productive intellectual exchange. This one-sidedness impedes scientific progress. And it surely has hurt the garbage can theory, which, despite its prominence, and despite the optimism and enthusiasm that mark its history, has never been able to overcome its inherent weaknesses and make the kind of contribution that, nearly thirty years ago, seemed its destiny.

This is unfortunate. Cohen, March, and Olsen's first paper is brimming with provocative insights that offer a promising basis for theory, but thus far their potential has largely gone untapped. Indeed, the theory that has grown up over the years is so complex and confusing, and some of its components are so seriously flawed, that there is little reason for thinking that it can look ahead to a more fertile future. For fundamental reasons, the theory lacks the rigor, discipline, and analytic power needed for genuine progress. We want to explain why this is so and what might be done to get it back on track.

Why expend so much energy examining an "old" article and its progeny? Such an exercise would be

eminently worthwhile if only because some of the ideas are well worth salvaging. But there is more to be gained than that. The 1972 article has had considerable influence on political science and on institutional theory more generally. Some of this influence is directly due to the original article, whose ideas have shaped thinking about organizations ever since, and which presents one of the most famous computer simulations in all of social science. Perhaps most of its influence, however, is now exercised indirectly, through more recent works that have moved into new terrain and gained much attention in their own right but are grounded in the original theory. A few of the latter have been carried out in the work of other authors, notably Kingdon's *Agendas, Alternatives, and Public Policies* (1984), one of the most highly regarded treatments of policymaking in the last two decades. But virtually all the basic theoretical work in this tradition has been done by the original authors, particularly March and Olsen.

Consider the lineage. *Ambiguity and Choice* (March and Olsen 1976a) expanded on the 1972 theory, received considerable attention, and solidified the garbage can's new status as a major school of organization theory. Subsequently, the *American Political Science Review* published March and Olsen's (1984) hugely influential overview article, "The New Institutionalism . . . ," which officially ushered the new institutionalism into political science and offered a perspective on institutional theory that gave a prominent role to the garbage can and its progeny. Henceforth, political scientists would routinely consider the March-Olsen line of theory as a main variant of the new institutionalism (Peters 1996). This article, in turn, developed into March and Olsen's (1989) longer statement on the new institutionalism, *Rediscovering Institutions*, which, as we will show, was strongly influenced by the original garbage can theory and its extensions. This book met with much acclaim and has been judged a "contemporary classic" by Goodin and Klingemann (1996) in their recent survey of the discipline.

This is an impressive hit parade; clearly, the garbage can theory has had an important impact on the field. Yet, remarkably, there have been no systematic at-

tempts to assess the theory or even to clarify its logic and content so that everyone can agree on what it actually says. This is not true for the other two major approaches to institutional analysis—theories of rational choice (e.g., Weingast 1996) and of bounded rationality (Cyert and March 1963; March and Simon 1958; Simon 1947)—that are its main rivals. Scholars are quite clear about what these theories say, and both traditions have been the subject of debate and criticism (e.g., Green and Shapiro 1994). The garbage can theory has escaped all this. Even if it were a shining example of social science theory, this would be unhealthy, for no theory and no field of study can do without criticism, clarity, and debate. The situation is all the more worrisome given the intellectual influence the garbage can has had over the years. If we are right about its deficiencies, this influence cannot help but propagate problems that the discipline should avoid in its pursuit of truly productive theories.

We offer a critical assessment of the garbage can theory that is as comprehensive as possible, befitting a research program that has gone unchallenged for its entire lifetime. We also seek to clarify what the theory actually says, which is absolutely necessary given the pervasive confusion that surrounds it. Finally, we try to map out in a bit more detail the theory's intellectual lineage in order to show how the original work—and its problems—are reflected in the new institutionalism. This is a lot to do in one article; there is not enough space to do more, although we would like to. Our immediate goal is to stimulate an intellectual exchange that will promote a better understanding of the garbage can approach and its role in institutional theory. In the longer term, we hope to be part of a collective effort to reconstruct the theory around some of its more promising (and still exciting) ideas and thus help revitalize the research program and set it on a more productive path.

We cannot delve into the details of this reconstruction here. That remains for future work. But we do think—for reasons we will cover below—that the garbage can's key ideas can best be developed by anchoring them firmly in theories of rational choice or bounded rationality, most likely the latter, whose analytic apparatus for exploring decision making under cognitive constraints is particularly appropriate. Understood in this way, a revised garbage can theory would become a parametric variant within the bounded rationality program. This would clarify the garbage can's theoretical logic, give it a clear microfoundation, and provide it with real deductive power for pursuing its insights—all of which it now lacks.

THE VERBAL THEORY AND THE COMPUTER MODEL

What is the garbage can theory (GCT)? This is a simple question with no simple answer. The original article sets out two distinct formulations. The first is a set of ideas about how decisions get made in "organized anarchies." These ideas, because they are expressed in ordinary English and their logical relations are not rigorously spelled out, constitute a verbal (or informal) theory. This is Cohen, March, and Olsen's (1972) general statement of the garbage can theory as originally conceived. The second formulation is the computer model, which is intended to capture in simplified, mathematical form the key features of the verbal theory and to provide a rigorous logical structure that can generate predictions. We will examine both of these.

The two formulations should be kept separate. The verbal theory is fundamental. The computer model is derivative: It is only one of many possible ways to formalize key features of the theory and draw out its implications. But we must remember that any particular formalization is not unique. We must also remember that the sophistication and rigor of the simulation do not in themselves guarantee that it faithfully reflects the theory; depending on the details of its design, so much may be lost in the translation that the model could turn out to have little value or even prove misleading.

The place to start if we want to understand the GCT, then, is with the verbal theory. The original article, however, sheds surprisingly little light on its content. Only two pages discuss it, and little is done to develop, connect, or clarify its central ideas. Instead, almost all the article is devoted to the computer model, whose properties are discussed in detail, and whose implications are explored at length and then applied to a specific empirical domain (universities).

Most of what we know about the informal theory comes not from the original article but from later work, particularly March and Olsen's *Ambiguity and Choice* (1976a), March and Weissinger-Baylon's *Ambiguity and Command* (1986), and March and Olsen's *Rediscovering Institutions* (1989). (See also March's *A Primer on Decision Making* [1994].) This later literature does more than fill in the basic contours of the original theory, however. It also dramatically extends the scope and complexity of garbage-can-like thinking and ultimately develops a new theory, the March-Olsen variant of the new institutionalism, which makes frequent, although imprecise, use of GCT ideas.

Unfortunately, the authors and their colleagues rarely distinguish between the informal theory and the computer model when specifying what GCT is and what it tells us about organizations. This holds for most of the more recent literature on the GCT and is even true of the original article. There the authors begin with a clear distinction between the two, but by the concluding section they have tossed both into the same idea-pool, characterizing garbage can processes by properties that come sometimes from the verbal theory and sometimes from the computer model—but without any recognition that this is being done. GCT thus emerges as an undifferentiated blend of the two.

We will sort through all this by proceeding as follows. First, we clarify and evaluate the ideas that appear most central to the verbal theory—emphasizing those in the original article but including some introduced in more recent work when these seem appropriate. Second, we turn to the computer model, clarify its

structure and operation, and assess its contribution to the larger enterprise. Third, we examine GCT's post-1972 theoretical development, including its influence on the March-Olsen variant of the new institutionalism, and assess these newer lines of research. Finally, we briefly illustrate how the GCT might be revitalized by using some classical ideas of bounded rationality to analyze one of the garbage can's most provocative insights.

THE EARLY VERBAL THEORY

The verbal theory is about organized anarchies, a class of organizations or decision situations marked by problematic preferences, unclear technology, and fluid participation. As the definition suggests, organized anarchies face certain kinds of ambiguity. This is what makes them interesting. GCT is an effort to explain how organizations make choices and solve problems under conditions of ambiguity so troubling they would appear to render decision making extremely difficult or impossible.

The various branches of rational choice theory have long had sophisticated tools for modeling individual choice under uncertainty. But GCT is founded on a radically different approach to choice. Its premise is that choice in an organized anarchy cannot be understood via the intentions of organizational participants, and imposing a rational explanation on organizational behavior can only distort what is really going on. Choices often just happen, with no clear connection to what participants want. They arise from dynamic organizational processes that are complex, highly contextual, and unpredictable, driven more by accident and timing than individual intention. This gives much of organizational life an almost chaotic appearance.

At the heart of GCT is the notion that organizational outcomes arise from independent "streams" of problems, solutions, participants, and choice opportunities, whose random intersection generates decisions. In this scheme, choice opportunities are the garbage cans. As problems, solutions, and participants move independently about the organization, various combinations find themselves dumped into these cans, and the decisions coming out (if any) depend on whatever mixtures the intersecting streams happen to generate. The organization's basic structure (notably, its rules) channels and constrains this multifarious action, thereby shaping the organization's patterns of choice and problem solving. But the driving force of the garbage can explanation is process.

The process-driven world of the garbage can is more than just dynamic. It is also strange and even pathological by conventional standards, and the authors make much of this. Alice has gone through the looking-glass, and nothing is as it seems. Choices happen for no apparent reason. Outcomes are divorced from intention. Solutions are disconnected from problems. People wander aimlessly in and out of decision arenas. In this welter of loosely coupled activity, some decisions do get made, and some problems do get solved. There is a chaotic brand of "intelligence" at work. But it is

largely due to the time-dependent confluence of events, not to the rational effects of plans or goals.

The organizational literature often calls the GCT a metaphor. Even the authors have used this term (e.g., March and Olsen 1986). In a sense this rings true: The notion of different inputs being mixed together in a garbage can, with output depending on what is mixed with what, is clearly metaphorical. But we must be careful with this kind of language, because calling a set of ideas a metaphor may inappropriately shield it from criticism. A metaphor is, after all, a literary device that need not meet the same standards as social science theories. Yet, the GCT's basic ideas are obviously intended to be a theory and should be treated as such.

Any effort to clarify and appraise the theory could highlight a large number of issues. We think the following are among the most important.

Individual Choice and the Level of Explanation

GCT is a theory of organizational choice. In developing it, the authors naturally move back and forth between individuals and organizations. They discuss how individuals make choices and how these choices depart from standard notions of rationality. They also talk about how, as an organization's dynamics work themselves out, the choices of individuals are aggregated into organizational choices. The impression is that GCT explains organizational choice via a logically coherent, if unorthodox, theory of individual choice.

But this is not really so. Although the authors make clear that they are abandoning conventional rational choice approaches, they do not develop their own model of individual choice that can function as a component of GCT. Indeed, the original article offers no theoretical treatment of individuals at all; and in the computer model individuals are little more than automatons, barely touched by motivations or beliefs. The article fashions a theory of organizational choice based on assumptions about process and structure—but without any underlying theory of individual choice. The explanation is at the macro level.

In more recent work, which moves toward a larger theory, the authors do talk theoretically about individuals and offer arguments about how decisions are made in the face of ambiguity. In these discussions, however, everything inherent to individual choice is left endogenous to the choice process. Among other things, it is not assumed that preferences exist prior to choice or that they determine actors' evaluation of alternatives; rather, people discover their preferences only after they have decided, which begs the question of how decisions are made in the first place.

Empirically, it might seem attractive to relax every assumption of classical rational choice theory and argue that all these aspects depend on experience. Analytically, however, with everything treated as endogenous and nothing taken as given, there is no logical basis for deriving behavioral expectations or for moving from individual to organizational properties. The authors try to fill in the gaps by referring to other

possible foundations of individual behavior—symbols, rules, duty, obligation, myths, and the like—but these complications, which may or may not be empirically warranted, do not add up to a coherent model of individual choice that can generate testable implications. If anything, they hinder a serious effort to construct one.

However one might evaluate these more recent efforts to build a theory of individual choice, the early GCT is essentially a macro theory of organizations. It talks a lot about choice and individuals, but the theory really focuses on process and structure—organization-level phenomena—and does not arise from individualistic foundations.

Independent Streams

The hallmark of GCT is its attempt to understand organizations by reference to streams of problems, solutions, participants, and choice opportunities, which are independent of one another and exogenous to the system. On the surface, this formulation seems to be a simple, straightforward way of characterizing the internal dynamics of organization. It is memorably provocative, widely cited—and crumbles upon closer examination.

First, in the real world problems are typically identified and put on organizational agendas by organizational participants. Similarly, solutions find their way into organizations because they are pushed by certain participants in light of the problems they face. There is nothing mysterious about this. It is obvious. *People are the carriers of problems and solutions.* Indeed, when Cohen, March, and Olsen discuss garbage can processes, they often say exactly that. Their original article (1972) even included this point in its list of "basic ideas": "To understand processes within organizations, one can view a choice opportunity as a garbage can into which various kinds of problems and solutions are dumped *by participants* as they are generated" (p. 2, emphasis added). This sort of reasoning is repeated elsewhere. But if problems and solutions are dumped by participants, then how can streams of problems and solutions be independent of the stream of participants? Clearly, which problems and which solutions get dumped into a particular garbage can depend on which participants happen to go there. They are not independent streams.[1]

There is a second important source of confusion that surrounds the notion of independent streams. This one arises from an odd use of language. Take, for instance, the concept of "solution." *Webster's Collegiate Dictio-*

nary defines this term as "an action or process of solving a problem" (p. 1123), which is surely the way it is commonly used and understood. By definition, then, solutions do not exist on their own, independent of specific problems. Nor does it make sense to call an object (like a computer) or an idea (like decentralization) a solution without indicating what problem it is meant to address. Objects are objects. Ideas are ideas. They qualify as solutions, and we are justified in applying that concept to them, only when we show that they somehow address particular problems. It follows that streams of solutions and problems cannot possibly be independent of one another—unless commonly accepted definitions are abandoned.

The 1972 article defines a solution as "somebody's product" (p. 3), using a computer as an example. By this definition, there is no logical connection between solutions and problems, and the two can be "decoupled" into independent streams. GCT proponents can then talk provocatively about solutions chasing problems and problems chasing solutions, a standard ingredient of garbage can chaos. But all this turns, as we have suggested, on a definitional distortion. Their concept of solution is not the one everyone else uses; it is the same word invested with an entirely different meaning. Hence, the theory is not about solutions as we understand them. It is about something else. Had this something else been given a label of its own, this source of confusion would have been eliminated. But GCT would have appeared much narrower and less interesting, and its claims, of course, would have been very different.

This linchpin of GCT's framework, then, does not stand up to scrutiny. The notion of independent streams may appear at first blush to be an inspired simplification, but we are hard put to see how it can be justified.

Organizational Structure

The original article emphasizes organizational dynamics, but it also recognizes a significant role for structure, which influences how people, problems, and solutions get matched up in choice situations and what outcomes are generated as a result. What it says about structure, however, is very limited—and what it leaves out is crucial.[2] Two problems stand out.

The first arises because the early GCT takes structure as exogenous, thus ignoring where structure comes from or why it takes the forms it does. The theory's focus is on the effects of structure, not on its origin, design, or change. All else equal, we would not object to this. Something must be taken as exogenous; not everything can be explained. But in this case there is a heavy price to pay.

Precisely because structure does have a major effect on organizational processes and outcomes—and therefore on its effectiveness and its distribution of costs and benefits—decision makers are likely to regard it as a

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[1] This has been reported as an empirical finding by Stephen Weiner (1976), who contributed a case study to March and Olsen's (1976a) edited volume on GCT: "The San Francisco study stimulates a view of the flows of problems, solutions, and energy, when the choice is subject to a deadline, that varies from the theoretical predictions. Problems and solutions, on the one hand, and participants, on the other hand, are intertwined. *Problems and solutions are carried by participants*" (p. 245, emphasis added). But his observation was virtually ignored in the book's theoretical chapters. (For an extended examination of the idea of independent streams, see Heimer and Stinchcombe 1998.)

[2] See Padgett 1980 and Carley 1986 for criticisms of GCT's treatment of organizational structure.

key means of shaping organizational behavior.[3] This is unavoidable. To say that structure has important effects is to imply that it can be used to achieve certain ends. And participants, especially those in authority, have incentives to do just that. When leaders act on these incentives, then, they need not accept the garbage can's chaotic dynamics as uncontrollable facts of life. If GCT is correct in claiming that garbage can processes work very differently in different structures, then the theory's own logic suggests that leaders could choose structures with this in mind.

It is difficult to deny that the instrumental use of structure by leaders is pervasive and fundamental to an understanding of organizations. Yet, GCT essentially ignores it. The result is that GCT is a theory of short-run dynamics that fails to account for how structure would adjust over time to garbage can processes flowing within—and it thus fails to explain how organized anarchies would ultimately behave. The thrust of GCT is to highlight the chaos, perversity, and ineffectiveness of organized anarchy—and what the theory explicitly omits, it turns out, are the structural adjustments that could create more orderly and productive organizations. Absent structural choice, the deck is stacked.

The second problem with the authors' treatment of structure concerns the kinds of structure they consider relevant to an understanding of organization. It turns out that essential components of real organizations are simply omitted and thus can never figure into the garbage can's short-term dynamics. The result is a perspective that misses and mischaracterizes much of organizational behavior.

The most significant omissions bear on issues of authority, delegation, and control. Whether or not organizations have neat hierarchies, virtually all—including universities, the quintessential organized anarchies—have authority structures of some sort. In these systems, superiors can tell others what to do, hire subordinates to do their bidding, and delegate authority to them. They thereby create control opportunities: They can get much more done by extending their reach into far-flung decision arenas, even if they cannot personally participate. But they also create control problems: Subordinates will not necessarily do what is in the superior's best interests. Other structures, such as incentive schemes and monitoring systems, are used to promote as much congruence as possible. The structure of an organization largely reflects efforts to control and coordinate the actions of separate but interdependent individuals.

These issues are at the heart of organization and, not coincidentally, have long been at the core of organization theory. GCT, however, is built around a simplified framework of independent streams that largely ignores

them.[4] Although the authors recognize that some participants may be more "important" than others, in having more access to "important" decision arenas, there is no systematic attempt to recognize that participants are connected—notably, that many are agents of others—and that these connections are not accidental: They are designed to extend the reach of superiors. Instead, the world of the garbage can is made up of anomic individuals (the notion of a "stream" of participants says it all), and organizational decisions turn on who happens to wander into particular decision arenas.

Structure can induce order and productivity, but GCT does not allow participants to recognize as much and simply removes structure from their realm of choice. Were these issues of structure seriously integrated into the theory, it is hard to see how the garbage can's basic claims could be maintained.

Organized Anarchy

A fundamental GCT claim is that in a certain domain, organized anarchy, garbage can processes prevail. To test this hypothesis one must identify the domain. What, then, are organized anarchies (OAs)? Cohen, March, and Olsen (1972, 1) define them as "organizations characterized by problematic preferences, unclear technology, and fluid participation." But this definition, though widely cited, suffers from serious ambiguities that subsequent work has done little to clarify.

The first problem arises from the 1972 article's ambiguous statements about whether OAs must have all three properties or just some of them. Much of the discussion suggests that each property is necessary.[5] Yet, in the conclusion, OAs are described as situations that depart from classical decision models "in *some or all* of three important ways: preferences are problematic, technology is unclear, *or* participation is fluid" (p. 16, emphasis added); that is, each property suffices to identify an organized anarchy.

This confusion between the logic of "and" versus "or" creates two problems. First, the size of the concept's domain is unclear: It is much larger if only one property is needed to identify an OA than if all three are required. Second, it opens up a theoretical can of worms for the research program. For instance, under the "some or all" stipulation, an agency with clear goals, rigid participation, and unclear technology and one with clear goals, clear technology, and fluid participation would both be OAs. But why would we expect either to behave like garbage cans? In both cases preferences are unproblematic, so solutions could not chase problems; options that fail the (clearly) relevant criteria will simply not be considered solutions. Furthermore, what are the theoretical reasons for assign-

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[3] "Organized anarchies interfere with fundamental tactical principles that depend on tight control and coordination ... adaptations employed by naval commanders include the use of SOPs, operational plans, [and] *strong centralized authority acting through a chain of command*" (Weissinger-Baylon 1986, 51, emphasis added).

[4] A subsequent article (March and Romelaer 1976) does recognize an important role for authority, particularly in allowing superiors to delegate and thereby participate in many decision arenas simultaneously, but this interesting line of analysis has not been integrated into the larger structure of the GCT.

[5] See, for example, the preceding quote, where the three properties are linked by "and."

ing both agencies to the same set? Would not an organization with unclear technology and rigid participation behave differently from one with clear technology and fluid participation?

Finally, the meaning of each property in isolation is open to question. Consider universities, which are held up as paradigmatic organized anarchies. In what sense is the technology of teaching unclear? True, it is uncertain how to teach graduate students to become creative researchers. But it is known how to teach statistics, chemistry, and many other subjects: Certain sequences of readings, lectures, problem sets, and discussion sections produce a stable percentage of students who grasp the material, as verified by exams. Thus vital aspects of teaching are understood rather well. So is this an "unclear technology" or not? The garbage can literature gives us no guidance. Much the same could be said for problematic preferences and fluid participation.

GCT and the March-Simon Tradition

It is a common perception (e.g., Goodin 1999, pp. 71–2) that GCT, though now important enough to be considered a research program of its own, evolved from the earlier behavioral theories of March and Simon and thus can be considered an offspring of the decision-making school of organization theory. On the surface, this makes sense. After all, James March is a founder of both the Carnegie school and GCT, so one expects a strong continuity of ideas. Moreover, Cohen, March, and Olsen (1972) go to some lengths to describe their work in exactly this way, emphasizing that they "build on current behavioral theories of organizations" (p. 1), that their ideas about organizations "have a broader parentage" (p. 2) in that literature, and that their intentions are "conservative," aiming "to continue a course of theoretical development" with roots in the decision-making tradition (March and Olsen 1976c, 22).

With effort, one might make a case for this view. On the whole, however, it mischaracterizes the garbage can. The March-Simon tradition is grounded in a theory of individual choice. Although it relaxes classical rational choice assumptions to account for human cognition, it borrows heavily from economic methodology (e.g., Cyert and March 1963; Simon 1947), and its explanations are individualistic. It sees behavior as intendedly rational, it sees organizations as products of boundedly rational choice, and it seeks to model all this as clearly and simply as possible.

At the heart of the March-Simon tradition is an analysis of how cognitively constrained individuals solve problems and what this entails for organization. The theory forges a connection between bounded rationality and organizational structure, demonstrating that, precisely because individuals are limited in knowledge and computational abilities, they must rely on organizational routines and other forms of programmed behavior to solve their problems. Above all, the theory provides an explanation of structure, arguing that it arises out of the cognitively limited yet

intendedly rational behavior of very human problem solvers.

The garbage can's approach is very different. Three contrasts stand out.

First and most fundamentally, GCT rejects the key tenet of the entire Carnegie tradition: that individuals are intendedly rational. Indeed, it does not even portray individuals as caring about solving problems.

Second, GCT does not relax rational choice assumptions selectively, as March and Simon do, in order to retain some of the power and rigor of the methodology. It rejects all the relevant choice-theoretic assumptions and abandons the entire program of methodological individualism. In contrast, what the March-Simon tradition says about organizations is firmly rooted in a coherent model of individual choice. That is its analytic foundation.

Third, the basic aspects of organization that March and Simon seek to explain—structure (especially) and process—are treated by GCT as exogenous. No explanation is given. The aim of GCT is to explain outcomes, not organization. The aim of the March-Simon tradition is to explain organization.

It is important not to confuse GCT with its authors, who have surely been influenced by the March-Simon tradition. Analytically, however, GCT is only tenuously connected to that tradition; indeed, it rejects the decision school's core components. So it is misleading to think of the current formulation of GCT as an outgrowth of bounded rationality conceptions of organization. A clear, cold look at the theory suggests that it has much more in common with the institutional school of organization theory (e.g., Meyer and Rowan 1977), which also rejects the idea that organizations can be understood in terms of the rational behavior of their members. Institutional theory emphasizes other influences—symbols, myths, legitimacy—that are reflected, nonrationally or even irrationally, in the foundations of organization. As the GCT authors develop their larger theory, it is evident that this is the branch of organization theory where the garbage can is most at home.

THE SIMULATION MODEL

The computer simulation formalizes the verbal theory and is widely regarded as the research program's scientific core. We agree that formal modeling is desirable, but the specific model they develop misrepresents the theory and exacerbates the GCT's problems. To show this, we need to describe the model's basic assumptions and how they generate outcomes.[6]

A Description of the Model

The computer model simulates the behavior of an organized anarchy over twenty periods, in which the various organizational streams interact to produce outcomes. The choice stream consists of ten choice

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[6] For a more detailed analysis of the simulation, see Bendor, Moe, and Shotts 1996.

arenas (the garbage cans), the participant stream of ten decision makers, and the problem stream of twenty problems.

This much follows the informal theory. But notice, right at the outset, what is missing. Whereas the verbal theory posits a central role for streams of choices, participants, problems, *and* solutions, *the computer model has no stream of solutions.* No explanation is ever offered as to why this stream is missing. Given the theory, the temporal pairing of problems with solutions ought to be fundamental. And it goes without saying that any notion of "solutions chasing problems and problems chasing solutions"—perhaps the most famous property of the verbal theory—cannot be part of the model at all if it does not include solutions in the first place.

Let us put this aside for now, however, and return to describing the computer model as it was constructed. The temporal sequencing is as follows. During each of the first ten periods, one new choice arena and two new problems enter the simulation, where they may be acted upon by the organization's decision makers, all ten of whom are assumed to be present from the start. By the tenth period, all choices and problems have been introduced. There are no new inputs during the last ten periods.

During each period, each problem and each decision maker attaches itself to (at most) one choice arena. They "decide" where to go, subject to two constraints. First, they cannot go to a choice that has not yet been introduced or has already been made. Second, their options are limited by the organization's "access structure" (for problems) and "decision structure" (for participants).

The access structure determines which problems have access to which choice arenas. In an unsegmented access structure, the problems can go anywhere: They can attach themselves to any active choice arena. In the hierarchical structure, problems are ranked by importance: The two most important problems can go to any choice arena, the next two can go to nine of the ten choice arenas (the off-limits arena being "too important" for them), and so on, down to the two least important problems, which can go only to one of the ten arenas. In the specialized access structure, each problem can go only to one particular choice arena: Two of the problems can go only to arena #1, two can go only to arena #2, and so on.

Similarly, the decision structure determines where decision makers can go. In the unsegmented decision structure, any agent can work on any choice. In the hierarchical structure, important decision makers have access to more choice arenas than do less important ones. And in the specialized structure, each agent can work in only one particular arena.

Within these constraints, each problem and decision maker follows a simple rule in selecting a choice arena: Go to the choice that is closest to being made. This rule is a crucial assumption that drives many of the results. We will discuss how it works in more detail below, after describing how choices are made.

How Choices Get Made

In the model, the key concept for understanding how choices get made is "energy"—a concept that has no role in the informal theory. The model, unlike the theory, sees choices as the product of an energy balancing mechanism. It works as follows.

Each problem requires a certain amount of energy to be solved. This requirement is the same for all problems in an organization but can vary across organizations depending on the "load" they face. (The higher the load, the more energy is required per problem.) The energy required to make a choice is the sum of the energy requirements of the problems attached to that arena.

Decision makers supply the energy that gets things done. In each period a participant supplies a fixed amount of energy to the choice on which he is currently working. Not all this energy is assumed usable, however, in solving problems. The usable energy is derived by multiplying the available energy by a fraction, set in the model at 0.6. The authors interpret this fraction as representing "solutions" in the simulation. Presumably, larger fractions mean more usable energy and thus more solution power. To reiterate, however, there is no stream of solutions, as the verbal theory requires. Indeed, apart from this fixed fraction, solutions play no role in the model.

In any period, the energy supplied to a choice arena is the sum of the amounts brought by each of the participants to that arena. Curiously, this energy supply is assumed to cumulate over time: The total energy in a choice arena is the sum of all the energy anyone has ever brought to that arena since that choice was first introduced. This creates the possibility of "ghost" energy (our term): A choice arena may have usable energy in a period even though no one is in the arena at that time.

If the total usable energy equals or exceeds the energy required by the problems currently attached to the arena, then the choice is made and the problems are solved. Because of ghost energy, however, problems may get solved even though no one is present to work on them. More perplexing still, a choice can be "made" *even when there are no problems in the choice arena at all.* Indeed, this is likely, because when there are no problems the energy requirement is zero, and the presence of any agents or (even without participants) any ghost energy automatically satisfies the energy requirement. By the simulation's logic, then, a choice can be "made" even though no problems are solved, no one is present—and, quite literally, nothing happens.

This is not, of course, what is normally meant when we say a choice has been "made." To claim, as the model does, that a choice is "made" when nothing is happening is an odd use of language and ultimately a source of serious confusion. It is better to say, under these circumstances, that the choice arena closes down, or the meeting adjourns without any decisions being taken. This accurately describes what actually happens, and it does not imply that choices are being made when

they are not. We will return to this peculiar form of decision "making" below, for it plays an important part in GCT analysis.

Finally, we want to be clear about how energy relates to the movement of people and problems. As noted, both follow a simple decision rule: Go to the choice that is closest to being made. Reinterpreted in energy terms, this rule says: Go to the choice arena with the smallest energy deficit, that is, the smallest gap between the energy required to solve problems and the energy supplied by agents (including ghost energy). In each period, problems and participants look over all choice arenas, calculate each arena's energy deficit, and move in the next period to the open arena with the smallest deficit (subject to the constraints of the access and decision structures).

The Verbal and Formal Theories: Different Worlds

These are the model's essential components, which largely determine how it simulates the behavior of organized anarchies. We have already pointed out some serious design problems: the absence of a solution stream, for instance, and that choices can be "made" even when no people or problems are present. Before we go on, we want to add four general observations.

First, the informal version of the GCT is clearly an information-processing formulation. It refers to intelligence in the face of ambiguity, to technologies that are poorly understood, to the allocation of attention. All these ideas are fundamentally cognitive or informational. The simulation is markedly different. It is built around the concept of energy; information and beliefs of decision makers are not explicitly represented at all. Instead, participants are simply carriers of energy. In addition, problems are defined in terms of energy requirements, solutions are coefficients that convert available energy to usable energy, people and problems move around in response to energy deficits, and choices are made when energy deficits are overcome. Nothing here resembles the verbal theory.

Second, the model's assumptions about participants are especially odd. They are basically bundles of energy—virtually automatons. They have no explicit objectives and are essentially unmotivated. They do follow a decision rule: They go to choices that are closest to being made. But the authors do not suggest that this reflects an underlying goal, nor do they explain why decision makers might want to do this. What we do know about participants is that they do not care in the slightest about solving problems. They are perfectly happy to go to an arena that contains no problems and participate in the "decision." Indeed, because such a can will have a light energy load, their decision rule implies that they actually prefer it to one with problems. It is hardly surprising, then, that the simulation ultimately shows that organizations are bad at solving problems. No one is interested in solving them.

Third, the verbal theory is about a stochastic world filled with ambiguity, chaos, and randomly intersecting streams of activity. Its depiction of organizational dynamics is built around randomness and the uncertain confluence of events. But the simulation is fundamentally deterministic. People and problems move at the same time, using the same deterministic rule. And the technology assures that choices will always get made in the same way when certain energy requirements are met. The world created by the computer simulation is dynamic—people and problems move around over time—but it is clearly not the stochastic world the informal theory describes, because it omits the very essence of what that world is supposed to be about.

Fourth, if we accept the interpretation that the three properties of problematic preferences, unclear technology, and fluid participation are individually necessary and jointly sufficient for something to be an organized anarchy, then this central concept of the informal theory is poorly represented by the simulation. Compare these defining properties of an OA to the simulation's nine structural variants, shown in Figure 1.[7] When the access structure is hierarchical (versions corresponding to cells 4, 5, and 6), problems are ranked by importance. Thus, the organization's preference ordering is completely coherent; its preferences are not problematic at all.[8] Regarding technology, versions in cells 7, 8, and 9 have specialized access structures: Problems are coded by type and go to appropriately coded choice arenas. This is a clear technology: The organization knows enough about problems to categorize and route them correctly. Finally, participation is completely rigid in cells 3, 6, and 9, for these have specialized decision structures that restrict each agent to a single choice arena; it is semirigid in cells 2, 5, and 8, where lower level decision makers are sharply constrained. Thus, to remain true to the verbal theory's central ideas, the simulation should have highlighted a single variant: the unsegmented-unsegmented version. This is not merely one version of the model; it is the prototypical organized anarchy.

A World of Remarkable Order

Given this design, it is not surprising that the computer model leads to very odd and inappropriate implications for the behavior of organized anarchies, implications that clash with the informal theory. This is not obvious from the original article, which does not present detailed information on how the simulated organizations function or what outcomes are produced. Instead, it gives summary statistics about things like "decision style" and "problem activity." But when the simulations are rerun (as we did, using the original FORTRAN program), the gulf between model and theory is strikingly clear.

This disconnect can be illustrated in many ways, for the underlying design problems leave their fingerprints

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[7] Figure 1 is a graphical representation of text information in Cohen, March, and Olsen 1972.

[8] Moreover, as we have seen in the analysis of choice behavior in the simulation, to the extent that individuals have goals, they all "want" the same thing: to go to arenas that are close to finishing up. Hence their interests do not conflict at all.

FIGURE 1. The Simulation's Nine Structural Variants

Decision Structure

		Unsegmented	Hierarchical	Specialized
	Unsegmented	**1**	**2** Semirigid Participation	**3** Rigid Participation
Access Structure	Hierarchical	**4** Unproblematic Preferences	**5** Semirigid Participation; Unproblematic Preferences	**6** Rigid Participation; Unproblematic Preferences
	Specialized	**7** Clear Technology	**8** Semirigid Participation; Clear Technology	**9** Rigid Participation; Clear Technology

Note: The decision structure maps decision makers onto choice arenas. The access structure maps problems onto choice arenas. Each cell represents a variant of the simulation.

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almost everywhere. We will focus on one problem that is absolutely fundamental—and fatal.

The heart of the verbal theory is the notion of independent streams of participants, problems, and solutions intersecting unpredictably in organizational garbage cans—choice arenas—to generate organizational outcomes. This is basic. Perhaps the most important question we can ask of the computer model, then, is whether it reflects this central feature of the informal theory. The obvious place to look for this brand of chaotic behavior is in the paradigmatic organized anarchy, where both the access and decision structures are unsegmented. In these "unsegmented-unsegmented" organizations, anyone can work on any choice, any choice arena can be burdened by any problem, and virtually any permutation of choices, participants, and problems seems possible. If the model can produce garbage-can-like behavior at all, then one would surely expect to find it in this prototypical case.

To see, we re-ran the purely unsegmented version under all three energy loads: light, medium, and heavy. The results are illustrated in figures 2, 3, and 4. To simplify, we have scaled these figures down by half in every dimension: Each depicts only five choice arenas, five decision makers, ten problems, and ten periods. This scaling loses no essential information and makes the figures easier to read. The text's descriptions of the simulation follow the figures and so are also scaled down by half.[9]

Consider the light load first (Figure 2). In the initial period, one choice arena and two problems enter the simulation.[10] Because only one arena is open, both problems and all decision makers move—as a pack—to that one. The sum of the participants' energy inputs exceeds the problems' energy requirements, so the choice is made (via the energy balancing mechanism described earlier), and the problems are resolved. In each of the next four periods, exactly the same thing happens: Decision makers and problems travel in a pack from arena to arena, so that each choice is made, and each problem is resolved, immediately upon entering the simulation. The organization is extremely orderly and totally effective.

Under a medium load, with more energy needed to solve each problem, the organization does worse. Nonetheless, the main behavioral pattern is much the same: From period three on, all agents and all active problems move together in a pack (Figure 3).[11] Here is what happens. When a new choice enters the simulation after the second period, all players go to that arena. There they supply energy, but because it is less than the problems' energy requirements, the choice is not made. In the next period, the participants and problems all go to the next new garbage can that opens up. The old choice that they left behind is now "made," even though it contains neither decision makers nor

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[9] For example, when we say "at the end of period five all five choices are active," this is exactly what the corresponding figure (Figure 4) portrays. In the full-scale simulation, all the choices (all ten) are not activated until period ten.

[10] The order in which choices and problems enter is irrelevant. Our figures use the following orders: for choices it is 3, 1, 5, 4, 2; for problems it is 8, 6, 7, 3, 2, 9, 4, 1, 5, 10.

[11] In period 2, the decision makers separate from the problems due to a slight difference in how energy balances are calculated. The details of this assumption are unimportant.

FIGURE 2. An Unsegmented-Unsegmented Organization Facing a Low Load

Choice Arena

	#1	#2	#3	#4	#5

Period

Period

Period

Period

Period

Period 6

Period 7

Period 8

Period 9

Period 10

In Period 1, arena #3 is active with Prob 8,6 and DM All.

In Period 2, arena #1 is active with Prob 7,3 and DM All.

In Period 3, arena #5 is active with Prob 2,9 and DM All.

In Period 4, arena #4 is active with Prob 4,1 and DM All.

In Period 5, arena #2 is active with Prob 5,10 and DM All.

Key

Not yet active, has not yet entered the stimulation.

Prob	DM
2,7	4,3,5

Active, does get made this period. The attached problems (2 and 7 in this example) are solved.

No longer active; already made.

Note: Each box shows the status of a particular choice arena in a particular period. Each column shows how an arena's status changes over time. A choice arena must be in one of four states: (1) not yet activated; (2) active, choice not yet "made" in the current period (none in this figure); (3) active, choice does get "made" in the current period; (4) deactivated (after a choice was made).

FIGURE 3. An Unsegmented-Unsegmented Organization Facing a Medium Load

Choice Arena

	#1	#2	#3	#4	#5

Period 1: #3 — Prob 8,6 / DM All

Period 2: #1 — Prob None / DM All; #3 — Prob 8,6,7,3, / DM None

Period 3: #3 — Prob None / DM None; #4 — Prob 8,6,7, 3,2,9, / DM All

Period 4: #3 — Prob 8,6,7,3, 2,9,4,1, / DM All; #4 — Prob None / DM None

Period 5: #2 — Prob 8,6,7,3, 2,9,4,1, 5,10 / DM All; #4 — Prob None / DM None

Period 6: #2 — Prob 8,6,7,3, 2,9,4,1, 5,10 / DM All

Period 7: #2 — Prob 8,6,7,3, 2,9,4,1, 5,10 / DM All

Period 8: #2 — Prob 8,6,7,3, 2,9,4,1, 5,10 / DM All

Period 9: #2 — Prob 8,6,7,3, 2,9,4,1, 5,10 / DM All

Period 10: #2 — Prob 8,6,7,3, 2,9,4,1, 5,10 / DM All

Key

(vertical lines box)	Not yet active, has not yet entered the simulation.
Prob 2,7 / DM 4,3,5 (thin border)	Active, does not get made this period. Problems 2 and 7 (for example) are attached to this choice in this period. Decision makers 4, 3, and 5 are working on this choice in this period.
Prob 2,7 / DM 4,3,5 (thick border)	Active, does get made this period. The attached problems (2 and 7) are solved.
(crosshatch box)	No longer active; already made.

FIGURE 4. An Unsegmented-Unsegmented Organization Facing a High Load

Choice Arena

	#1	#2	#3	#4	#5

Period 1			Prob 8,6 / DM All		
Period 2	Prob 7,3 / DM All		Prob 8,6 / DM None		
Period 3	Prob 7,3 / DM None		Prob 8,6 / DM None		Prob 2,9 / DM All
Period 4	Prob 7,3 / DM None		Prob 8,6 / DM None	Prob 4,1 / DM All	Prob 2,9 / DM None
Period 5	Prob 7,3 / DM None	Prob 5,10 / DM All	Prob 8,6 / DM None	Prob 4,1 / DM None	Prob 2,9 / DM None
Period 6	Prob 7,3 / DM All	Prob 5,10 / DM None	Prob 8,6 / DM None	Prob 4,1 / DM None	Prob 2,9 / DM None
Period 7		Prob 5,10 / DM All	Prob 8,6 / DM None	Prob 4,1 / DM None	Prob 2,9 / DM None
Period 8			Prob 8,6 / DM All	Prob 4,1 / DM None	Prob 2,9 / DM None
Period 9				Prob 4,1 / DM All	Prob 2,9 / DM None
Period 10					Prob 2,9 / DM All

Key

Not yet active, has not yet entered the simulation.

Prob	DM
2,7	4,3,5

Active, does not get made this period. Problems 2 and 7 (for example) are attached to this choice in this period. Decision makers 4, 3, and 5 are working on this choice in this period.

Prob	DM
2,7	4,3,5

Active, does get made this period. The attached problems (2 and 7) are solved.

No longer active; already made.

problems. This happens because its ghost energy (energy cumulated over past periods) now exceeds the energy demanded of it—which is zero, simply because all the problems have left. In the second half of the simulation, no new choice arenas enter the process, so all the participants and problems must remain in one arena (the last one that entered) for the rest of the simulation. They have no place to go, because all the other choices have been "made" (with no problems or people present). In each period the decision makers exert more energy in that arena, but the simulation ends before enough has accumulated to solve the problems. Thus, the organization facing a medium energy load is completely ineffective. It solves no problems.

In a bizarre turnabout, however, going from a medium to a high load makes the organization totally effective again (Figure 4). In the first period two problems enter and go to the one available arena. All participants go to this choice—again traveling in a pack—but they lack the energy to solve the problems. In the second period a new arena enters the simulation, and two new problems enter and go to this choice. The old problems remain at the old garbage can.[12] The entire pack of decision makers then goes to the new arena but again lacks the energy to solve the attached problems. In periods three, four, and five, exactly the same thing happens: All the participants and two new problems go to the newly entered arena; all old problems stay at the old cans. At the end of period five all five arenas are active, each with two attached problems. Each choice has exactly the same energy balance because each has received one period's worth of energy from decision makers. In period six, all decision makers return in a pack to choice arena #1. There they exert additional energy, make the choice, and resolve the attached problems. All participants then go en masse to arena #2, arena #3, and so on, in order. Hence, the pure unsegmented system facing a heavy load is completely effective, just like the organization facing the light load.

Thus, the three load versions present a peculiarly nonmonotonic pattern. Increasing the load initially makes the organization completely ineffective, but increasing the load once more sends problem-solving effectiveness soaring back to 100%. Sometimes nonmonotonic patterns are bold insights, but we doubt that this one is. The patterns bear no resemblance to expectations based on the informal theory. The latter certainly does not lead us to expect that a heavily burdened organized anarchy will be extremely effective at solving problems.

This is troubling, but the more profound problem is that the computer model generates clockwork dynamics that do not remotely resemble the decision processes associated with a disorderly garbage can. *The simulation's hallmark in the prototypical organized anar-*

chy *is that decision makers always move together in a single pack.* This pattern, moreover, is not confined to the unsegmented-unsegmented case highlighted here. It holds for all simulations with unsegmented decision structures, regardless of the other parameters of the model. The reason is straightforward. In these simulations, all decision makers start in the same place (the first can that opens) and use the same rule to decide where to go in every period. These features, together with synchronous adjustment, imply that they virtually[13] always stay together.[14]

It is difficult to imagine a more fundamental disjunction between model and theory. The computer model is supposed to represent the disorderly world of garbage can decision processes, but even in the prototypical case it generates an incredible degree of order. This is so clearly incompatible with the independent, randomly intersecting streams of the verbal theory that the simulations can offer no real insight into the workings of organized anarchies. They are from another world.

A New Look at Well-Known Properties of the Garbage Can

Given the problems at work here, there is little point in examining the model's implications at length. They inevitably reflect the model's basic design, which is seriously at odds with the informal theory. We do, however, want to focus briefly on two well-known claims about organized anarchy that emerged from these simulations. Over the years, they have been presented by the authors and others as basic properties of organized anarchies, and as central components of the theory itself.

The first is that "resolution of problems is not the most common style of making decisions" in organized anarchies (March and Olsen 1989, 13); decisions are more often made by flight or oversight. Decision making by flight and oversight are by now famous properties of the garbage can, conjuring up visions of a paradoxical organizational world in which many decisions get made, but problems are not resolved.

In fact, there is nothing paradoxical here. These properties are simply artifacts of the model's inappropriate assumptions. Flight occurs when all the problems attached to a choice arena leave, and the choice is subsequently "made." Oversight occurs when a choice is "made" before any problem attaches itself to the

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[12] The old and new problems go to different cans because the computer program assumes that new problems, when they first appear, calculate energy deficits somewhat differently than existing problems do. The details of this assumption are not important.

[13] It is possible for agents to separate in simulation variants in which they have unequal amounts of energy. In simulations with unsegmented decision structures, however, all decision makers travel in a single pack more than 99% of the time.

[14] Indeed, not only do the participants move simultaneously, but the problems do so as well: Being synchronized with the movement of decision makers, they all move at the same time. It is true that the assumption of synchronous adjustment is often used in computer simulation. But this is in general a risky modeling strategy: Synchronicity can produce artifactual results that disappear if even small asynchronies are introduced (Huberman and Glance 1993). Furthermore, in the special context of organized anarchies, it is substantively inappropriate to assume that decision makers and problems adjust synchronously.

arena. In neither case are decisions actually made; indeed, *nothing happens*. In ordinary English, decisions involve selecting an alternative. The model, however, uses its definitional fiat to brand truly vacuous events as "decisions." Because its own assumptions guarantee that empty choice arenas have zero energy requirements, its own logic of energy balancing guarantees that all these "decisions" will get "made." The much-touted prominence of flight and oversight, therefore, is literally built into the model, an artifact of unwarranted assumptions; it says nothing about how organizations make decisions under ambiguity.

A second famous property associated with the garbage can is that decision makers and problems tend to track one another. This is presented as an interesting pattern in the otherwise mysteriously confusing complexity of garbage-can dynamics. We make two points here. First, even if the model and its implications are taken at face value, the tracking phenomenon cannot properly be regarded as a general pattern of the simulation, for it does not and cannot hold for five of the nine variants. In systems with specialized access structures (versions 7, 8, and 9 in Figure 1) problems can go to only one choice arena, so they cannot track participants. In systems with specialized decision structures (versions 3, 6, and 9) participants can only go to one choice arena; they cannot track problems. Only in the other four versions is it even theoretically possible for decision makers and problems to track one another. So claims that the tracking phenomenon is a central feature of the simulation would be misleading even if the model were acceptable.

But it is not. The model assumes (with a few minor wrinkles) that all decision makers and all problems make the same calculations simultaneously: They go to the arena with the smallest energy deficit. In general, therefore, when the access and decision structures allow them to go to the same, smallest-deficit arena, that is what they do. They go together, *in a pack*, which is another way of saying that they track one another. This, then, is our second (and more important) objection to the famous claim that tracking is a central property of organized anarchy: Like the prominence of flight and oversight, it is simply guaranteed by the model's inappropriate assumptions about how participants and problems calculate where to go. It is an artifact of the design, and it tells us nothing about what we should really expect from organized anarchies in the informal theory, which clearly does not lead us to expect pack behavior.

Summary Comments

Formalization often helps science progress, but it is not a panacea. In this case a heavy price was paid, for the verbal theory and the computer model do not represent the same phenomena at all. Decision makers in the verbal theory confront a chaotic world in which they, solutions, and problems dance around one another, meeting by chance in choice arenas. But in the simulation, packs of decision makers—and often prob-

lems as well—march in lockstep from arena to arena. And solutions never move at all.

This is ironic. The informal theory of the garbage can is famous for depicting a world that is much more complex than that described by classical theories of organizational choice. The latter's tidy image of goal specification, alternative generation, evaluation, and choice is replaced by a complex swirl of problems looking for solutions, solutions looking for problems, participants wandering around looking for work, and all three searching for choice opportunities. Yet, the simulation depicts almost none of this and in fact creates a world of remarkable order.

Garbage Cans and Universities

In the final section of the 1972 article, the authors apply their analysis to what the literature now routinely spotlights as the quintessential organized anarchy: universities. They aim to show that GCT is more than just an abstract line of reasoning and can be used to shed new light on an important class of real organizations. This analysis has been influential (foreshadowing Cohen and March 1974 [1986] and March and Olsen 1976a) and is thus worth discussing here, if briefly.

The authors begin by asserting that universities conform to the basic implications of the theory, which they summarize as follows:

> University decisionmaking frequently does not resolve problems. Choices are often made by flight or oversight. University decision processes are sensitive to increases in load. Active decision makers and problems track one another through a series of choices without appreciable progress in solving problems. Important choices are not likely to solve problems (1972, 11).

Note that these implications are based without modification on the simulation, not on the verbal theory. This is an early, important, and very clear example of what has been going on in the literature ever since: The computer model and its implications have been bonded to the theory. The two have become one.

Applying the simulation is only the first step in the authors' attempt to develop a garbage-can analysis of universities. To derive more detailed predictions they extend the model by adding auxiliary assumptions, which posit relations between the model's properties (e.g., access structure) and other variables, such as a university's size and wealth.

In principle, of course, it is legitimate to introduce auxiliary assumptions, particularly when applying a general theory to a specific domain. But it is important to recognize that in this case the new assumptions are not part of GCT in any of its guises. They belong neither to the informal theory nor to the computer model. They are freestanding assertions, introduced casually and with relatively little discussion. Here are the sorts of claims they entail.

1. In bad times, the access structures of large rich schools are hierarchical; those of small rich ones are unsegmented (Cohen, March, and Olsen 1972, 15, Figure 4).

2. As times improve, the access structures of large rich schools change from hierarchical to specialized; the same environmental change has no effect on the access structures of small rich universities or poor ones (p. 15).

3. As times worsen, large rich schools and small poor ones change their decision structures; large poor schools and small rich ones do not (p. 15, Figure 5).

No justification, theoretical or empirical, is given for these assumptions. They are simply employed, along with the simulation, to generate predictions about university decision making, predictions that are supposed to represent what GCT says about this important class of organizations.[15]

The predictions cannot do that, however, because they are based on premises of sand. This is not a meaningful application of GCT to universities. The assumptions and implications of the simulation are largely unwarranted and have little to do with the verbal theory. And the auxiliary assumptions about universities are questionable, unrelated to the theory, and offered with scant justification.

RECENT DEVELOPMENTS

We have now taken a close look at both key branches in the early development of GCT, the informal theory and the computer model. As our analysis suggests, this early work left much to be desired. Yet, such a story is not unusual in the early stages of a research program. Path-breaking work is often crudely developed. Its real function is to shake up conventional ways of thinking. With that done, the kinks, gaps, rough edges, and unexplored implications can eventually be worked out to yield a coherent, well-defined theory. But has this happened with the garbage can? Nearly three decades have gone by, and a substantial literature has expanded upon the original research. Let us see what this follow-on work has produced.

The Computer Model as a Source of Confusion

The computer model became a source of confusion almost immediately. Although the 1972 article begins with a clear distinction between the "basic ideas" and the attempt to model them via simulation, it later conflates the two. The GCT is summarized by statements such as "it is clear that the garbage can process does not resolve problems well," or that, quite often, decisions get made only "after problems have left a given choice arena or before they have discovered it (decisions by flight or oversight)" (Cohen, March, and Olsen 1972, 16). It is as though these simulation results, rather than being one set of implications from one possible model, were central to an understanding of

garbage can processes. The model was fused to the theory.

In more recent work, this hybridized GCT, an undifferentiated blend of informal theory and computer model, is the norm rather than the exception.[16] Decisions by flight and oversight, access structures, decision structures, the tendency of decision makers and problems to track one another across choice arenas, the sensitivity of the system to variations in load—these and other components of the computer model tend to be presented as central, even defining features of garbage can processes in general, not as the highly specific assumptions or findings of one particular model.

This conflation of theory and model is especially debilitating because the computer model formalizes the theory so poorly. As the model's findings and building blocks have been tightly woven into the fabric of GCT, the "theory" itself has been transformed by claims and properties that are not legitimately part of the theory at all and that distort its real message. This would not have happened had the theory been granted a life of its own, separate from the model. Nor would it have happened had critics pounced on the model from the outset, exposed its inadequacies, and prevented it from gaining such prominence and influence. As things have developed, however, the garbage can literature has been profoundly and subtly shaped by an invalid model, to the point that GCT and its central ideas have become the model's illegitimate offspring. They are what they are, in good measure, because the computer model is what it is.

The Computer Model, Science, and the Liberation from Science

We refer, here and elsewhere, to "the" computer model. This is because Cohen, March, and Olsen did not develop other garbage can models after their 1972 article. A few others have tried their hand at it (Anderson and Fischer 1986; Carley 1986; Padgett 1980).[17] Unfortunately, their models have not gained anything like the attention accorded the original, and their modifications have not been assimilated into mainstream organization theory's understanding of GCT.[18]

[15] These predictions are reproduced in toto in Cohen and March 1986 (Table 28, p. 88). By this point, however, the extended model's auxiliary assumptions not only are unjustified but also are *invisible*: The book omitted the 1972 article's original figures that had repre-sented the auxiliary assumptions.

[16] See, e.g., March and Olsen 1984, 746; 1986, 17–8; 1989, 11–4; Mezias and Scarselletta 1994, 658–62; Moch and Pondy 1977, 354–5; and Weissinger-Baylon 1986, 37–40. For exceptions see Crecine, 1986, 86; March 1994, 201; and Masuch and LaPotin 1989, 40.

[17] Interestingly, each of these works abandons key components of the GCT. Padgett (1980) and Carley (1986) drop the assumption that participation is fluid. Anderson and Fischer (1986, 145) modify the assumption of independent streams: "Individuals are the vehicles which carry solutions and parochial problems to choice opportunities." Most important, all three emphasize the instrumental nature of individual and organizational action (Anderson and Fischer 1986, 153–4; Carley 1986, 165, 177; Padgett 1980, 585, 591).

[18] We are not sure why this happened. One colleague (Carley, personal communication, 1996) has suggested that the problem is that simulation is not part of the standard repertoire of doctoral students. (See Masuch and LaPotin 1989, 38, for other explanations.) Thus, mainstream organization theorists have overlooked technical challenges and new simulations, attending instead to Cohen, March, and Olsen's own verbal accounts of their simulation.

Apart from this small body of technical work, the field has not benefited from a proliferation of formal models. The nontechnical literature on the garbage can has continued to center on just the original model, which has survived for nearly thirty years without challenge or change from the mainstream.

Throughout, this computer model has served as GCT's scientific core. However perplexing the authors' discussions of the garbage can, underpinning it all was a literature-wide recognition that the informal ideas could be rigorously formalized, implications deduced, and tests conducted. The computer model was the justification, for it demonstrated, by sophisticated analytical means, that all this was indeed possible. It thereby bestowed scientific legitimacy on the entire GCT enterprise, no matter how unscientific it might otherwise seem. Thus, the computer model has liberated the rest of GCT to throw off the shackles of science and go its own way. The result has been an expanding, eclectic collection of loosely coupled ideas that have somehow seemed relevant to organized anarchy. Much of this is mind-bogglingly complex and vaguely expressed, and it is never explained how all these elements can be organized into a coherent and powerful theory.

The Framework Expanded

Since 1972 the authors (primarily March and Olsen) have moved beyond their initial formulation to assemble a larger, more general theoretical apparatus. All of the more recent work is rooted in the GCT and has inherited, as a result, its fundamental problems. Indeed, in important respects the newer work is actually more complex and confusing than GCT ever was.

At the heart of this entire research tradition is a general theme that is a hallmark of the GCT's verbal theory: the juxtaposition of ambiguity and socially constructed order. The centrality of the first pillar was well reflected in the title and content of March and Olsen's 1976 book, *Ambiguity and Choice in Organizations*, and of many other publications.[19] But the second pillar is also central. Their work consistently emphasizes an interplay between the two. As March and Olsen (1984, 743) put it in their seminal statement on the new institutionalism, "institutional thinking emphasizes the part played by institutional structures in imposing elements of order on a potentially inchoate world."

In the 1972 article, ambiguity meant organized anarchy's triad of properties (ambiguous goals, unclear technologies, fluid participation), and socially constructed order—what they would later call temporal order—was that allegedly produced by garbage can decision processes. This particular juxtaposition between latent chaos and institutional order is often referred to in later work.[20] But they have also added

new types of ambiguity and of order. On the first dimension they have added the ambiguities of experience and of the past (Cohen and March 1986, 199–201; March and Olsen 1975), of power and of success (Cohen and March 1986, 197–9, 201–3), of relevance (March and Olsen 1976a, 26; March 1988, 390), of self-interest and of deadlines (March and Olsen 1976a, chap. 2 and p. 226, respectively), and of intelligence and of meaning (March 1988, 391–5). On the second they have added, among other things, symbolic order (March and Olsen 1984, 744), normative order (p. 744), and, most prominently, interpretive order (March and Olsen 1976a, chaps. 4 and 15; 1989, chap. 3).[21]

This sprawling theoretical framework is so complicated and ill-specified that there is very little chance of recasting it as a coherent formal structure. Consider, for instance, its treatment of individual choice. March and Olsen have often argued that their work is fundamentally about individual choice, with deep roots in the bounded rationality tradition. Presumably because the 1972 article did not actually use or develop any theory at the micro level, a defining feature of their post-1972 work has been an effort to map out a theory of individual choice that could be a micro foundation for the new research program.

How have they done this? Although they claim they are engaging in the "gradual relaxation of rigid assumptions in classical theories of choice" (March and Olsen 1986, 28), and thus following in Simon's footsteps, they actually abandon the framework of bounded rationality—and of rational choice, too—and strike out on their own. The result is a free-floating discussion of decision making that has no clear framework. Here are some of the factors that *Ambiguity and Choice* argues should be included in a theory of individual choice.

First, it must recognize that "beliefs and preferences appear to be the results of behavior as much as they are the determinants of it. Motives and intentions are discovered post factum" (March and Olsen 1976c, 15). Hence, the bedrock components of decision theories are to be treated as endogenous. The problem is that, although exogenous parameters are vital to the generation of predictions in any testable theory, nothing about individual choice is taken as exogenous here.

Second, it must recognize that individual preferences are shaped less by self-interest or problem solving than by roles, duties, and obligations, and by "the definition of truth and virtue in the organization, the allocation of status, the maintenance or change of friendship, goodwill, loyalty, and legitimacy; [and] the definition and redefinition of 'group interest' " (p. 16).

Third, it must recognize that beliefs are shaped by

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[19] See, for example, Cohen and March 1986, 195–203; March 1978, 1988, 1994, chap. 5; March and Olsen 1975; and March and Weissinger-Baylon 1986, chaps. 1–2.

[20] See, for example, Cohen and March 1986, xv, 91, 195–215; March

1994, 177–80; and March and Olsen 1976a, 1984, 738–41, 743–4, 1989, 11–4).

[21] This newer work has a postmodernist flavor. The emphases on language and symbols, on meaning and its social construction, and on the chaos that lurks beneath our delicately maintained interpretations—are all postmodernist themes. These ideas are only touched on in the 1972 article but are given much more attention in *Ambiguity and Choice*. Indeed, the latter may well be the first sustained postmodernist study of organizations—a fact recognized early on by Perrow (1986, 137–8).

decision makers' interpretations and by "myths, fictions, legends, and illusions" (p. 18). All this and more would be part of a larger theory of learning that is needed to explain the evolution of beliefs.

Fourth, it must recognize that people do not attend to all issues. Their involvement is selective, often for reasons unrelated to substance, and any explanation of this behavior—a theory of attention—must emphasize "duty, tradition, and routine"; the "educational, ideological, and symbolic role of choice situations in organizations"; and how each person's opportunities depend on what everyone else is doing (March and Olsen 1976b, 45).

This book's perspective on choice, which was complicated even further in their later work, may be a plausible description of the countless forces that affect people's decisions. But description is not theory. And we do not see how this enormously complicated tangle of possibly relevant factors can ever lead to a rigorous, productive theory. Theories are supposed to reduce complexity, not surrender to it.

Despite these difficulties, *Ambiguity and Choice* quickly had a substantial effect on the field of organizations. Its influence on political science lagged a bit, but in the 1980s several works that were highly visible to political scientists disseminated its ideas in a major way. In 1983 the *American Political Science Review* published March and Olsen's article on administrative reorganization, in which garbage can concepts figured prominently.[22] The research program got an even bigger boost the following year when the *APSR* published March and Olsen's "The New Institutionalism." This much-cited work followed *Ambiguity and Choice* in "deemphasiz[ing] metaphors of choice and allocative outcomes in favor of other logics of action and the centrality of meaning and symbolic action" (1984, 738). Furthermore, among the article's six "institutional conceptions of order," three are clearly linked to the 1976 book or the 1972 paper: temporal order (pure GCT), normative order,[23] and symbolic order. And when they suggest three "examples of possible theoretical research" (1984, 744–6), it is no accident that one of these is the garbage can model.

The 1984 article, then, brought substantial attention within the discipline to their own variant of institutional thinking, which they presented as an integral part of the new institutionalism. What solidified its status, however, was the more elaborate presentation of these ideas in *Rediscovering Institutions* (March and Olsen 1989). It was quickly hailed as a major contribution and (as noted above) has come to be regarded as a "contemporary classic" (Goodin and Klingemann 1996, 16).

Obviously, we cannot analyze this complex book in detail. But its anchoring in March and Olsen's earlier work is evident throughout, and we will simply offer a few (of many possible) illustrations of its intellectual lineage.

1. Chapter three, "Interpretation and the Institutionalization of Meaning," begins with a section on how people make sense of their world. This was a central concern of *Ambiguity and Choice*. But the similarity goes far beyond common themes: Much of this section in the new book (pp. 41–5) is taken word-for-word from the earlier book.

2. One of the most-noted themes in *Rediscovering Institutions* is the contrast between "the logic of appropriateness associated with obligatory action and the logic of consequentiality associated with anticipatory choice" (1989, 23). This same theme was developed in *Ambiguity and Choice* as well, in its analysis of "attention as obligation" (pp. 48–50).

3. Most of the chapter on institutional reform is based on March and Olsen's (1983) *APSR* article, which in turn essentially restates the argument developed years earlier in Olsen's chapter, "Reorganizations as Garbage Cans," in *Ambiguity and Choice*.

4. The references section of the 1989 book gives individual citations to all but one of the 17 chapters from the 1976 book, and *Ambiguity and Choice* is far and away the most cited item of their references.

In short, the GCT of the 1972 article led to the interpretivist vision of the 1976 book, which in turn led to the new institutionalism of the 1984 article and the 1989 volume. There has been growth and elaboration (if not progress) throughout this evolution, but the continuity in themes and substance is striking. It is clearly a single research program, which carries the genes of the garbage can (Sjoblom 1993).

This research program does have intriguing ideas and assertions, but the problems remain and are much the same. The formulations are overly complex, the arguments unclear. Consider one of the best-known themes of *Rediscovering Institutions*, the importance of the "logic of appropriateness" in politics.[24] The core hypothesis is that "most behavior follows such a logic of appropriateness, that rules are followed and roles are fulfilled" (p. 161). This may seem to be a straightforward and empirically falsifiable claim about politics, but in fact it is neither. Whatever the logic of appropriateness refers to, it clearly involves rule-governed behavior. To know what it implies and how it might be tested, then, we must know what rules are.[25] Early in the book, the authors deal with this explicitly. What they do, however, is take a concept with clear meaning in ordinary language and transform it into a conceptual morass: "By rules we mean the routines, procedures, conventions, roles, strategies, organizational forms, and technologies around which political activity is constructed. We also mean the beliefs, paradigms, codes, cultures, and knowledge that surround, elaborate, and contradict those roles and routines" (p. 22).

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[22] See especially section four, "Reorganization as Garbage Cans" (1983, 285–7), and the concluding observations (p. 292).

[23] For example, the discussion in the 1984 article on "normatively appropriate behavior" (p. 744) is completely consistent with March and Olsen's earlier analysis of how attention is organized (1976b, 49).

[24] This idea is also central to March and Olsen 1996. See also March and Olsen 1995, chapter 2.

[25] The first definition of "rule" in *Webster's Collegiate Dictionary* (9th ed., 1030) is simply "a prescribed guide for conduct or action."

This staggering expansion of the concept trivializes the claim that political behavior is rule governed. Since their definition leaves so little out, what else *could* drive institutions? This expanded definition also makes it impossible to figure out what the logic of appropriateness refers to in concrete terms, and thus what kinds of behaviors are consistent or inconsistent with it. There is no clear content here to test.

In addition, *Rediscovering Institutions* never makes clear how rule-driven behavior differs from instrumental behavior. The predominance of the former, and the relative insignificance of the latter, is one of their central theoretical claims; yet, at times they seem to recognize that rule-following can be instrumentally based, and that the two are not at all distinct.

> To say that behavior is governed by rules is not to say that it is either trivial or unreasoned. Rule-bound behavior is, or can be, carefully considered. Rules can reflect subtle lessons of cumulative experience, and the process by which appropriate rules are determined is a process involving high levels of human intelligence, discourse, and deliberation. Intelligent, thoughtful political behavior, like other behavior, can be described in terms of duties, obligations, roles, and rules (p. 22).

Any notion that there is a clear distinction between rule-following and instrumentalism, moreover, is contradicted by cognitive science. The logic of appropriateness involves the matching of actions to situations (p. 23)—as in, "if you are in situation *x*, then do *y*"—but cognitive scientists argue that this is a broad rubric, and very often involves rules that are cognitive guides for instrumental action, encoding or summarizing the problem-solving effectiveness of skilled (although boundedly rational) agents (Anderson 1995). In a larger social setting, some of these instrumentally based rules may emerge as social norms, such as standards of professionalism.[26]

The confusion surrounding the logic of appropriateness could have been dispelled had the authors anchored their variant of the new institutionalism in a coherent micro theory. Ironically, one was right at hand: March and Simon (1958, 139–50) analyzed how cognitively limited decision makers would be guided by rules and routines, or what they called performance programs. There is now a substantial literature, some of it quite formally sophisticated, that seeks to explain organizational behavior on these individualistic foundations. Using the same foundations would have encouraged March and Olsen to examine individual choice in a clear, rigorous way and would have given them a framework of demonstrated analytic power that

anchors organizations in the choices of individuals. It also would have allowed them to generate detailed—and testable—implications, thus encouraging the kind of empirical research and theoretical adjustment to evidence that characterizes all scientifically progressive fields.

Why did the authors not ground their version of the new institutionalism on the Simon-March research program? Their framework is crying out for a micro-foundation that the latter could easily provide—yet, they do not use it. Given their intellectual sympathies for the Simon-March tradition, this is puzzling. The answer may turn on a self-inflicted wound: their stance against reductionism (March and Olsen 1984, 735–6, 738; 1989, 4–5). They seem to view this position as a key feature of their new institutionalism. If one accepts it as a meta-premise (don't build theories of institutional behavior on individualistic premises), then the option of building their new institutionalism on the bounded rationality program is simply precluded.[27] Yet, this merely pushes the puzzle back another step: Why is antireductionism a central tenet of their approach? Why did they constrain their theory construction in this manner? We do not know. But if the reasons for their methodological choices are not apparent, the effects are. Their arguments tend to be unclear and much too complicated, due in large measure to the absence of a disciplined micro foundation.

Chaos, Order, and Causality

Work on GCT has been much cited in organization theory, political science, and sociology. Yet, apart from work done by the authors themselves and some of their colleagues, the garbage can and its descendants have been put to little empirical use in these fields. For a theory often considered one of the major perspectives on organizations (e.g., Perrow 1986; Scott 1992), this neglect might seem odd, but we do not find it so. As it stands the theory is almost impossible to test, and this can only discourage serious empirical research.

Many applications of the approach do little more than describe some parts of an organization as garbage cans,[28] offering descriptions—usually detailed ethnographic accounts—that emphasize GCT's central themes. These case studies are, as Anderson and Fischer (1986, 141) note, often "only loosely coupled to the theory" (for a similar criticism see Perrow 1977, 297). They describe events using garbage can terminology, but it is not at all clear that they provide real GCT *explanations*. Authors of such applications (e.g., Sproull, Weiner, and Wolf 1978) tend not to see it this way; they seem to believe that, by merely labeling and describing organizations along these lines, they have

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[26] To be sure, some institutions (e.g., religious ones) have rules that encode sacred rituals. These differ in many ways from the problem-solving operators analyzed by Anderson. In particular, sacred rituals need not have a "logic of consequentiality," so *their* logic of appropriateness may be independent of secular notions of instrumentality. But *Rediscovering's* (1989, 160) argument that "politics is organized by a logic of appropriateness" is unqualified by any distinction between the sacred and the profane. Unquestionably, the claim is intended to cover ordinary behavior in ordinary governmental institutions. In this domain, the fatal ambiguity identified above—that rule-governed behavior may be infused by the consequentialism of problem-solving operators—is quite evident.

[27] In one section (March and Olsen 1989, 40–5), the authors did construct an argument with individualistic foundations. But this was an exception to the book's central tendency, and they made no attempt to introduce or summarize their overall approach via an explicit micro theory.

[28] A major exception is Kingdon (1984), whose work is distinguished by a careful empiricism tied to theoretical concerns.

somehow helped us understand them—that description has produced explanation. But it has not.

Latent here is a methodological issue of great importance: What counts as an explanation, and what kind of explanation do Cohen, March, and Olsen aim to provide? Clearly, description is not explanation; to equate them is a mistake. But what is the answer to the larger methodological question? If GCT and its offshoots did their job properly, what kind of explanations would they provide? How would they help us understand organizations?

Addressing this issue is crucial if the authors are to put their work in scientific perspective. And in fact they do address it, although in less depth than it deserves. They argue that their type of explanation is methodologically very different from those of conventional theories: It rejects the "consequential order" of conventional theories, substituting a "temporal order" in which "problems, solutions, and participants are joined together more by the timing of their arrivals than by other attributes" (March and Olsen 1986, 12).

Here, the authors are emphasizing that the theory is not only about chaos and disorder, the feature that has caught the attention of most social scientists, but also about order. Through the lenses of conventional theories, organized anarchies look chaotic, but they have an underlying order that the theory can help us see. This is good to point out, especially given how others have interpreted their work. But they still need to tell us how temporal and consequential orders differ, and in what sense the two give different types of explanations. So far, these issues remain unclarified.

We are reasonably sure what the authors mean by a "temporal ordering": It links events—"orders" them—based on the timing of the various streams' intersections. We are less sure what "consequential ordering" means. It seems to be a perspective that explains organizational outcomes via the intentions of individuals. It is "consequential," presumably, because actors intentionally make choices—and thus generate organizational outcomes—by assessing consequences.

From this vantage point the authors' methodological claim seems straightforward: GCT explains organizational outcomes on the basis of timing rather than intentions. Yet, if this is all there is to the argument, no new language is needed; and without the new language there would be no confusion. If we take the new concepts and the accompanying (brief) discussions seriously, however, there is much to be confused about and much to pay serious attention to—for it appears that what the authors are really doing is distancing the garbage can framework from causal explanations in general.

This is reflected in a second, more general way of thinking about "consequential order" that sometimes seems to orient the authors' analysis. In this interpretation any causal sequence involves a consequential logic, that is, certain outcomes are effects of certain determinants. X causes Y, hence Y is a consequence of X. Intentionality rests on such a logic, but this does not distinguish it from other theories. All causal theories use a consequential logic, in this sense.

Had the authors meant only intentionality when using the term "consequential order," as in the first interpretation, then presumably they would have said so explicitly. But they did not. We suspect this is because they were referring to more than intentionality—directing their criticism, at least implicitly, to causal approaches in general.

This inference is reinforced by the way the authors discuss temporal ordering. The essence of a temporal ordering, in their portrayal, is not just that timing determines outcomes; it is that events are not driven by an identifiable causal structure. Never, in fact, do Cohen, March, and Olsen present such a causal structure as the foundation of their theory, and they rarely say that identifying and modeling it is the key to explanation, as it would be in conventional theories.[29] In their view, the flow and intersections of events are largely driven by random occurrences, accidents, and a huge variety of complex institutional, social, psychological, economic, and political forces that cannot be represented by a causal model. The upshot is a perspective in which outcomes are explained not by a well-specified causal structure but by the timing of events and their random, unspecified determinants. As a statistician might put it, all the action is in the error term.

Because the authors' methodological discussions are so brief, we cannot be sure that we have correctly described their position. Yet, in a recent book March seems to confirm our judgment, for he explicitly states that his framework entails "the orchestration of decisions through temporal orders *rather than causal orders*" (1994, 180, emphasis added). In any event, if GCT and its extensions are ever to be legitimate scientific theories, they must set out a causal logic of some kind. That timing is central to their explanations has no bearing on the causality issue. A proper dynamic theory differs from other theories only in having a time component; all theories worthy of the name, including dynamic ones and those allowing for randomness, identify some causal structure as the basis for explanation. To suggest that their temporal order is relieved of this responsibility, that it can explain events without delineating causation, creates a fundamental confusion.

Finally, this methodological confusion ties into the garbage can's empirical applications in an ironic and unproductive way. It is hard to read this literature without encountering a convoluted line of reasoning that goes roughly as follows. What we do not understand about certain organizations—because their behaviors appear horribly complex, seemingly random, and impossible to disentangle—is actually just what the garbage can theory tells us to expect. In this sense we do understand it. Although we have almost no idea why certain events occur, we "understand" them because they are precisely the kind of incomprehensible phenomena that garbage can processes produce. That organizations are so confusing, then, *is actually strong evidence of the theory's great explan-*

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[29] For an exception see Cohen, March, and Olsen 1976, 36.

atory power. The less we can figure anything out—the less, that is, we can identify a causal structure—the more the theory seems to be working, and the better a theory it seems to be.[30]

This reasoning may seem far-fetched, but anyone familiar with the literature knows it is quite common. The solution is simple: GCT, if it is to become a genuine theory, must play by the same rules as all other theories in social science. The job is to identify the causal structure of institutional behavior. That is how behavior is explained and understood.

REVITALIZING THE GCT

Despite the many problems identified above, we believe that the garbage can program can be revitalized. Because we cannot carry out a full reconstruction here, we merely provide an example to show that basic themes of the GCT can be grounded in theories of bounded rationality. Our example will show how the well-known property of temporal ordering—that solutions may be linked to problems more by chance than design—can be derived as a natural implication of a classical (Simon 1957) model of adaptive search. This is a significant test case because temporal ordering looms large in the GCT literature.[31]

We begin with Simon's satisficing model and assume there is an organizational superior who decides whether to accept an alternative to solve a particular problem. She has three subordinates, trained in different professions. (Imagine a diplomat, a military officer, and an economist working for the secretary of state.) Due to the "trained incapacity" of specialists, the diplomat sees the problem as one of diplomacy, the officer sees a military problem, and the economist sees an economic one. They craft their solutions accordingly. The quality of proposals and how long it takes to generate them are random variables. Once a proposal is done it goes to the boss. She has an aspiration level that acts as a stopping rule: If she gets exactly one proposal that exceeds her aspiration, she selects that one, and the process ends. If no proposal satisfices, her staffers keep working. If more than one does, she picks the one she thinks is best.[32] Thus, the superior is picking samples from three different distributions, produced by the three specialists.

The value of the superior's aspiration level powerfully influences this process, as the following (easily

proven) implications show. (1) If the superior's aspiration level is very demanding, then the randomness of proposal generation will have little effect on which type of solution is accepted (although it will affect how long it takes to find a satisfactory option). If her aspiration level is so high that only one type of proposal can satisfy it, then random generation has no qualitative effect on the outcome. (2) *The lower the superior's aspiration level, the more the process exhibits temporal coupling*, that is, the more the proposals' chance arrival times influence what type of solution is picked. If her aspiration is below the worst possible alternative, then choice is driven completely by chance.

This shows how a central GCT property can easily be generated by a classical model of behavioral search. But it also suggests how the prediction of temporal coupling may be empirically limited. The preceding sketch fixed aspirations exogenously, as they were in Simon's model. But many have argued that aspirations adjust to experience, rising in good times and falling in bad (e.g., Cyert and March 1963). If aspirations were thus endogenized, then temporal coupling would tend to diminish over time. The superior, discovering that her payoffs beat her initial standard, would tighten her requirements, which would make her less prone to accept poor solutions.

Temporal coupling has been held up by garbage can theorists as a finding of almost mystical significance. But it turns out to be very compatible with choice-theoretic formulations. So, we believe, are all the other GCT ideas that are worth keeping.

CONCLUSION

The philosopher Max Black (1962, 242) once remarked that science moves from metaphor to model. In part he meant that science *should* so move. As he went on to say, metaphors are invaluable at the start of an inquiry. Ideas often come to us first as metaphors, a vague notion that x is like y. These can be creative insights; they may revolutionize a field. But they should not remain as they are born, for as scientific formulations, metaphors are flawed (Landau 1972). Their logic is obscure. How do claims relate to each other? Which are premises and which conclusions? And the empirical content of metaphors is often thin: Few, if any, of their claims may be empirically testable. Thus, both to clarify their reasoning and to provide targets for empirical scrutiny, scientists should—and typically do—transform metaphors into models.

The garbage can research program has done just the opposite: It has moved from model to metaphor. A model was presented first, in the same pathbreaking article that presented the metaphorically inspired informal theory. Later, instead of critiquing and revising the original formal model—which certainly needed attention—the research program focused mainly on embellishing the verbal theory. And it did so in ways that not only failed to clarify GCT but also actually made it more metaphorical.

Because the initial computer model misrepresented the informal theory, perhaps tinkering with it would

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[30] For example, in his well-known text on organization theory, Karl Weick applies the idea of organized anarchies to universities and states: "If that's partially what a university organization is like, then a thick description of that organization will be confusing when it starts to comment about goals..., technology..., or participants.... The irony is that *this confusion in the observer's report testifies to its authenticity and not to its sloppiness. Confusion as an indicator of validity is a critical nuance*" (1979, 11, emphasis added).

[31] "The central idea of the garbage can models is the substitution of a temporal order for a consequential order" (March and Olsen 1986, 17). See also Cyert and March 1992, 235; March 1994, 198–200; and March and Olsen 1989, 12.

[32] Evaluation error can easily be introduced: The superior could sometimes select an option that is below her aspiration (e.g., Bendor 1995).

have been a mistake. It probably should have been replaced by a new model. This did not happen. Instead, the original simulation was frozen in time.

The fault should not be laid only at the feet of the authors. Their 1972 article included a copy of the FORTRAN program, and everyone had a chance to critique it. The article's text described the simulation well enough so that the discrepancy between the informal theory and formal model could be discerned. So the authors discharged their obligations by giving readers plenty to work with. By and large, readers passed up this opportunity. Thus, the verbal theory and the simulation remained so deeply conflated that few could tell where one left off and the other began. That is no way to create a research program with ever clearer logic and growing empirical content.

We believe it is possible to revitalize the GCT. Doing so requires radical surgery. Because this operation would make the framework's logic transparent to all, it would deprive the GCT and the March-Olsen variant of the new institutionalism of a certain mystique. Without this bold move, however, there is little chance that these ideas will shed much enduring light on institutions.

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Garbage Cans, New Institutionalism, and the Study of Politics

JOHAN P. OLSEN University of Oslo

*B*endor, Moe, and Shotts want to rescue some of the ideas of the garbage can model and the new institutionalism. Their rescue program, however, is alien to the spirit of not only our work but also some recent developments that may promise a climate of dialogue between different approaches in political science. Bendor, Moe, and Shotts place themselves closer to a tradition of unproductive tribal warfare than to more recent attempts to explore the limits of and the alternatives to (means-end) rational interpretations of political actors, institutions, and change. By building on a narrow concept of what is valuable political science, they cut themselves off from key issues that have occupied political scientists for centuries.

It is, of course, flattering that Bendor, Moe, and Shotts (2001) have found it worthwhile to spend time and energy to critique the garbage can and the new institutionalism. The exercise is even more appealing because they argue that some of the ideas are worth developing and, with a little help, might create "scientific progress." The result, however, is disappointing. As storytellers, they misrepresent what we have aspired to do and what we have done. As innovators, their substantive contribution is modest. Most important, they point the field in the wrong direction. They suggest an approach that assumes away most of the complexity of political actors, the organized settings within which they operate, and institutional change, rather than make a serious effort to understand that complexity. Therefore, the last part of this response suggests an alternative perspective to the one they present.

BENDOR, MOE, AND SHOTTS AS STORYTELLERS

Bendor, Moe, and Shotts construe the garbage can and the new institutionalism in a way that makes dialogue difficult. Available space limits this response to three short comments.

Ambitions and Labels

Bendor, Moe, and Shotts (2001) misread the theoretical ambitions and the spirit of our work. They use the phrase "the garbage can theory of organizations (GCT)," which ignores our title: "A garbage can model"—"a" not "the," and "model" not "theory." Even after observing that we call the garbage can a meta-

phor, they conclude: "Yet, the GCT's basic ideas are obviously intended to be a theory and should be treated as such" (p. 171). Furthermore, they claim that the more recent work of March and Olsen "moves toward a larger theory" (p. 171).

In contrast, our claims are more modest. We have tried to comprehend some empirical observations of actual organizational and institutional behavior. We have tried to specify why the observations are surprises by showing how they are incongruent with dominant theoretical ideas. We have explored the implications of a limited set of ideas intended to provide better interpretations of the phenomena observed.

For instance, the goal of the original article (Cohen, March, and Olsen 1972) was to elaborate and modify existing theoretical ideas about organizational decision making to make sense of some empirical observations. We presented one way of looking at organizations—ideas assumed to be useful for some purposes and to capture some organizations, activities, and situations more than others.

In the 1986 return to these ideas and the reactions they had provoked, the spirit was the same:

> Since the complexity of decision making in an organization is unlikely to be captured by a single model, any more than by reports of a single participant or historian, the role of garbage can ideas is limited. They seek to identify and comprehend some features of decision making that are not well treated in other contemporary perspectives and yet are important. Thus, they are efforts to extend, rather than replace, understandings gained from other perspectives (March and Olsen 1986, 12).[1]

The garbage can ideas have been specified at several different levels of precision. In its "purest" form, the model assumes that problems, solutions, decision makers, and choice opportunities are independent, exogenous streams flowing through a system (Cohen, March, and Olsen 1972). Yet, a number of garbage can models exist, and these variations modify most of the key assumptions of the "pure" model, exploring the circumstances under which different results are produced (March and Olsen 1986). For instance, the four streams have been assumed to be more or less inde-

[1] The spirit has been the same throughout the last few decades (Cohen and March 1986, xi, xix; March and Olsen 1995, 3, and 1998, 943).

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pendent, tightly or loosely coupled. Structures have assumed various forms. The possibilities for intelligent action and management in garbage can situations also have been explored (Cohen and March 1974, 205–15). Surprisingly, Bendor, Moe, and Shotts (note 17, p. 183) see such attempts at exploration, many of them discussed in March and Olsen (1976) and March and Weissinger-Baylon (1986),[2] as indicating that components of the garbage can have been abandoned. On the contrary, the spirit has always been to encourage colleagues to play with the basic ideas, rather than defend them endlessly.

Bendor, Moe, and Shotts suggest that we pose as part of the bounded rationality tradition, although "a clear, cold look at the theory" shows that we are mistaken (p. 174). The framework of bounded rationality, they argue, has been abandoned (pp. 174, 184). The opposite conclusion is reached by the founders of the bounded rationality program (Cyert and March 1992; March 1996; March and Simon 1993) as well as by recent reviewers of the tradition (Goodin 1999; Jones 1999). Moreover, Bendor, Moe, and Shotts fail to suggest any important substantive consequences of labeling us as belonging, or not belonging, to the bounded rationality tradition.

The Simulation

Bendor, Moe, and Shotts are consistently decisive in their judgments about the role of the computer model that appeared in the original garbage can article, but they have some difficulty being decisive in a consistent way. Initially, they observe that "the verbal theory is fundamental. The computer model is derivative: It is only one of many possible ways to formalize key features of the theory and draw out its implications" (p. 170). Subsequently they proclaim: "The computer simulation formalizes the verbal theory and is widely regarded as the research program's scientific core" (p. 174). Finally, they decide that the function of the computer model is cosmetic, to bestow scientific legitimacy on the entire GCT enterprise (p. 184). It is hard to have it all three ways, and Bendor, Moe, and Shotts have a problem: If the simulation is derivative, then comments on it are not necessarily germane to the broader issues that they want to discuss. But if the simulation is central, then they have to explain why most of the scholars who find garbage can ideas informative have paid no attention to it. Since this is a dilemma they cannot resolve, they ignore it. By almost any standard of credibility, they exaggerate the significance of the simulation model. In comparison, March (1994, 201) assessed it as "one illustrative set of simulations."

Although the larger literature critiqued by Bendor, Moe, and Shotts does not rest on the simulation, several points in their discussion of it seem misguided. Although the simulation is certainly not the one we

would build today, with thirty years of hindsight and technical development to draw upon, we remain happy with the choices we made. Most of the changes we might make are not the ones discussed by Bendor, Moe, and Shotts. In particular, they focus on the absence of explicitly modeled solutions. This simplification is something we ourselves pointed out originally (Cohen, March, and Olsen 1972, 3), so we are surprised that they are surprised. It is true that the variegated content of solutions is not modeled in the simulation, but dealing with solutions implicitly rather than explicitly allowed us to have a much simpler structure to analyze and present. This would be an important advantage even now, and it was very important earlier, when few organization theorists had substantial intuition about computational models.

The interpretation of Bendor, Moe, and Shotts completely misses the close connections between "energy" in the simulation and "attention" in the bounded rationality tradition. Overlooking this aspect of a key variable lets them assert that the simulation is disconnected from the verbal positions of the article and from the bounded rationality tradition. But the connection has been clear to other readers. In addition, the main concept linking the simulation to events in universities is "slack," perhaps the most widely employed conceptual innovation of "A Behavioral Theory of the Firm" (Cyert and March [1963] 1992).

Bendor, Moe, and Shotts portray the clumpy movements of actors and problems from choice to choice as the most disturbing evidence that the garbage can simulation and verbal statement are not aligned. Somehow they have the expectation that these movements should be more of a uniform random buzzing (see their remark on chaos, p. 176). But that expectation is not in the verbal part of our article. Saying that organizational processes are not always ordered as conventionally assumed did not mean to us that the processes should exhibit no order. The sense of "disjunction" detected by Bendor, Moe, and Shotts arises from their misconstruing the introductory discussion. What they see as a major "bug" we saw—and still see—as a "feature." Namely, the simulation reproduces an experience that many of us recognize from our own lives: moving through a series of meetings on nominally disparate topics, reaching few decisions, while talking repeatedly with many of the same people about the same problems.

The complete lack of understanding of the spirit in which the garbage can model was offered makes it difficult for Bendor, Moe, and Shotts to avoid muddling the issues. They tell us (p. 172) that the dictionary defines "solution" in terms of problems. So for them the fundamental premise of the original article—that it is instructive to imagine problems and solutions as independent—is literally unintelligible. The account of our work that follows resembles the grumbling of humorless people who accidentally wander into the lively part of town. We are glad that most readers have had more imagination and have found it stimulating to consider a world in which a solution can usefully be

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[2] See also Anderson and Fischer 1986; Carley 1986; Crecine 1986; March and Romelaer 1976; Weiner 1976; Weissinger-Baylon 1986; and even March 1994.

conceived as "an answer actively looking for a question.",

The Garbage Can and the New Institutionalism

Bendor, Moe, and Shotts expand their critique of the GC, and particularly by the simulation, into a general critique of the work of March and Olsen. It is not clear, however, when they are writing about ideas and when they are writing about authors. On the one hand, they argue that it is important not to confuse GCT with its authors (p. 174). On the other hand, they generalize from the GC to the "research program" and the "larger enterprise" of March and Olsen. They argue that "all the more recent work [of March and Olsen] is rooted in the GCT and has inherited, as a result, its fundamental problems" (p. 184). The suggestion is truly extraordinary on the face of it and is neither demonstrated nor even argued with anything more than an assertion.

Insofar as Bendor, Moe, and Shotts attempt to say something about the whole authorship, they fail. Cohen's later work is ignored, in spite of its obvious relevance to the GC and the use of simulation techniques (Axtell, Epstein, and Cohen 1996; Cohen 1986). March's work on risk taking, learning, and adaptation is not mentioned (Cohen and Sproull 1996; March 1999; March and Shapira 1987). Olsen's work on institutional reform is overlooked (Brunsson and Olsen 1993; Olsen 1997; Olsen and Peters 1996).

In fact, garbage can ideas are a minor part of the recent work by March and Olsen (1989, 1995, 1998). Yet, in order to identify the new institutionalism (NI) with the garbage can (GC), Bendor, Moe, and Shotts ignore changes in subject matter. The focus has been extended from decision making in formal organizations, to an interest in political institutions and democratic governance, to how and when international political orders are created, maintained, changed, and abandoned.

Moreover, the core theoretical ideas of the GC and NI are more loosely coupled than suggested by Bendor, Moe, and Shotts. To a considerable degree, those ideas explore organizations and institutions from different perspectives. The GC views organizational life as highly contextual, driven primarily by timing and coincidence. The pure garbage can model is basically institution free, or structure is treated as exogenous. Decisions are produced to a large extent by the temporal linkages of problems, solutions, choice opportunities, and decision makers.

In contrast, NI represents an attempt to supplement ideas of consequential action, exogenous preferences, garbage cans, and efficient histories with ideas of rule- and identity-based action, institutional robustness, and inefficient histories (March and Olsen 1998, 969). A key question is where structures originate and how they are maintained and transformed, including the relative importance of deliberate reform and design. NI assumes that institutional structures impose elements of order on a potentially inchoate world and that institutions have a certain robustness against changes in

external environments as well as deliberate reforms (March and Olsen 1984, 743; 1989). History does not follow a course that leads inexorably and relatively quickly to a unique equilibrium dictated by exogeneously determined interests and resources. Instead, history is inefficient and follows a meandering path affected by multiple equilibria and endogenous transformations of interests and resources (March and Olsen 1998, 954).[3]

NI also represents a shift in focus from the logic of consequences and rational calculation of expected utility and prior preferences to alternative forms of intelligence and behavioral logics. In particular, it explores a logic of appropriateness based on a sense of identity (March and Olsen 1989, 23; March 1994). Like the logic of consequences, the logic of appropriateness is explicitly a logic of action or justification for an individual actor (March and Olsen 1998, 952). Actors behave in accordance with their interpretation of rules and practices that are socially constructed, publicly known, anticipated, and accepted. A polity (and society) is a community of rule followers with distinctive sociocultural ties, cultural connections, intersubjective understandings based on shared codes of meaning and ways of reasoning, and senses of belonging. Identities and rules are constitutive as well as regulative and are molded by social interaction and experience.

Strangely enough, Bendor, Moe, and Shotts do not explicitly confront such identity-derived, rule-driven behavior with rational (strategic) behavior based on a calculation of expected utility. Rather, their discussion of rule-driven behavior confuses the cognitive processes through which actors go when making decisions with the origins of rules and their effects (p. 186).

BENDOR, MOE, AND SHOTTS AS INNOVATORS

Although Bendor, Moe, and Shotts consider the GC confused, they think there are "intriguing ideas and assertions" worth saving (p. 185). It is possible to rescue not only the GC but also NI from the morass into which they have been led. All this will take radical surgery and a collective effort of reconstruction (pp. 170, 188, 189). With remarkable assurance, Bendor, Moe, and Shotts offer assistance in the form of an alternative model. It is a generous offer, but it is difficult to discover a basis for their hubris.

The alternative model would focus on the "temporal ordering test," but it is based on assumptions about stable participation and stable authority (principal-agent) relations (p. 188), so it is hardly in the spirit of the garbage can ideas. Bendor, Moe, and Shotts assume away, rather than incorporate, key observations from behavioral studies of organizational decision making. These show that participation is not always

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[3] Brennan and Buchanan (1985, 149) also criticize the Invisible Hand assumption in economic theory: "Great damage has been and is being done by modern economists who argue, indirectly, that basic institutional change will somehow spontaneously evolve in the direction of structural efficacy."

stable, that there is unresolved conflict, and that authority relations are ambiguous or shifting, not organized into stable hierarchies. Assuming away complexity is especially problematic because many modern polities seem to have a multicentered, multilevel, network character rather than a single center with lawful hierarchical authority (Kohler-Koch and Eising 1999). A possible implication is that the garbage can ideas, originally assumed to capture specific aspects of university governance and thus a small part of the world, now may capture a much larger and more important part of political life.

Quite aside from the alternative model, the basic approach of Bendor, Moe, and Shotts to revitalizing the garbage can research program is misguided. According to them, Cohen, March, and Olsen can best be "salvaged" (p. 169) by conventional economic theories of rational choice. They want the heretics to return to the true faith (March 1992). The suggestion is hardly surprising, given their previous work, and is not totally without precedent, but such a rescue would be a retrogression, more likely to impoverish the line of work than to produce a leap forward.

POSSIBLE FUTURE DIRECTIONS

Bendor, Moe, and Shotts castigate the discipline before they understand it. They do not understand why so few colleagues share their own low opinion of the GC and NI. They reprimand their colleagues for not having "pounced on the model from the outset, exposed its inadequacies, and prevented it from gaining such prominence and influence" (p. 183). Yet, the most important question is: Exactly how do Bendor, Moe, and Shotts intend to move the field forward, beyond the specific principal-agent model presented?

Styles of Research

For Bendor, Moe, and Shotts, the core of genuine social science theory and "scientific progress" lies in formal modeling of a particular kind. They tell us that "description is not theory" (p. 185) and "description is not explanation" (p. 187), forgetting the equally trite dictum that "rationalization is not explanation." This is not the place to recite the long list of useful models of social behavior that look quite different from the one sketched by Bendor, Moe, and Shotts, or to recollect the history of hopes dashed in the pursuit of models of that class. I share much of the enthusiasm of Bendor, Moe, and Shotts for such modeling, but it does not define scholarship. It is simply a part of it. There is no reason to downgrade the value of raising good research questions, of detailed ethnographic accounts of how political organizations and institutions really work, and of theoretical speculation based on surprising empirical observations. Such questions, observations, and speculations often advance political science, even when all the elements are not "organized into a coherent and powerful theory" (p. 183).

Surprisingly, Bendor, Moe, and Shotts attach little value to the process of developing and exploring the-oretical ideas, compared to the process of testing such ideas.[4] As will be argued below, ignoring the exploration of competing assumptions is likely to impoverish, not enrich, the understanding of politics.

Competing Assumptions

Students of the dynamics of political action and structure start out with different assumptions when it comes to three basic questions. (1) How do we understand the nature of human beings as political actors? (2) How do we understand the organized political settings within which modern political actors most typically operate: political organizations, institutions, and normative orders? (3) How do we understand political change and development, that is, how are political institutions, identities, and policies established, sustained, and transformed?

Bendor, Moe, and Shotts seem to start with the following assumptions.

- *Rational actors.* Political action is the making of rational choices on the basis of expectations about the consequences for prior objectives.
- *Instrumental organizations and institutions.* Organizational forms are instruments for making and implementing rational decisions in order to achieve prespecified ends. Organizations have authority structures, and "superiors can tell others what to do" (p. 173).
- *Political development as structural choice and limits on rationality as temporary.* Organizational forms are deliberately designed instruments, stemming directly from the desires of identifiable political actors. The instrumental use of structure by leaders "is pervasive and fundamental to an understanding of organizations." Leaders do not have to accept the GC's dynamics (p. 173). Temporal coupling will tend to diminish over time (p. 188).

This constitutes a quite conventional framework in interpretations of politics, and it has become more prominent with the elaboration of economic models of politics. Yet, students of political life, both historically and currently, have observed considerably more complexity in how political decisions are made and meaning is created; in how political institutions are organized—the principles upon which they are structured, how they work and are governed; and in the processes through which political development takes place.

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[4] Allow me a personal note. This attitude is particularly surprising because their Stanford colleague, Jim March, has been "a pioneer in raising good new questions for further research" (Thoenig 2000). Always wanting to make "artists out of pedants" (March 1970), March sometimes works in a style close to poetry. At other times he works in a formal and mathematical style. John Padgett (1992) calls him "the Miles Davis of organization theory," always willing to explore new ideas and never letting himself be caged into a single style of research or school of thought. Thoenig and Padgett emphasize the value of such activities in the development of the social sciences. Bendor, Moe, and Shotts downgrade their importance.

Actors

Close observers of political life have found great diversity in human motivation and modes of action, not a single dominant behavioral logic. They see the character of political actors as variable and flexible, not universal and constant. Actors may be driven by habit, emotion, coercion, and interpretation of internalized shared rules and principles, as well as calculated expected utility driven by incentive structures (Weber 1978). Most rational choice interpretations conceive an individual's preference function as exogenous, given, and essentially arbitrary, but other approaches emphasize the significance of the basic values of the culture (or subcultures) in which actors are born and live. Actors are socialized into culturally defined and institutionalized ends and purposes to be sought, as well as modes of appropriate or required procedures for pursuing the purposes (Merton 1938, 676). Legitimate institutions, principles, procedures, methods, rights, and obligations give order to social relations and restrict the possibilities of a one-sided pursuit of self-interest or drives (Weber 1978, 40–3).

Legitimacy and efficiency do not necessarily coincide. There are illegitimate but technically efficient means as well as legitimate but inefficient means (Merton 1938). Legitimacy can be established by showing that decisions accomplish appropriate objectives or are made in appropriate ways (March and Olsen 1986, 22; Meyer and Rowan 1977). Rationality can be substantive or procedural (Simon 1976, 1985). Organizational and professional identifications make a difference; they provide not only behavioral logics and standard operating procedures but also mental frames and individual and collective identities. Rational calculations (as well as other forms of intentional intelligence) do not reliably converge on uniquely sensible action (March 1994). Fundamental miscalculations and their (sometimes) unintended and undesired consequences are important aspects of, not footnotes to, social life.

Organized Settings

Students of political life have observed the great diversity of organized settings and types of collectivities and social relationships within which political actors operate (Finer 1997). In modern society special attention has been given to the properties of formally organized settings, that is, to how political orders, organized systems of governance, institutions, and organizations best can be understood as the context of political behavior. Understanding the polity as a configuration of institutions, norms, rules, and practices is a necessary supplement to the idea of political life organized around the interaction of a collection of autonomous individual actors who pursue prior preferences by calculating future outcomes (March and Olsen 1989, 1995).

Whereas Bendor, Moe, and Shotts assume a given authority structure of principals and agents, an old theme in political science has been to understand the conditions under which there is authority and political order at all. How can order develop out of anarchy, and through what kind of processes is authority achieved, maintained, and lost (March and Olsen 1998)? It has been observed that political orders are more or less institutionalized and that political organizations and institutions are structured to different degrees and in different ways. To understand the organizational mosaic of modern society, it is necessary to go beyond images of hierarchies and markets (Brunsson and Olsen 1998; Dahl and Lindblom 1953).

Likewise, it is necessary to go beyond understanding organizational effects in functional-instrumental terms. That is, institutions cannot be viewed solely as incentives and opportunity structures that regulate behavior by affecting calculations and transaction costs. Institutions constitute political actors. Institutional effects in terms of civic education and socialization are key processes in political life. Historically, forms of government have been assessed according to their ability to foster the virtue of intelligence of the community (Mill [1861] 1962, 30–5).

Change in Political Orders

As students of politics have observed, political life achieves and loses structure, and the nature of political order changes in a variety of ways. The basic units are constituted and reconstituted, and so are their relationships:

> At some periods in some areas, political life has been rather well organized around well-defined boundaries, common rules and practices, shared causal and normative understandings, and resources adequate for collective action. At other times and places, the system has been relatively anarchic. Relations have been less orderly; boundaries less well-defined; and institutions less common, less adequately supported, and less involved (March and Olsen 1998, 943–4).

To understand processes of change, scholars have not always assumed that leaders simply choose structures. Rather, it has been asked, what is the role of human intention, reflection, and choice in the development of political institutions and good government? Under what conditions, and through what mechanisms, can political actors rise above, and get beyond, existing institutional structures (Hamilton, Jay, and Madison [1787] 1964, 1; Mill [1861] 1962, 1; see also Brunsson and Olsen 1993; March and Olsen 1989, 1995, 1998; Olsen 1997)? Rational structural choice and adaptation, like competitive selection and other change processes, are less than perfect. They also interact in complicated ways (March 1981, 1994).

Studies of political and administrative reform have documented the need to go beyond processes of rational adaptation and competitive selection in order to understand change (Brunsson and Olsen 1998). "It is probably important to distinguish situations in which organizations may be susceptible to deliberate willful reorganization from situations in which the process of change more clearly resemble a garbage can process" (March and Olsen 1986, 25). Furthermore, weakly

institutionalized processes, such as comprehensive administrative reform, are more likely to have garbage can properties than are more institutionalized processes (March and Olsen 1983).

Imperialism versus Multimechanism

For observation-driven behavioral research, the first set of questions is: What are significant and interesting political phenomena? What is worthwhile knowing anything about? The next set of questions, then, is: How can we best understand such phenomena? How will different basic assumptions be of help?

To answer the latter set of questions, it is helpful to know a repertoire of possible ways of understanding political actors, institutions, and change. Then one may better explore the relative importance of—the scope, conditions, and domains of application for—a specific set of assumptions. For example, one can explore the understanding achieved by the conventional assumptions about rational decision making, instrumental institutions, institutional design, and deliberate reform or by the assumptions underlying the garbage can or NI perspectives. One also may identify factors that make different basic assumptions more or less salient to an understanding of different parts of political life. If no single set of assumptions is viewed as more fruitful than all the others under all conditions, and if different assumptions are not seen as necessarily mutually exclusive, then theoretically inclined scholars may also explore ideas that can reconcile and synthesize different sets of assumptions. In particular, one may explore possible interrelationships and transitions among logics of action, roles of institutions, and processes of change.

Bendor, Moe, and Shotts are representative of an imperialist intellectual tradition. That is, they embrace the notion that a single, simple theory of human action will suffice to interpret complex historical events and political phenomena. The strategy is as risky as it is pretentious. I prefer a more catholic approach. For example, what is the relationship between strategic and rule-driven action?[5] Political actors are constituted both by their interests, by which they evaluate their expected consequences, and by the rules embedded in their identities and political institutions. They try to calculate consequences and follow rules, and the relationship between the two is often subtle (March and Olsen 1998, 952). The richness of the alternatives mirrors the richness of the phenomena, and wishing that the phenomena were less complex will not make them so.

Similarly, understanding the role of political institutions in coercion, in managing exchange through incentives, in redistribution, in building a political culture, and in developing structures for the sustenance of civic virtue and democratic politics (March and Olsen 1995,

245) is not likely to be furthered by seeking a single, simple interpretation. Political change is similarly a study in historical and contemporary complexity. Understanding institutional abilities to adapt spontaneously to environmental changes, environmental effectiveness in eliminating suboptimal institutions, and the latitude of purposeful institutional reform requires attending to several processes of change.

The European Union as an Example

Are there significant political processes in which interactions and shifts among logics of action, institutional forms and functions, and processes of change can be observed? An obvious candidate is the European Union. As a political order, the EU is involved in a large-scale institutional experiment and a search for constitutive principles and institutional arrangements.

Principled action based on various identities mixes with detailed calculation of material self-interest. As a multicentered and multilevel polity, the EU combines a variety of institutional forms and degrees of institutionalization. As part of the transformation process, struggles over the European mind combine with struggles over institutional structures. No identifiable group of actors can unilaterally choose the future institutional arrangements of the EU. Yet, there are elements of choice in the coevolution of institutional structures across levels of governance and functional structures, and some actors are more powerful than others. A variety of "local" (territorial as well as functional) processes interact in complex ways, combining willful choice, chance, and compelling structures in a fashion that generates both complex processes and outcomes that are difficult to predict.

The current transformations remind students of politics of Tocqueville's ([1835] 1945, 7) comment upon observing the new American democracy: "A new science of politics is needed for a new world." Yet, the difficulty of understanding political phenomena such as the European transformation may have important implications for prospective theory builders. It may be necessary to recognize that the relationship among political action, institutions, and the flow of history involves a complicated interplay among several logics of action, institutional roles, and processes of change. It also may be necessary to accept that significant political phenomena sometimes are complex enough to make any simple theory of them unsatisfactory (March and Olsen 1986, 29).

CONCLUSION

The comments by Bendor, Moe, and Shotts are unlikely to improve our understanding of political organizations and institutions. They misrepresent the garbage can and the new institutionalism, and their unsuccessful example of how these ideas can be "rescued" is hardly promising. By building on a narrow concept of what is valuable political science, and by assuming away interesting challenges, they cut themselves off from some of the key issues that have

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[5] To some degree, different logics are located in different institutional spheres. Where they compete, a clear logic may dominate an unclear logic. One logic may be used for major decisions, the other for minor refinements of those decisions. Logics can be sequentially ordered, or either logic can be viewed as a special case of the other (March and Olsen 1998, 952–3).

occupied political scientists. Their own program is without substantive political content. They do not tell us which political phenomena they want to understand, and their separation of politics from its institutional and historical context makes it difficult to discuss which basic assumptions are most likely to be helpful—those they suggest or those of the garbage can or institutional perspectives. In sum, they indicate an unpromising route and point research in the wrong direction.

The Bendor, Moe, and Shotts rescue program is alien to the spirit of not only the garbage can model and the new institutionalism, but also some recent developments that may promise a climate of dialogue between different approaches, in contrast to unproductive tribal warfare (Bates et al. 2000; Cook and Levi 1990; Elster 2000; Green and Shapiro 1994; Katzenstein, Keohane, and Krasner 1999; Levi, Ostrom, and Alt 1999). It is a pity that the willingness to explore the limits of and the alternatives to (means-end) rational interpretations of political actors, institutions, and change; to accept that different approaches may contribute something of value to the study of politics; and to look for possible ways of integrating competing perspectives has not yet influenced Bendor, Moe, and Shotts.

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BOOK REVIEWS

Political Theory

Our Lives before the Law: Constructing a Feminist Jurisprudence. By Judith A. Baer. Princeton, NJ: Princeton University Press, 1999. 276p. $55.00 cloth, $18.95 paper.

Gayle Binion, *University of California, Santa Barbara*

It is a daunting assignment to review a book after it has garnered a major award bestowed by the organization that publishes this journal. Judith Baer's *Our Lives before the Law* not only is a very worthy recipient of the 2000 Victoria Schuck Award for the best book on women in politics but also is an erudite and wide-ranging critique of feminist thought with the goal of "forc[ing] feminists to confront mainstream discourse and mainstream discourse to confront feminism" (p. 175). This goal is in the service of Baer's desire to construct a new jurisprudence of sexual equality, one that avoids the pitfalls Baer perceives as inherent in the efforts of others to date.

The analysis ranges well beyond the boundaries of what is routinely conceived to be legal, indeed, well beyond the realm of the political, to draw upon all manner of feminist thinking. Of particular interest to Baer is the debate between those who focus on gender difference and those who focus on (male) gender dominance. Declaring that "this book will show that I am firmly on the side of the dominance jurists" (p. 11), Baer proceeds to interrogate the feminist theorizing that she believes has been unduly tied to the centrality of the body, indeed of genitalia, as sources not only of male/female difference but also of the relative disempowerment of women. She ponders why women are presumed to be psychologically, no less politically, structured around biology and the sexual and reproductive events in their lives. Robin West (e.g., "Jurisprudence and Gender," *University of Chicago Law Review* 55 [Winter 1988]: 1–72) and Nancy Chodorow (*The Reproduction of Mothering*, 1978) are here subjected to significant criticism for their lack of empirical evidence for the gender role analyses they offer and for their quasianalytic methods. Carol Gilligan (*In a Different Voice*, 1982) similarly receives an intellectual drubbing for her weak evidence of the different voice as well for having spawned voluminous works by others that valorize "care" as a feminine distinguishing attribute.

Baer defines all approaches to feminism focused on gender difference as "character" theories, which she rejects as irrelevant to the realities of women's lives; unequal power, not being different, is at the heart of the problem. As further evidence of what she sees as the foolishness of the difference enterprise, Baer makes the case that much of what is deemed "feminine" in legal analyses, such as emotionalism, is employed by the most retrograde of Supreme Court jurists. In sum, she expresses greater respect for the Catharine MacKinnon (e.g., *Feminism Unmodified*, 1987) situational school of thought, which views domination of men over women as both the source and consequence of law.

The book ends with a plea for reconstituting feminist jurisprudence in a variety of ways: give up the feminist monopoly on "care," challenge the excessive/unequal level of responsibility on women without the concomitant level of rights, address the liberal state's inability to connect "needs" with "rights" as compatible phenomena, and institute an imperative jurisprudence based on the expectation that society will meet human needs and ensure the survival of the species. From these premises expressed within the last few chapters, it is argued that one can imagine an equality of

rights and responsibilities between men and women. Baer does a fine job of building these principles into her critique of the law of reproduction, fetal protection, divorce, and corporate downsizing, at all points intertwining the conventional dictates of liberalism and indicating where within them feminism would demand modification.

In its analysis of difference and dominance as well as its deconstruction of the legal principles governing various areas of public policy, Baer's book makes a very valuable contribution to the literature on feminist jurisprudence and stimulates thinking about where the discourse has been, where it is, and where it should be going. But in some other ways the volume is less satisfying. Two points of concern or disagreement are noted below with reference to her critical assessment of the state of feminist jurisprudence and its identified weaknesses.

First, Baer criticizes feminist theory (including MacKinnon, with whom she generally agrees) for its attempts to develop grand theory that more often than not misses critical points. It is equally if not more plausible to perceive feminist jurisprudence as insufficiently grand, indeed, as partial, provisional, and inductive. Katharine Bartlett, in a pathbreaking attempt to outline the contours of feminist jurisprudence ("Feminist Legal Methods," *Harvard Law Review* 103 [February 1990]: 829–88), notes the self-consciousness of feminism toward issues of differences among women and the need to build theory from the realities of everyday life. This imperative has been at the heart of the work of a significant pool of feminist legal scholars, who insist on remaining deeply rooted in the empirical and eschew theory that is tied to abstract first principles. Indeed, Carole Smart's very compelling book, *Feminism and the Power of Law* (1989), concludes that it is law's demand for "grand theorizing" and its insistence on knowing where one's claim fits within predetermined theory that render the institution unreformably male and patriarchal. Thus, structurally, it may well be that the predominant modes of feminist legal thought cannot effectively compete with the abstract premises of liberalism, which dictate the primacy of such nonempirically based values as individual autonomy and institutional license.

Second, although Baer does an excellent job of demonstrating the problems she believes are associated intellectually and politically with difference theory, her critique may ignore the value of incremental steps toward sociopolitical change and may also focus wrongly on the difference model rather than on some of the uses to which it has been put. Martha Fineman, in an equally compelling analysis of feminist thought in the world of divorce and child custody (*The Illusion of Equality: The Rhetoric and Reality of Divorce Reform*, 1991), condemns "feminists" for insisting on gender neutrality in family law, which ignores the situational differences between men and women. As is to be expected in empirically rooted theorizing, in each case one must ask against which status quo in law is the criticized feminist activity, intellectual or litigative, occurring. In the case of marital law, gender neutrality arguably is a step forward in a system still bearing vestiges of unity of person and coverture. With respect to public policy more generally (re: Baer's work), the different voice proposition serves to address whether law is modeled on the life experiences of only men, and in no way is itself incompatible with dominance theory.

The problem for feminism is not that there is an interest in difference, it is how that difference would be understood and to what uses that stream of thought is put. I share much of Baer's concern that this enterprise is capable of undermining the quest for sexual equality, but the fact that she calls such theories character theories suggests that she sees the work as

essentialist. The best work in this area, however, very clearly rejects any notion of inbred difference and assumes that different life experiences (and certainly the range of life experience differs for women and men) have consequences for the way women view the world as well as the demands that they would make on the public policy system if that experience were empowered. Although Carol Gilligan cannot control how others understand her work, she has never claimed "natural" differences between the sexes. She has also never suggested that care be valorized as feminine, and she does not reject the idea that it be more equally redistributed as a responsibility.

Prometheus Wired: The Hope for Democracy in the Age of Network Technology. By Darin Barney. Chicago: University of Chicago Press, 2000. 340p. $29.00.

Ted Becker, *Auburn University*

This is a welcome and provocative addition to the growing literature on the politics of the Internet. In addition to its rich intellectual texture and mother-lode of information about computer hard- and software, it is a quick read because the author has a sharp tongue and makes excellent points. It is a unique blend of political philosophy, political economic theory, and computer network technology in support of a political F-5 tornado warning.

Prometheus Wired is aimed directly at the heart of naiveté about the Internet and computer networking that characterizes much teaching and writing in fields such as communications, futuristics, journalism, and information science as well as media and politics courses in our own discipline. Barney's explicit goal is to debunk those who have been preaching about the innately prodemocratic, nonhierarchical, chaotic infrastructure of the Internet. He contends that the new computer networks are producing greater alienation among workers and greater mastery over citizens.

One strength of the book is Barney's clever, cogent, coherent, and compelling critique of the emerging global economy and how it deploys the "control utility of network technology" to produce "a hegemonic economic order" (p. 187) or the "universal homogeneous state" (p. 262). The result of this new information and communication technology, or what the Europeans call ICT, has not been to free and empower ordinary people but to tighten the screws and make their global economic and political rulers richer and less visible than ever before.

Barney points to all the usual indicators of what others call the new world order: diminishing union strength; proliferating electronic surveillance of workers and citizens; longer hours on the job; the isolating properties of telework and telecommuting; the downward pressure on wages and decline of employee benefits while profits and the value of stocks soar; greater polarity in wealth; the massive intrusion into personal privacy; and so on. "Insofar as they bolster the already formidable control of capital over the means of power, computer networks are an essentially conservative, not revolutionary, technology—conservative, that is, of the prevailing liberal and capitalist order" (p. 188).

As for democracy, Barney calls the Internet its Trojan horse. What appears to be a Promethean-like gift is in reality a most insidious weapon of destruction. In his view, if citizens do not enjoy much power in capitalist and quasicapitalist societies, then how can they accrue more when the networking technologies are even more alienating, demeaning, and manipulative of the blue- and white-collar working class? Another of his arguments is that "network technologies can

never be an adequate substitute for the *techne* of government, and will never fully satisfy the appetite human beings have for governing themselves well" (p. 267). Indeed, "if computer networks are to be involved in democracy at all, they are likely to be instruments of democracy at its worst" (p. 267).

Barney's treatise is a highly readable polemic that deconstructs the present system and, as is the wont of this analytic ilk, leaves the reader feeling that the future of democratic politics is somewhere between grim and hopeless. One could well finish this book feeling that all is probably lost, largely because of the "control utility" of computer networking, the ultimate capitalist tool. The brave new cyber world not only is coming to town but also is inside your PC, cell phone, wallet, and psyche.

Does *Prometheus Wired* provide conclusive evidence that those who laud the democratic potential of the Internet have been deluded? Not really. Despite the persuasiveness of Barney's argument, a good case can still be made for another scenario, that the Internet will yet prove to be a key factor in strengthening democracy in the immediate, proximate, and/or distant future. As a good advocate, Barney is aware of some data such analysts would proffer. He describes several cyber democracy experiments, although sparingly, and then cavalierly dismisses them; for example: "The limited role network technology *might* play in community enhancement overrides the generally uprooting quality of its essence" (p. 218, emphasis in original).

What could proponents of a counterthesis offer to support their position? First, they would probably claim that the tremendous capacity of computer information and networking technology to help develop both geographic and "virtual" communities is only a small part of the picture. There is already a massive trove of experimentation and literature on how other new technologies and techniques have empowered citizens in innovative ways, such as scientific deliberative polling (the deliberative poll, citizens juries, consensus panels; authentic electronic town meetings; the growth in and cyber networking of citizen initiatives in America and national referenda throughout Europe and other parts of the world). Computers sometimes play a leading role in this ensemble but are rarely the superstar and may never be.

A second major line of argument concerns the history of democracy globally and in America. There is something to be said for the view that democracy is part of humanity's genome and that as human beings have evolved, as our knowledge has grown, so has democracy. There is abundant evidence that the process of democratic transformation is not linear but surges and ebbs and may be nowhere near high tide at this time.

Proponents of this position are equally capable of peering into the future and asking: What happens to all this global cyber control when the universal homogeneous state falters or implodes because of some American, European, or global political, economic, biological, environmental, and/or technological disaster(s)? Mass movements preceding democratic surges take a long time to gestate but thrive under catastrophic conditions. There is merit to the position that increasingly alarming evidence of extreme global warming raises reasonable doubt about the viability and invincibility of this global economy, with all its "control utility mechanisms."

Huxley, Orwell, and Barney have every right to fret over and forewarn about the variety of dictatorial futures humankind faces due to current and emerging electronic and drug technologies of control, manipulation, and repression. Yet, no one really knows our destiny. The counterposition to Barney's has equal credibility: When political, economic, and social crises reach a turning point, at least the American

solution has been to transform the democratic process, not deform it into tyranny. All the pioneering work by political scientists and democratic activists around the world to engage and empower citizens through new and improved methods of informed deliberation—electronic and face to face—may well come into play when the time is right. Posterity alone will be the judge.

Finally, Barney and I interpret the myth of Prometheus differently: I view his gift as a net plus for humanity's growth and self-actualization. True, Zeus fumed, and Prometheus was severely punished. But his gift of fire helped lure the human life form out of darkness so as to transform all other earthly elements, if not the universe itself, not as self-proclaimed gods but as seekers and doers. In their own way, current and future networking technologies may yet prove to be as enlightening and liberating as fire was to primitive humanity. To believe otherwise will only benefit those Barney fears the most.

Virtue and the Making of Modern Liberalism. By Peter Berkowitz. Princeton, NJ: Princeton University Press, 1999. 235p. $27.95.

Kenneth L. Grasso, *Southwest Texas State University*

"For quite a while," Peter Berkowitz notes, "leading academic liberals and their best-known critics formed an unwitting alliance, promulgating the view that liberal political theory" ignores the whole subject of virtue and cultivation (p. 170). If that view is correct, this neglect not only would spawn "fatal theoretical lacunae" (p. 4) but also would raise serious doubts about liberalism's capacity to sustain the "qualities of mind and character" (p. 172) required for "the operation and maintenance" of "free and democratic institutions" (p. 6).

In recent years, however, a new generation of liberals have challenged this widely held view. Thinkers such as William Galston and Stephen Macedo acknowledge that liberal regimes depend "upon a specific set of virtues," which "they do not automatically produce" (pp. 27–8). Their work points toward the "dependence" of liberal societies on "extraliberal and nongovernmental sources of virtue" (p. 28), such as "the family, religion and the array of associations in civil society" (p. 6). Simultaneously, they insist that "limited government is not the same as neutral government" (p. 173), and they affirm "that the liberal state, within bounds, ought to pursue liberal purposes" and, thus, "may, within limits, foster virtues" that serve these purposes (p. xii).

Although impressed by their work, Berkowitz believes that these thinkers have not "taken full advantage of the resources within the liberal tradition for illuminating the connections between virtue" and liberal politics. Likewise, they have not fully appreciated either "the disproportion between liberalism's need for virtue and the means" it "can muster to foster" it or "the vices that liberal principles can engender" (pp. 31–2).

It is these subjects that this volume seeks to address. Its "core" (p. xiii) is historical and consists of an examination of the thought of Hobbes, Locke, Kant, and Mill. Each, Berkowitz admits, adduced "strong practical and theoretical reasons for ... circumscribing virtue's role" in political life (p. 3). Nevertheless, a reading of their work that attends to "text and context" (p. 171) demonstrates that they may disagree on the specific qualities needed and how these are to be cultivated, but all ultimately recognize that liberal regimes cannot flourish without citizens and statesmen "capable of exercising a range of basic virtues" (p. 32). At the same time, each faces an array of "practical and theoretical obstacles" in

affording "virtue the breathing room it needs to perform its function well" (p. 32).

Berkowitz realizes that this claim may seem implausible, given virtue's close association with an idea about which liberalism harbors profound reservations, namely, that human nature has "a single perfection" (p. 8) or "ultimate end" (p. 71). Virtue, however, can be understood instrumentally— and hence independently of this idea—as designating various "functional" excellences. It can be understood, in other words, as designating various "qualities of mind and character" (p. 172) necessary to the effective performance of the various roles (e.g., citizen) human beings may find themselves playing. There is thus no necessary incompatibility between liberal principles and the concept of virtue.

Yet, the work of these thinkers illuminates not merely the important but largely unacknowledged role virtue has played historically in liberal thought; it also illuminates the "genuine difficulties" of liberalism "giving virtue its due" (p. xii). To begin with, "liberal ideas about human nature, metaphysical first principles and the [human] good shift attention away from" the "moral and intellectual virtues" constitutive of human excellence, whereas liberal ideas about politics "concentrate attention on the restraint of government from legislating morals." Liberal principles thus "set in motion a conceptual dynamic that all too easily induces silence about virtue and encourages indifference to its cultivation" (pp. 171–2). Simultaneously, liberalism's recognition of its need for virtue has always coexisted with "a certain ambivalence in regard to virtue" (p. xii), an ambivalence that derives from virtue's association with ideas liberalism rejects and practices it deplores, and with the fear that the claims of virtue represent a threat to individual freedom and limited government.

Moreover, insofar as "liberalism depends on virtue that it does not readily summon and which it may ... even stifle" (p. xiii), liberal societies experience grave difficulty in cultivating the qualities they need. Their commitment to limited government sharply circumscribes the ability of the state to protect and promote the virtues on which it depends. Simultaneously, taking for granted the extraliberal nongovernmental "institutions, practices, and beliefs" (p. 23) on which it has historically relied to foster the qualities liberal societies need, liberalism generally fails to appreciate how "the very actualization of liberal principles" can "erode" (p. 174) liberalism's "extraliberal and nongovernmental foundations" (p. 177).

In common with other regimes, liberal orders tend "to form citizens with an immoderate enthusiasm for its guiding principles" (p. 177). This enthusiasm acts to undermine the extraliberal institutions (e.g., the family) on which liberal societies have historically relied to transmit the qualities of mind and character on which they depend. Liberal orders also tend to produce "a dialectic of extravagance and neglect," in which "a favored [liberal] principle" is "carried to [such] an extreme" that other principles and considerations are ignored (p. 179). (Feminism, deliberative democracy, and postmodernism, Berkowitz contends, all exemplify this temptation.) This dialectic acts to obscure both the preconditions of such regimes and the "contending principles" and "practical necessities" (p. 183) that led "liberalism's founding fathers" (p. 33) to "limit the scope and application" of various liberal principles (p. 183).

Virtue thus poses a predicament for liberalism that has no easy or permanent solution. This does not mean, Berkowitz insists, that liberalism should be abandoned. Rather, we need to cultivate a "chastened" (p. 23) and "self-critical" (p. 29) liberalism, one that appreciates the need to maintain "a delicate balance" among the various principles affirmed by

liberalism and "the practical necessities" that require limiting their "scope and application" (p. 183). By taking more responsibility for fostering "the basic qualities" of mind and character "essential to the flourishing of liberal societies" (p. 184), such a view would recognize that "making liberalism work today requires either the renewal" of the "old sources" on which it has traditionally relied to foster the virtues it needs "or the creation of new ones" (p. xiii). It would also be able to distinguish between "indirect and relatively unobtrusive [governmental] measures for fostering these qualities" and "invasive laws and regulations that foist on citizens state-sanctioned conceptions of human perfection" (p. 191).

A thoughtful study, this volume casts light on both a major issue confronting contemporary liberal theory and a neglected aspect of the work of some of the seminal figures in the liberal tradition. By bringing into sharp focus one of the enduring problems of liberal thought, Berkowitz makes a significant contribution to contemporary liberal theory. One wishes, however, that he had pursued the reasons liberalism finds it difficult to give "virtue its due" in a more systematic manner. One particularly wishes he had pursued two subjects on which he only touches: the nature of liberalism as a distinctive intellectual tradition and why "the internal dynamics" of "liberal thought" operate to erode its "capacity" to address the whole subject of virtue (p. 19).

Berkowitz recognizes that "a complete understanding of virtue" involves "a defense of controversial opinions about human nature and the cosmos" (pp. 13, 171) and admits that "to understand virtue's embattled position today" one must appreciate how "the philosophical ideas that partially constitute liberal, Enlightenment modernity" seem "to remove the ground from underneath" it (p. 14). Nevertheless, he seems content to treat liberalism simply as "a political doctrine" (p. 4) and never really pursues either the philosophical ideas that inform the liberal tradition or their bearing on the evolution of liberal political theory. The failure to do so prevents him from seriously considering the possibility that liberalism's difficulty in "giving virtue its due" stems from these very ideas, the possibility that the understandings of man, the good, and the cosmos on which virtue rests are simply unsustainable in the light of liberalism's metaphysical first principles. It prevents him, in short, from seriously entertaining the possibility that liberalism's own metaphysical premises preclude both the theory of virtue that liberal political theory needs and the moral affirmations and politicocultural commitments that are necessary to sustain free and democratic societies.

The Environment: Between Theory and Practice. By Avner de-Shalit. Oxford: Oxford University Press, 2000. 238p. $60.00 cloth, $19.95 paper.

John S. Dryzek, *Australian National University*

Although there is a lot of it about, environmental philosophy has so far had little influence on the world of environmental politics and policy, avers de-Shalit in this fine contribution to ecological political theory. In this assessment he is quite right. As someone who believes there should be a role in public discourse for the philosopher, de-Shalit seeks to provide a remedy for this deficiency. Why should the rest of us care? Because, he argues, the legitimacy of a policy depends on its rationale, which in turn is a matter of "the degree of moral persuasion and conviction it generates within the public arena" (p. 5). Who better to develop warrants for persuasion and conviction than the philosopher? In a democratic society of the sort de-Shalit favors, the philosopher is not just a participant in public deliberations but "leads the discourse, in the sense that she is committed to certain standards" (p. 35).

To de-Shalit, the problem is that environmental philosophers have not measured up to the task. They are guilty on two counts. First, most of them are keen to establish a biocentric ethic that locates intrinsic value in nature, irrespective of human interests. So, when it comes to animal rights, for example, activists care about stopping cruelty, whereas philosophers worry about the moral status of animals. More generally, de-Shalit believes that the biocentric or ecocentric attitude is unlikely to get very far in a world in which most people are more concerned about human suffering, be it from environmental harms or the host of other ills that afflict us. Thus, animal rights will seem absurd so long as human rights are subject to gross violation (p. 20). This claim is not quite true. Biocentric legislation, notably the U.S. Endangered Species Act, protects species irrespective of any human interest in them.

The second count on which environmental philosophers are found guilty is their use of environmental issues to pursue a political agenda that is not at all about nature's intrinsic value. Deep ecology, the most prominent biocentric doctrine, turns out not to be a moral theory for the environment but a program of psychological self-realization that uses environmental consciousness as an instrument to this end (p. 48). Ecofeminism is no better, as it seeks to use the environment in instrumental terms to further its essentially feminist agenda, which is all about relationships of humans with one another, not with the environment. De-Shalit is perhaps a little unfair to deep ecology. On a more charitable interpretation, the material that key texts in this area contain about the need for self-realization through deep ecological consciousness could itself be seen as merely instrumental to respect for nature's intrinsic value.

De-Shalit is right, however, about the implicitly antipolitical character of much ecological philosophy; as a democrat, he wants its claims to be tested in a broader (human) public arena. Such testing goes straight to the heart of how environmental philosophy, or any moral philosophy with political intent, must proceed. De-Shalit advocates a method of "public reflective equilibrium," whereby the philosopher tests his or her theories against the intuitions and reactions of actual participants in political dialogue. This is contrasted with the "private reflective equilibrium" of John Rawls and the "contextual reflective equilibrium" of Michael Walzer, neither of which involves any dialogue. Clearly, de-Shalit offers a philosophical method much more appropriate to a democratic society than that of these two alternatives.

De-Shalit's method fits in nicely with his own environmental political theory. He argues for a democracy that is both participatory and deliberative as well as (for good measure) socialist. Liberalism is found wanting because environmental affairs call for "a politics of the common and the good, and consequently for interventionism" (p. 92). In the environmental area no less than elsewhere, liberalism is perhaps a bit more slippery than de-Shalit allows, although his critique of individualistic liberalism is absolutely on target. Community fares better, but not the historically given version beloved of most communitarians. Instead, it must be "community as collective rational reflection" (p. 108), which provides a nice field for the public reflective equilibrium sought by the philosopher. One might question whether this is really community, but it is democracy, and here de-Shalit is on strong ground. A deliberative and participatory democracy would, he believes, force governments both to disclose environmental risks and to incorporate a broader variety of values in collective decisions (provided, of course, that participants

have developed sufficient environmental consciousness). It also would prevent environmental critique from being converted into financial resource politics, to be addressed by directing government expenditure in the appropriate direction. In the end de-Shalit has great faith in what citizens (as opposed to consumers) will want and seek in environmental affairs.

A noteworthy omission in the book is any sustained attention to the idea of environmental justice, as both a political movement and a political theory. This omission is surprising in light of de-Shalit's comment that "we cannot turn a blind eye to injustice to humans when discussing environmental matters" (p. 215). Overall, however, de-Shalit offers a refreshing and well-argued political alternative to environmental philosophy as guidance for public affairs, one that he hopes will also be received by "both activists and members of the general public" (p. 214). The big question remains: Is anyone listening?

Public Integrity. By J. Patrick Dobel. Baltimore, MD, and London: Johns Hopkins University Press, 1999. 260p. $38.00.

Andrew Stark, *University of Toronto*

Integrity is a shifty, furtive concept. Philosophers have had a hard time defining the idea because it raises a couple of recurrent perplexities. First, consider former Speaker Jim Wright's remark that "integrity is ... the state or quality of being complete, undivided, [and] unbroken," or the *Oxford English Dictionary* connotation of an "unbroken state" of "material wholeness." The problem is that integrity, so understood, seems to leave no room for the possibility of individuals whose lives display any kind of self-critical revision, changes in course, or discontinuities over historical time, or for those who compartmentalize, differentiate, and assume conflicting roles across social space; in other words, for all of us. We need, as Amelie Rorty has written ("Integrity: Political, not Psychological," in Alan Montefiore and David Vines, eds., *Integrity in the Public and Private Domains*, 1999), a far better account as to how and where "integration and integrity ... coincide".

Second, even assuming integrity is simply a matter of straightforward wholeness and integration, the stock criticism is that we would have to consider people who pursue a career of larceny as having integrity, as long as they do so in a whole-hearted and integrated way. Thus, integrity also must connote (to quote the *OED*) "soundness of moral principle; the character of uncorrupted virtue." As Speaker Wright once said, integrity is also "a matter of ... moral behavior." But this is where the notion of public integrity becomes problematic. Public life is a realm of heightened pluralism—we differ deeply as to what constitutes "moral principle" or "moral behavior" in the ends or goals of policy—and of widespread pragmatism—actors whose means or instruments consist only of "uncorrupted virtue" risk getting nowhere. It appears as if we cannot bestow the title "person of integrity" on anyone who holds (major) public office. Perhaps it is because we are so deeply uncertain as to when someone qualifies for the term that integrity, unlike honesty, sincerity, or authenticity, has no adjectival form.

Although J. Patrick Dobel's book is not explicitly organized to do so, it sheds considerable light on each of these questions. Referring as needed to philosophy, fiction, the real world, and his own hypotheticals, Dobel performs a kind of balancing act. He never once glosses over the paradoxes, Catch-22s, grey areas, and hard cases that abound in his

topic, but he manages throughout to offer illumination and insight.

Consider the inconsistencies—shifts, reversals, broken commitments—an official's career can display over time while still remaining one of integrity. On the one hand, as Dobel notes, integrity seems to vanish if we believe that "bureaucratic interest [or] group conformity pressures" (p. 93), "giving in to interest groups" (p. 135), or other "conflicting social influences" (p. 26) have driven an official off the course of her commitments. On the other hand, integrity seems more likely to survive if "any [such] changes are reflective and self-conscious" (p. 118), if they flow from evolutions in a "person's character over time" (p. 184), or if the officials concerned have "train[ed] themselves over time to approach problems in different ways, to judge according to different standards, and to choose in new ways" (pp. 195–6). All this seems to suggest, unexceptionably enough, that inconsistencies in personal history do not jeopardize integrity to the extent that they are internally generated, through character, reflection, or self-consciousness, rather than externally imposed, through interests, pressures, or influences.

Yet, as Dobel shows, quite the opposite is the case when the inconsistencies are arrayed not over personal history, the way we operate within a given role over time, but "social geography" (p. 178), the way we move between different roles at any one time. Here, integrity survives precisely to the extent that we believe any such inconsistencies are externally forced, associated with the different roles themselves. We all understand that diverse roles mutually "entail discontinuity and tensions" (p. 179). But once those inconsistencies move from the domain of "socialized power into one's psychological world" (p. 172), disrupting "stable cognitive and emotional responses" and "internalized patterns of behavior" (p. 196)—in other words, once the discontinuities cease being a feature of the official's differentiated external world and become instead characteristic of a schizoid internal world—we begin to fear for the official's integrity, for her inner ability to connect.

Integrity, then, survives to the extent that an official's psychological gyroscope, the instrument that directs his swerving and careering over time, remains under internal, not external, control. And it persists as long as the social kaleidoscope, the array of shifting and colliding roles faced at any given time, remains an artifact of the official's external world, not part of the internal lenses through which he views that world. What of the problems that pluralism and pragmatism pose for official integrity? As Dobel notes in considering the issue of pluralism, an ethics of integrity cannot ignore "the broader issue of the right," but any moral theory that argues integrity "serves only good ends" does not "capture the complexities and ironies of life" (p. 217), since people hold "various conceptions of the public good" (p. 96). So, Dobel suggests, if we are to associate integrity with an official who supports a policy we believe is wrong, then we must at least be satisfied that his actions flow not from self-interest or self-deception, but from "deliberation and careful judgment" (p. 198) or "moral reasons [such as] internal dissent" (p. 113). Dobel evocatively contrasts Robert McNamara, who by upholding the Vietnam war to the outside world gained sufficient credibility with Lyndon Johnson to challenge its conduct internally, and Henry Kissinger, who cultivated an external reputation as someone troubled by the war, which required him to "assert his own toughness" internally so as not to lose Richard Nixon's confidence (p. 107).

What of pragmatism, the everyday possibility that officials may have to use the wrong means, never mind pursue the wrong ends? Here, Dobel argues, John Le Carré's character

George Smiley "represents the best hope of learning how to live with the moral ambiguity of flawed means" (p. 87). Smiley dislikes violence and even coercion, but he is not someone who, coddling his integrity, believes that his "uprightness" alone will "ensure his position" in any conflict (p. 202). In other words, he is not above getting others to abet a course he believes is right for reasons he himself would never abide, even by appealing to their weaknesses for alcohol, affection, or anonymity. More generally, Dobel writes, to "garner ... consent among self-interested agents" (p. 197), an official must sometimes appeal to those interests, even though he does not share them.

Dobel suggests that if an official of integrity pursues what we think is a wrong course of action, she will at least have done so for what we accept as the right reasons, not as a result of self-interest but after reasoned consideration of principles conducted in deliberation with others. We expect an official of integrity to avoid wrongful means, but we will at least give her scope to accomplish her goal—to induce others to follow what she thinks is the right course of action—by sometimes relying on what she sees as the wrong reasons: by appealing to the sometimes squalid interests of others, instead of relying pristinely on the persuasive power of her own views and principles.

In Dobel's book the outlines of public integrity come into view. We see the extent to which integrity remains compatible with inconsistencies over personal history and social geography, the ways in which it can abide both pluralism and pragmatism. Of course, questions remain. What does it mean, is it even possible, to ensure that one's inconsistencies over time are generated internally, not externally, while those over social space remain externally structured, not internally assimilated? How, in hard cases, do we distinguish between the calculations of interest and the deliberation over principles that help define integrity in a world of pluralism and pragmatism? It is testimony to the success of this fine work that Dobel now directs the study of integrity toward these questions.

Thomas Jefferson and the Politics of Nature. Edited by Thomas S. Engeman. Notre Dame, IN: University of Notre Dame Press, 2000. 232p. $17.00 paper.

Ralph Ketcham, *Maxwell School, Syracuse University*

This volume offers, as its blurb asserts, "substantive discussions of the key issues facing Jeffersonian scholars." Beginning with Michael Zuckert's by now familiar argument that Jefferson's thought is best understood as resting in a Lockean natural rights framework, other able scholars more or less take issue with this analysis by appealing both to Jefferson's own writings and to the works of major interpreters of the political thought of the founding era. The result is a serious reconsideration of Jefferson's thought that takes up most of the key themes raised by Louis Hartz and Bernard Bailyn forty or more years ago over the place of the liberal tradition in American thought. Veteran expositors of Jefferson's thought in a more civic republican and Christian way, Jean Yarbrough and Garrett Ward Sheldon, take issue with Zuckert's Lockean, natural rights emphasis, upholding instead the influences of Aristotelian, Kamesian, and Christian thought on Jefferson. Though one cannot deny the strong Lockean strand in Jefferson's thought, Yarbrough and Sheldon argue persuasively for the strong presence of the other dimensions as well. Zuckert's effort in his "Response" to claim that this mixes without resolving conflicting philosophies, and thus, if Yarbrough and Sheldon are right, leaves Jefferson hopelessly

inconsistent, misses Jefferson's brilliant blending of these outlooks, all obviously present in his writings, into what might be called a Jeffersonian republicanism.

Thoughtful articles by James Ceaser and Joyce Appleby take up important, more limited themes. Ceaser explains the danger, widely by-passed in Enlightenment thought, of projecting from nature as physical science to nature as authority in political and ethical matters. Thus, he details the unfortunate consequences of Jefferson's flawed anthropology (supposedly "scientific") of African racial inferiority in preventing him from moving against the institution of slavery, despite his earnest opposition to it on natural rights grounds. Joyce Appleby insists, as she has in other seminal works on founding era thought, that Jefferson's agrarian bias was neither backward-looking nor anticapitalist. Rather, he advocated (and practiced) an agriculture that depended on the future growth of population (and hence markets) at home and abroad, and on a vigorous international trade in the Atlantic world, in which farmers would participate through growing commercial towns from Boston to Charleston. In particular, Appleby points out that the thirty-year high demand for American grains, from about 1788 to 1818, driven by both the Napoleonic wars and the inability of European farmers to increase production, was the foundation of good times for farmers (including Virginians who switched from tobacco to grains), and the material base for Jefferson's agrarian ideology. Although Appleby understands the moral and political grounds as well, she too little emphasizes how those grounds were in fact a centerpiece of Jeffersonian thought: Those grain farmers would likely be independent, responsible citizens of the republic. Articles by Robert Dawidoff and Robert Fowler, on Jeffersonian "Rhetoric" and "Mythologies," though elegantly written, too much simply quarrel with other scholars and too little get beyond tropes of literary criticism to reach Jefferson himself.

Zuckert's lead essay on "Founder of the Natural Rights Republic," though, sets the basic theme for the articles: Jefferson's thought points consistently toward Lockean, rights-protecting self-government. Zuckert's intention, furthermore, James Ceaser asserts, "is nothing less than to revive the doctrine of natural rights as a ground for modern American political life" (p. 165). This places Zuckert in the large camp of contemporary, liberal theorists who see the protection of the rights of autonomous citizens as the chief function of government. Another large group of contemporary theorists, though, designated civic republicans, communitarians, and so forth, harking to Aristotle and classical understandings of government, see this as not so much wrong as "thin," or "minimal," not in any way fulfilling the rich potential for government, responding to active, deliberative citizens, to further not life merely, but the good life. In Zuckert's summary, "Lockean liberalism affirms natural individualism and self-interestedness, the artificiality of the political community, and the ultimate ordering of government to the securing of self-centered rights." Classical republican theory, on the other hand, "builds on natural human sociability, the moral sense, a rejection of egoism, and an understanding of the human good as the natural fulfillment of human beings in political participation" (p. 197). Zuckert insists that these are incompatible differences, or conflicts, and that Jefferson's thought "at bottom ... is a version of Lockean liberalism" (p. 197). As Yarbrough and Sheldon especially point out, however, there is in Jefferson's writings at least as much sympathy for the classical republican guidelines, as for the Lockean liberal ones. Though modern analysis of political ideas tends to find Locke and Aristotle in opposing camps, one starting with individual rights and the

other with a deliberative political community, somehow Jefferson did not get it. He combined the perspectives in the Declaration of Independence, his First Inaugural Address, and numerous other documents and letters. Near the end of his life he declared explicitly that in the Declaration of Independence, seeking to express "common sense" and "the harmonizing sentiments of the day," he set forth ideas found "in the elementary books of public right, as Aristotle, Cicero, Locke, Sidney, etc." (pp. 15, 73; quoting Jefferson to Henry Lee, May 8, 1825). More attention to Jefferson's writings themselves, and less applying of presentist labels and less bickering among the authors, might have produced an even more interesting and enlightening book of essays.

Hegel's Philosophy of Freedom. By Paul Franco. New Haven, CT: Yale University Press, 1999. 391p. $35.00.
Hegel's Idea of Freedom. By Alan Patten. Oxford: Oxford University Press, 1999. 216p. $45.00.

Peter J. Steinberger, *Reed College*

In a sense, these two books, bearing almost identical titles, could not be more different. Patten's work is a narrowly focused study of those passages in Hegel (primarily in the *Philosophy of Right*) that deal explicitly and pointedly with the idea of freedom. He proposes a "civic humanist" interpretation of Hegelian freedom. Such an interpretation is designed to make sense of what Patten calls the *"Sittlichkeit* thesis," according to which ethical norms are composed of, or otherwise reducible to, duties and virtues embodied in the central institutions of modern social life. Franco's work is much broader in scope. It offers a commentary on the entirety of the *Philosophy of Right* and argues, plausibly enough, that Hegel's political philosophy is fundamentally a philosophy of freedom. It briefly situates that philosophy in the context of Hegel's immediate predecessors (Rousseau, Kant, Fichte) and reviews Hegel's own intellectual development, but its main goal is to show how Hegelian arguments about abstract right, morality, and ethical life constitute an account of what it means to be free.

In another sense, though, the two books are surprisingly similar. Each purports to offer an interpretation of Hegel's political thought that takes seriously the larger philosophical or metaphysical claims in which that thought is embedded. Patten's civic humanist view explicitly presupposes the idea that "Hegel is committed to giving a rational warrant for existing institutions and practices through philosophical reflection" (p. 41), while Franco agrees that Hegel's political philosophy cannot be separated from his "speculative logic insofar as that latter articulates and justifies his views on philosophical method" (p. 140). Evidently, this is now the dominant trend in Hegel studies and is much to be applauded. Nothing could be clearer than that Hegel understood his political thought in general and the philosophy of right in particular to be part of a larger philosophical system. Without an understanding of the system, the political claims are unintelligible, in principle and in fact. Of course, all of this presents special problems for interpreters of Hegel, and the degree to which Patten and Franco deal successfully with such problems is an interesting question.

Patten distinguishes his civic humanist view from a "metaphysical view," which he associates primarily with Charles Taylor, and also from a "historicist view," which he associates mainly with Robert Pippin. He is painfully equivocal on the nature of the difference, however. He readily concedes that the three views are not necessarily incompatible and, indeed, may be thought of as "converging perspectives" (pp. 40–1).

In this context, the utility of identifying a distinct civic humanist view remains unclear. Presumably, Patten wants to say that his account does a better job of showing how Hegel identifies freedom with "self-actualization" and with the idea of "participation in community with others" (p. 38). But this seems to me less than a departure than a gloss on much of what Taylor and Pippin (and many others) have already said; if anything, it runs the risk of identifying Hegel's political thought with a much more conventional, and philosophically much different, tradition of civic republicanism.

In pursuing the idea of self-actualization, much of what Patten says makes good sense. For example, he shows that freedom, as Hegel understood it, is not so much a rejection of desire per se but a matter of acting from desires or feelings that are "reasonable or appropriate in the circumstances"— for example, a husband's feeling of love for his wife (p. 63). One's desires are or ought to be self-legislated, so to speak. The point is an important one, and Patten makes it well, although it has also been made elsewhere and at greater length, something Patten fails to acknowledge.

At the same time, his interpretation, even as it seeks to approach Hegel on his own terms, seems to back away from the full force of the larger Hegelian system. Hegel's ethical thought (like Kant's) presents a seeming paradox: If the free agent is not primarily motivated by authority or desire, then what other motivation could there be? Patten's answer is that the agent "has reason to establish and maintain his own freedom and independence" (pp. 102–3). On the one hand, such an answer seems to be circular and question-begging. Why is the agent interested in maintaining freedom and independence? On the other hand, and even more importantly, it seems to ignore some of the boldest yet most characteristic claims of Hegel's philosophy, namely, that rational inquiry necessarily produces a determinate content, and that the freedom of the individual qua rational agent is a matter of thinking and living in accordance with that content.

I find similar equivocations or hesitations in Franco's book. Sometimes this is a matter of failing to explain fully what needs to be explained. In working through Hegel's concept of the will, for example, Franco concludes that the idea of freedom is the idea of "being with oneself in an other" (p. 162). But exactly what does that mean? After five pages of background, Franco devotes barely a single page to the idea itself and offers little more than a series of quotations from Hegel's text without substantial explication.

Elsewhere, the problems seem to me rather more technical. For example, Franco says that Hegel identifies ethical life with "an unreflective and habitual ethical disposition" (p. 228), and criticizes certain authors (including me) for overemphasizing the reflective and self-conscious rationality of Hegelian citizens. But while he dutifully quotes an appropriate passage from Section 151 (Addition) of the *Philosophy of Right*, he pointedly fails to mention a crucial part of the very same section in which Hegel explicitly says that "habit [*Gewohnheit*] is part of ethical life just as it is also part of philosophical thought." Presumably, philosophers take for granted, say, the law of the excluded middle or the other basic principles of logic, all of which are invoked more or less habitually. Does this mean that philosophers are unaware of or do not rationally understand those principles? If Hegel's analogy of the philosopher means anything, surely it suggests that citizens are more rational—more capable of self-consciousness and self-reflection—than Franco would have us believe. (On this general question, I think Patten [p. 75] makes much the stronger case.)

More generally, I believe that Franco's work reflects the same kinds of hesitations that one finds in Patten's. This

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involves, again, a certain unwillingness to accept fully the claim that rationality involves, for Hegel, a determinate and necessary content of some kind. Franco acknowledges that Hegel identifies freedom with rational necessity, but he denies that this necessity is a kind of "fact that first exists outside of human freedom and only later comes to lose its alien character by being understood" (p. 181). I don't see a clear argument for such a denial. Franco insists that "the rational necessity of right ... is itself derived from freedom, is produced by the logic of freedom, consists in the immanent development of freedom." This seems to me quite right, but it also seems to me entirely consistent with the idea that freedom unfolds according to an internal, objective logic of some kind and that the process of unfolding results both in the fulfillment of human freedom and in the attainment of substantial truth.

I have mentioned here only a few of my criticisms—of Patten and Franco alike. I do not regard these criticisms, moreover, as mere quibbles; they speak to the heart of Hegel's idea of freedom and reflect very serious disagreements. On the other hand, disagreements of this kind are only to be expected, and I might very well be wrong on all counts. The larger point is that these two books, in their different ways, are to be taken seriously as sober, informed, and intelligent efforts to make sense of their subject matter. I would not hesitate to recommend them, both as very useful introductions to Hegel's political thought and as plausible contributions to a series of important and on-going scholarly conversations.

Marxism, Revisionism, and Leninism: Explication, Assessment, and Commentary. By Richard F. Hamilton. Westport, CT: Greenwood, 2000. 288p. $59.95.

Alex Callinicos, *University of York*

A distinguished political sociologist, Richard F. Hamilton is perhaps best known for *Who Voted for Hitler?* (1982). More recently his attention has shifted to the broader methodological issues raised by the empirical claims of sociological macrotheories. This critical study of three versions of Marxism—the original statement by Marx and Engels and two rival reformulations, by Eduard Bernstein at the end of the nineteenth century and by Lenin during World War I—is to be seen in the light of this concern.

There are, even so, two ways of taking this book. One is to accept at face value Hamilton's statement of his aim: "to provide a statement of the principal claims of three major theories and to indicate, through a review of relevant evidence, the empirical adequacy—the validity—of those claims. The task, put simply, is to take stock, to undertake an inventory of our theoretical holdings" (p. 9). He concludes that a clearing out is needed: We should "consign the Marxian theory to intellectual history courses" (p. 208).

The other way to view the book is to treat the objectivist language in which it is written as a rhetorical disguise for a much more partisan exercise, a systematic debunking of Marxism that generally selects for the least charitable interpretation. For a number of reasons, I think this view is the more appropriate.

The book starts with a detailed description of the liberal tradition to which Marx stands as both heir and critic. This is an interesting way to approach the subject and makes a refreshing contrast with the often tedious trot through Hegel and Feuerbach on which less imaginative commentators take us when situating Marx. Yet, the aim is more than mere scene setting. Hamilton stresses liberalism's economic achieve-

ments and laments the failure of university teachers to provide the citizens of liberal societies with the intellectual resources required to defend this tradition. Strike one against Marxism, even before the game begins.

The *trahison des clercs* is also a theme of the final chapter, which puzzles over the continuing influence of Marxism when it has been so manifestly refuted. The reasons Hamilton offers are partly (to adopt his own classification) psychological (individuals' psychic investment in theories) and social-psychological (intellectuals' tendency toward conformism), but they also involve sociologists' ignorance of economic history, which leads them still to accept such theories as Weber's Protestant Ethic thesis or Lenin's theory of imperialism, long since abandoned by the accredited experts in economic history departments. The *parti pris* that these reflections manifest, not only toward Marxism but also toward (it has to be said, a somewhat outdated view of) sociology, is unmistakable.

Between these two chapters Hamilton subjects the principal claims of Marx, Bernstein, and Lenin to serious analysis. Yet, he fails to observe one of the elementary principles of criticism, namely, to take on the strongest version of the opponent's theory. There are two particularly clear instances of this. First, Hamilton makes the *Communist Manifesto* the basis of his statement of Marx's theory, but this text was written before Marx had performed the bulk of the economic research whose eventual outcome was *Capital*. In the course of this research, Marx both considerably elaborated and, in certain crucial respects, substantially altered the theory of capitalist development sketched out in the *Manifesto*. For example, Marx came to reject the "iron law of wages" developed by Ricardo and Malthus, accepted by most nineteenth-century economists as well as Marx himself in the *Manifesto*, according to which wages tend toward the bare minimum of physical subsistence. Yet, Hamilton attributes this idea to Marx, presenting its rejection as if it were a defensive adjustment to recalcitrant facts rather than a consequence of Marx's theoretical critique of Ricardo's basic assumptions in the 1850s. I had thought that modern Marxist scholarship had made further discussion of the so-called immiseration thesis otiose. By relapsing into outdated anti-Marxist folklore, Hamilton does neither himself nor his readers any favors.

This lapse is symptomatic of Hamilton's failure to address properly Marx's economic theory. The focus on the *Manifesto* means that Hamilton does not mention the theory of the tendency of the rate of profit to fall, developed between the *Grundisse* and *Capital*, which has further damaging consequences for his appreciation of Marx's thought. He asserts that "Marx and Engels regularly portrayed the bourgeoisie as rational, informed, and knowledgeable" (p. 61), but Marx claims that a collectively suboptimal result—a falling general rate of profit—is produced by capitalists behaving in ways that are individually rational in seeking to maximize their own profits, which suggests a view of their behavior as myopic rather than perfectly informed.

The second instance is the easy enough work Hamilton makes of Lenin's theory of imperialism, pointing out the various respects in which it is contradicted by historical research. Once again, this is a soft target. Lenin's pamphlet *Imperialism* is subtitled "A Popular Outline" and draws heavily on more original research, not only Hobson's *Imperialism* but also Hilferding's *Finance Capital*. Amazingly, Hilferding does not even appear in Hamilton's index. Entirely ignored is the substantial, theoretically sophisticated, and empirically wide-ranging body of Marxist writing on imperialism to which not only Hilferding but also Luxem-

burg, Bukharin, Bauer, Grossman, and Preobrazhensky contributed. To judge this entire discourse by one relatively crude text is not what one would expect from a scholar of Hamilton's standing.

I do not in the slightest suggest that Marxism is beyond criticism. On the contrary, capitalism is still with us, and even if one takes a less favorable view of this system than Hamilton does, its persistence raises pressing questions that those still committed to the tradition Marx founded must address. Hamilton is well equipped to put many of these questions, as is shown by the quality of those sections in his book that compare Marxian claims with the class structure of twentieth-century societies. It is a pity that he should lapse so frequently from the standards set by scholarly discussion of Marxist theory over the past generation.

Revolutionary Values for a New Millennium: John Adams, Adam Smith, and Social Virtue. By John E. Hill. Lanham, Maryland: Lexington Books, 2000. 213p. $55.00.

Peter McNamara, *Utah State University*

No one will deny John E. Hill's claim that he has written an "unabashedly didactic" book (p. xi). This is not social science, or political theory, or history as it is usually understood by those disciplines: I do not mean that as a criticism, for there is great merit in writing as a concerned citizen-scholar. Hill puts his political cards on the table. He is a self-described "moderate liberal" (p. xi) who wants universal health insurance, public funding of elections, more restraints on the corporate sector, a more progressive tax system, more spending on education, and community service programs. He also wants liberals to rethink their attitude toward morality: They need to be more forthright about the importance of morality—social virtue—for the health of the Republic. In addition, Hill does not shy away from telling us that he does not like "individualistic excess" (p. ix), Alexander Hamilton, Ronald Reagan, or the religious Right.

Revolutionary Values attempts to provide an intellectual grounding for this very contemporary liberal agenda and to show the continuities between it and the goals and beliefs of the founders. Hill looks to John Adams as his primary source of insight into that generation and to Adam Smith as a way to buttress and, on the issue of the role of government in the economy, to supplement Adams. Hill achieves only partial success in what must be acknowledged as a bold intellectual strategy.

Large parts of the book are devoted to setting the record straight about Smith and Adams. The latter, Hill argues, has suffered from undeserved neglect. He regards Adams as a first-rate statesman and a political thinker of the highest order. He grants that Smith is still famous, but usually for the wrong reasons. Smith is erroneously regarded as an "advocate of pure laissez-faire" (p. 150) and, furthermore, his often overlooked moral philosophy provides a serviceable moral foundation for a liberal society. Hill acknowledges that he is not the first to make either of these general arguments. What is distinctive is his identification of a strong egalitarian streak in both men, who were highly suspicious of the rich and powerful. Adams sought to restrain their influence by placing them in a separate chamber of the legislature. Smith's attack on the mercantilist economic system was clearly, at one level, an attack on entrenched economic power.

But to make the leap from this egalitarian streak in Smith and Adams to the contemporary liberal agenda Hill advocates requires that certain key features of their thought be virtually ignored. Hill does not, for example, share the reservations of Adams (and the other founders) about democracy. Similarly, Hill all but obliterates Smith's reliance on a "system of natural liberty" to resolve income distribution

issues. That is, he neglects the extent to which Smith relied on an indirect rather than a legislative approach to solving problems. In other words, it is one thing to say that Smith is not Milton Friedman, but it is very much more arguable to suggest that there are no huge differences between Smith and John Kenneth Galbraith or Robert Reich. Also, it does not profit Hill to point to an indigenous American tradition of activist government that is coeval with the Revolution. To suggest, as he does, a simple continuity between this kind of activism, which was usually directed toward promoting economic development, and the kind of social agenda Hill favors is again to blur important distinctions.

Hill is on stronger ground when he addresses the social virtues. By these he means civility, tolerance, moderation, public spirit, justice, generosity, and the like, which formed the core of what might be called the revolutionary consensus. Adams—the family man, republican citizen, and statesman—was an exemplar of these virtues. This consensus was the product of a blending of three strands of American life: the liberal, the civic republican, and the religious. Hill points out that the social virtues found support in institutions of all kinds, public and private, secular and religious. One would be hard pressed to find anything objectionable in setting up these values as some sort of ideal, but there is a significant problem in Hill's optimism that this consensus can be more or less fully revived (and updated, e.g., to include equal rights for women). The difficulty is evident in Hill's frequent lament that postrevolutionary America quickly transformed into a dynamic commercial republic with an abundance of "individualistic excess," by which he means, chiefly, speculation, avarice, and luxury, accompanied by a decline in public spirit and republican simplicity. The question that immediately arises is whether the revolutionary consensus was the product of a particular and rare set of circumstances, notably a grave and unmistakable crisis. Without another such crisis, one wonders just how likely is a revival of those revolutionary values Hill justly celebrates.

Constituting Feminist Subjects. By Kathi Weeks. Ithaca, NY: Cornell University Press, 1998. 196p. $39.95 cloth, $13.95 paper.

Kimberley Curtis, *Duke University*

Feminist theorists have struggled to develop accounts of women's oppression that are historically specific enough to capture the variability in forms of male domination but do not neglect possible transhistorical and transcultural features. A related challenge has been to theorize a feminist subjectivity that can anchor an oppositional politics but avoid an overly unified notion of feminist subjectivity, while nevertheless attending to the patterned effects that women's systematic location in the social whole may have on their subjectivity.

These challenges were the subject of sometimes polemical but mostly fruitful debates throughout the 1980s and 1990s. They have been deeply shaped by two distinct developments. The first is the political struggles and theoretical writings of women of color, whose work on subjectivity and male domination has consistently pluralized women's subjectivity as well as historicized and contextualized male domination in relation to other axes of power. The second is postmodern critiques that charge modernist modes of theorizing are governed by either ahistorical metanarratives insufficiently attentive to variation and contingency or essentialist understandings of subjectivity in either a humanist/liberal/voluntarist or determinist/Marxist form.

It is in the context of this second development, which she casts as the modernist-postmodernist paradigm debate, that Kathi Weeks situates her carefully argued study. Charging

that the either/or nature of this paradigm debate, although initially useful, has become symptomatic of stagnation, especially in relation to feminist theory, Weeks attempts to "clear a space, propose some tools, and suggest a framework" (p. 159) for theorizing feminist subjects. She searches for concepts subtle and supple enough to support a conceptual framework that attends simultaneously to the constitutive power of structural forces and to the subversive potential of feminist collective subjects. Undergirding this enterprise is a clear commitment to social transformation, and it is to a reworking of feminist standpoint theory, a version of socialist feminism, that she turns.

To do this reworking, Weeks first wages battle with the paradigm debate's reductions, polarizing animosities, and suppression of innovation. She rightly argues (and joins a growing chorus of contemporary voices) that both modern and postmodern theorizing have rich and heterogeneous traditions, and the "repertoire of reductions" (p. 63) each side uses to disable the other serves only at this point to cripple the task of theorizing.

The first two chapters of *Constituting Feminist Subjects* pursue the question of how the selective critiques of modernist theorizing by Nietzsche and Foucault could become transformed in North American discourse into "a radical challenge to modernism itself" (p. 57). Weeks is interested here primarily in two recoveries. First, she seeks to disrupt the seamless lineage the postmodern paradigm traces between Nietzsche and Foucault because she believes Nietzsche alone constructively works with the "post-Enlightenment" dilemma of the subject, namely, of being both determined and agentic. In so doing, it is Nietzsche rather than Foucault who has the most to offer feminist theorists at this historical juncture. The pearls lie, in Weeks's view, in the doctrine of the eternal return, read in a Deleuzian vein as a selective ethical principle.

The second recovery that preoccupies Weeks is the heterogeneity of the modern tradition itself. The hegemony of the modernist-postmodernist frame has silenced modernity's still relevant interparadigm debate with ancient thought and, of primary interest to Weeks, the debates among Marxists, between Marxism and Enlightment liberalism, and between Marxism and socialist feminism.

These recoveries set the stage for the work of chapter 3, where the concern is to reinvigorate and legitimize our aspiration to totality. Weeks draws selectively from Marxist social theory—Lukacs, Althusser, and Negri—to argue for the necessity and existence of theoretical tools with which to grasp the social at the level of system, but to do so by conceiving it as "a totality of social forces that is open to the possibility of antagonistic subjectivity" (p. 93).

Not until the fourth (and last real substantive) chapter does Weeks, having cleared her ground and gathered some of her tools, turn to feminist standpoint theory. This is the most original chapter in the book, and it is a shame it takes quite so long to get there. Here, too, Weeks gathers concepts with which to relate system and subject in nonessentialist ways. The first resource is the project of totality understood as "a methodological mandate to relate and connect, to situate and contextualize, to conceive the social systematically as a complex process of relationships" (p. 5). The second is the feminist standpoint theorists upon whom Weeks draws (Nancy Hartsock, Hilary Rose, and Dorothy Smith), who focus on the gendered division of labor and understand women's laboring practices as constitutive practices, constitutive of both what women know and who they are or can become. The third resource is the concept of standpoint itself, which Weeks defines as "a collective interpretation or reworking of a particular subject position rather than an immediate perspective automatically acquired by an individual" (p. 8). Put together, and properly elaborated, these resources form "a

political project based on the alternative ways of being, desiring, and knowing that can be developed from women's laboring practices" (p. 8).

The creative move Weeks makes in her selective appropriation of existing feminist standpoint theory is her contention that we must depart from the dominant tendency to develop the epistemological consequences of the gendered division of labor. If we are to milk its resources in the service of constructing antagonistic feminist subjects, it is to the ontological consequences of women's laboring practices that we must turn our critical attention. Moreover, a Nietzschean critical ontology of practices is of most use. Our questions must become: What alternative pleasures, desires, habits, wills, abilities, and practices do women enact that could, selectively valorized, provide the basis for an alternative, antagonistic standpoint from which to order/value our world differently? What are the values imminent in our practices that could be affirmed, not in the sense of mere repetition but of subversive repetition that allows us to take our being beyond itself without appealing to something outside of itself?

Weeks' Nietzschean-inflected focus on the ontological aspects of women's laboring practices as processes of constitution and enactment breathes new life into feminist standpoint theory. Moreover, she works carefully, and her nuanced effort to bring disparate, often antagonistic traditions of theorizing into fruitful dialogue is exemplary. Yet, because of the time she spends clearing the theoretical ground for the project, the actual space and effort she devotes to giving us a rich account of feminist subjectivity in practice is disappointingly small. What is especially absent (to her credit, Weeks points this out herself) is engagement with the work of women of color that persistently draws attention to the divisions of class, race, ethnicity, and cross-national boundaries. The book really is a prolegomena: a gathering of tools and a clearing of space to prepare the way for a rich theorizing that, we hope, Weeks will take up in her next project.

Weeks does a marvelous job of identifying the deforming aspects of the modernist-postmodernist debate as she unlocks the cases that imprison her pearls. In so doing, she confronts us with the clubs we often use and with the destruction they do. That is no small contribution.

Constitutional Interpretation: Textual Meaning, Original Intent, and Judicial Review. By Keith E. Whittington. Lawrence: University Press of Kansas, 1999. 320p. $39.95.

Judith A. Baer, *Texas A&M University*

With friends like Edwin Meese and Robert Bork, "jurisprudence of original intent" (p. 3) needs no enemies. These polemicists have so corrupted originalism by associating it with reactionary ideology and partisan politics that, in Keith Whittington's words, "the task now is to convince critics to take [it] seriously again" (p. xii). *Constitutional Interpretation* ably performs this task. Whittington's rescue of originalist jurisprudence from its strangest bedfellows in itself is a major contribution to the study of constitutional law. But, although originalism has found a genuine friend, the book's powerful argument against "dismissing originalism as an interpretive method" (p. 162) does not constitute an affirmative defense. Whittington's efforts to make this case are informative and provocative, but they fail. This failure is traceable to serious defects in both the structure and content of the book.

Whittington has made organizational choices that militate against clarity. He jumps from analysis and critique of arguments against originalism (chaps. 3 and 4), to his argument for originalism (chap. 5), and finally to a defense of

originalism against still more critiques (chap. 6). This approach has the virtue of inviting the reader to join the author in a process of scholarly inquiry, and Whittington makes important points along the way. If his response to postmodern, hermeneutic, and literary "theories of language and interpretation" (pp. 12-3) does not quite rescue originalism from these new and powerful enemies, it 'fills a gap in constitutional jurisprudence. But the organization of the book has the defect of obscuring both theme and thesis. The author would have done better to make the positive case first and follow it with critique and rebuttal. Yet, this sequence would have made the argument clearer, not necessarily better.

Whittington reminds us that "originalists have not committed themselves to a conservative policy program and could, in principle, reach results that are actually antagonistic to that program" (pp. 167-8). Anyone familiar with Hugo Black's dissent in *Adamson* v. *California* or Michael Curtis's *No State Shall Abridge* (1986) will take the point. Whittington's effort to rescue originalism from its other familiar bedfellow—judicial self-restraint—is less successful. "Uniform passivism in the face of violations of the interpretable Constitution," he insists, "destabilize the meaning of the text and contradict the expressed intent of the sovereign people" (p. 168). Whittington avoids the common error of conflating democracy with majoritarianism: "The Court is not simply an antidemocratic feature of American politics but is an instrument of the people for preserving the highest promise of democracy" (p. 111). Yet, after making clear that his position is not dependent on a preference for restraint, he ends the book with this ringing statement: "The discipline of originalism promises to protect the Court from itself, and, in so doing, to protect us from the Court" (p. 219).

Why do we need this protection? Why not continue to muddle through as we always have, with our motley collection of interpretive strategies? An effective argument that choice among methods is necessary must specify decisions in which the judiciary exceeded its power and mount a persuasive argument that these rulings, taken together, represent a threat to the people. Every student of constitutional law has a personal list of specific examples of judicial usurpation. The usual suspects include *Lochner* v. *New York*, *Roe* v. *Wade*, *INS* v. *Chadha*, *City of Boerne* v. *Flores*, and, of course, the New Deal cases. But several of these decisions have been reversed or neutralized—indirectly, at least, through the democratic process they allegedly usurp—and not all these cases appear on everyone's list. Judgment on any actual case depends as much on personal politics as on constitutional theory. The challenge for the constitutionalist is to separate theory from politics by linking diverse cases into a coherent constitutional jurisprudence. Alexander Bickel's *The Least Dangerous Branch* (1962) is a model for such scholarship, whatever one

thinks of its thesis. But the index to *Constitutional Interpretation* lists exactly eight cases.

Without establishing the existence of a problem for originalist jurisprudence to solve, Whittington has difficulty making a case for the method. Nevertheless, he makes a significant original contribution to the study of constitutional law. Whittington "seeks to ground the authority of originalist jurisprudence, and the Constitution itself, in a theory of popular sovereignty" (p. 111). As does Bruce Ackerman (*We the People,* 1991), Whittington asserts that "the people emerge at particular historical moments to deliberate on constitutional issues and to provide binding expressions of their will, which are to serve as fundamental law in the future when the sovereign is absent" (p. 135). "Democratic dualism" (pp. 135–42) distinguishes between the sovereign will, expressed in the constitutional text, and the will of government agents, who are bound by that text. Judges must adhere to original meaning because the Constitution contains binding expressions of the popular will.

Alexander Hamilton and John Marshall were right after all. Whittington argues that the founding moment was more democratic than the authors of *Federalist* 78 and *Marbury* v. *Madison* may have desired or realized: "Popular campaigning, pamphlet and newspaper commentary, and barroom discussions demonstrated the expansiveness of the deliberative process and the inclusiveness of the popular sovereign" (p. 152). Barrooms, yes, but what about sewing circles and quilting bees? The founding moment was inclusive and expansive only if one presumes that democracy is whatever exists in the United States at any given time. To confine judges to original intent in the name of popular sovereignty might frustrate rather than enhance democracy. And spurious inclusiveness is not the only problem with the "constitutional moment" thesis, which works only when the exercise of popular will results in constitutional (re)making. The founding period qualifies, of course, and so does Reconstruction. But what about the New Deal controversy? Where are judges to go for guidance about this constitutional moment? A more basic difficulty with Whittington's thesis is its reliance on duality and dichotomy: The sovereign will is present or absent; the people or their agents act. Yet, both constitutional interpretation and agent-principal colloquy go on continually; popular sovereignty may be more constant and less episodic than this book presumes.

Constitutional Interpretation is an ambitious project that does not quite accomplish all the goals the author sets. Yet, the book is well worth reading, both for its rehabilitation of originalist jurisprudence and its linkage of constitutionalism and democracy. Whittington has produced a book that demands the serious attention of scholars in constitutional law and American government.

American Politics

Campaign Dynamics: The Race for Governor. By Thomas M. Carsey. Ann Arbor: University of Michigan Press, 2000. 232p. $49.50.

Nelson C. Dometrius, *Texas Tech University*

Carsey presents a sound piece of research planted solidly in two fields: voting behavior and state politics. The primary thrust is testing a model of campaign strategy and voter

reaction. Carsey takes advantage, as have many recently, of the steadily accumulating state campaign and exit polling data. Although not nearly as rich as the American National Election Survey (ANES) in content, these data provide reliable state samples and an abundance of cases in each state, which the ANES seldom does. We now often can use the states as true social science laboratories of democracy—expanding *N* without adding the confounding factors that plague across-time comparisons of national elections.

Carsey's theoretical base, lucidly presented, is in the ra-

tional choice and spatial voting literature. His complaint, starting from the *American Voter* (Angus Campbell, 1960) and moving forward, is that too much research treats key electoral elements as either fixed or exogenous: There are certain voter attitudes that even the most golden-tongued candidate is unlikely to shift. Candidates thus must try to locate the issue stances of the mystical median voter, proclaim them their own, and defend that position to the death. Yet, candidates cannot shift ground freely, for they are likely both to alienate their activist base and to be disbelieved by voters. Carsey, borrowing from William Riker ("Heresthetic and Rhetoric in the Spatial Model," in James M. Enelow and Melvin J. Hinich, eds., *Advances in the Spatial Theory of Voting*, 1990), proposes an alternative view, heresthetic change: A campaign dynamic alters the nature of the issue space. Campaigns can increase the salience of those issues on which the candidate is in a favorable position vis-à-vis the electorate and ignore those on which the candidate is at a disadvantage. To control the campaign agenda—the weight voters place on an issue—is to win.

Carsey proceeds to test this model in multiple ways. The Virginia and New Jersey 1993 gubernatorial campaigns are used as case studies in chapters 5 and 6. This is followed by quantitative examinations of abortion salience in 34 gubernatorial elections in 1990; presidential approval salience in 29 elections in 1990 and 1994; and the salience of various issues in 71 gubernatorial elections between 1982 and 1992. In the quantitative analyses, an interactive term is used to test for the heightened effect of an issue if it was stressed by one of the candidates. Carsey gathered an impressive set of data and has produced a work of considerable value to scholars of both voting behavior and state politics.

My qualms stem equally from the analyses performed and from recurrent overstatement of the inferences they justify. Carsey is honest about data and analytic shortcomings, but he tends to forget his own caveats in the chapter conclusions, a failure to which we all succumb on occasion. First, the term "heresthetic change" may be distinctive, but the concept is not novel. Witness numerous articles on priming or issue salience beyond those cited by Carsey, and especially a very similar approach of combining public opinion polls with insider information about John Kennedy's 1960 campaign strategy (Lawrence Jacobs and Robert Shapiro, "Issues, Candidate Image, and Priming: The Use of Private Polls in Kennedy's 1960 Presidential Campaign," *APSR* 88 [September 1994]: 527–40). There are some mild theoretical distinctions between priming and heresthetic change but not empirically distinguishable ones. Carsey does add a clear theoretical embedding in rational choice as well as a model test with multiple campaigns, both of which are sufficiently important to justify the work without padding.

Second, the theoretical dismissal of persuasion and candidate position change is too harsh. Campaigns are less about total attitude change—candidate or voter switches from pro-life to pro-choice—and are more about contemporary policies that constitute baby steps, such as battling over partial birth abortions. Simplification may be a necessary analytic strategy, because mild persuasion, candidate change, and heresthetic change would be hard to disentangle empirically, but one should be cautious about reifying an analytic simplification into a theoretical verdict.

The case studies are a critical element, for the argument requires both intentionality and consequence. Campaigns must consciously seek to highlight issues on which the candidate is well positioned, and the voters must respond by seeing that issue as particularly salient to their choice. The case studies tackle the intentionality component but not

convincingly. A true case study would test the argument by gathering data on attempts at various campaign strategies to determine whether manipulating issue salience was a primary one. As presented, the cases are illustrations, selective incidents or comments consistent with the theory; they lack any criteria or comparison that would allow the reader to assess this explanation versus others. The cases are useful, especially the polling data that show voter changes across the campaigns, but they leave the theoretical argument as only plausible, not demonstrated.

Chapter 7, on the role abortion played in 1990 gubernatorial campaigns, is effective but could be stronger. Here and in chapter 9, Carsey identifies issue salience in each campaign through an exhaustive review of newspaper coverage for each election. He convincingly argues that an article or word count would misstate an issue's importance, but he substitutes his summary judgment instead (a salient or not salient dichotomy), which is also problematic. We have no intercoder reliability information (this part stems from his solo dissertation research) and doubtful replicability. Carsey's judgments are probably reasonable, but one wonders if an issue is really salient because a news story states that "Candidate Smith today continued to stress her views on taxes" (p. 51), even if that issue is seldom mentioned in the newspaper. The assumption is that such a news story reflects recurring themes highlighted in candidate commercials, literature, and debates. This is reasonable, but I would prefer some attempt at verification.

I am puzzled by Carsey's inclusion of interactive terms for gender, Catholicism, and abortion attitudes to assess their increased effect when abortion is a salient campaign issue. Gender and Catholicism are sources of abortion attitudes in a causal sequence, and to include all three variables in a single equation equals controlling for the very effect for which you are searching. Carsey recognizes this at the end of the chapter, but a simple respecification of the model, which would have clarified things, is not done.

Chapters 8 and 9 extend the analysis by exploring the salience of presidential approval in 1990 and 1994 gubernatorial campaigns, as well as the salience of numerous issues in 71 campaigns between 1982 and 1992. In each case, assumptions about issue salience in the campaigns (chapter 8) and to various groups (chapter 9) are far more tenuous than the earlier abortion analysis. They augment the argument, but the abortion analysis remains the crucial test.

In sum, Carsey has produced a valuable work. Certain reservations about various key elements are perhaps inevitable, given the size of the task Carsey undertakes. It is good and readable scholarship and should encourage others to take up the challenge and extend the analysis.

Supreme Court Decision Making: New Institutionalist Approaches. Edited by Cornell W. Clayton and Howard Gillman. Chicago: University of Chicago Press, 1999. 344p. $55.00 cloth, $19.00 paper.

Michael McCann, *University of Washington*

The editors of this timely volume announce at the outset that their aim is to provide a forum for recent scholarship that reacts critically to the previous generation of behavioralists who, since the 1950s, have analyzed the U.S. Supreme Court as little more than an aggregate of the relatively stable and identifiable policy preferences held by individual justices. Specifically, these essays pose a collective "response by a succeeding generation of Supreme Court scholars who are trained in political behavioralism but who have rediscovered

the value and importance of understanding institutional contexts" (p. 12).

Instead, the volume reveals a rather more complex intellectual dialogue among three distinct approaches to an understanding of judicial decision making that are all vying for prominence today. The leading voices in the collection espouse two quite different versions of the "new institutionalism" that have become broadly recognized throughout the discipline: rational choice institutionalism, also known by its more formal title of "a positive theory of institutions" (PTI), and historical or interpretive institutionalism. The third approach is the attitudinal model, which was inherited from the earlier behavioral era but is still very much alive for its critics and the spirited advocate (Jeffrey Segal) who writes one of the volume's chapters.

The book is organized into three general parts that follow the editors' introductory chapter. Part 1 provides a historical and comparative theoretical grounding. Separate chapters by Cornell Clayton and Howard Gillman make the case for expansive historical interpretive study of institutional norms and forces that shape judicial decision making. A chapter by Forrest Maltzman, James F. Spriggs II, and Paul J. Wahlbeck outlines the alternative rational choice version of new institutionalist analysis. All these chapters are highly interesting because they clearly draw the lines of theoretical division, which enables readers to discern and assess the similarities and differences among the three different approaches in unusually accessible terms. From these accounts, it is apparent that both versions of new institutionalism push beyond behavioral approaches to a broader, more complex understanding of the context in which justices act. For both approaches, institutional forces that are both internal and external to the court matter. At the same time, neither approach simply echoes traditional institutionalist, and especially legal formalist, convictions. The new institutionalists are openly postrealist and postbehavioral; they accept to some degree that justices act on preferences, that justices are political policymakers, that legal rules or norms do not "determine" what justices do, and that instrumental strategic factors matter for judicial decision makers.

But the two new institutionalist approaches also are at odds with each other in important ways. Rational choice, or strategic action, analysts are much closer to the older behavioral approach. Like attitudinalists, they emphasize that judicial actors are motivated by "sincere" personal policy preferences that tend to be stable, well ordered, and exogenous to the analysis. The notable contribution of PTI theorists is the complex view that balances appreciation for preference maximization with recognition that justices "are strategic actors who take into consideration the constraints they encounter as they attempt to introduce their policy preferences into law" (p. 46). As such, institutional rules, relations, and actors shape the context of judicial choices, but they do so primarily as external instrumental forces that constrain options for practical action.

By contrast, historical or interpretive approaches emphasize the constitutive power of institutional norms and practices that enable and authorize as well as constrain justices. In this view, institutional rules and norms are not external to actors; they are internalized and expressive of judicial actors' very understandings, aspirations, and choices. In sum, institutions shape the very identities, interests, and imaginations of policymaking justices in complex ways. The development of such insights typically results in studies that are inherently more varied, indeterminate, and complex, and hence less positivist and parsimonious, than the formal efforts of rational choice institutionalists.

The diverse chapters in the other two parts of the book tend to be more empirical, less theoretical, and rather less contentious in style. Many of the essays avoid easy identification with either institutionalist camp identified in the early essays, but all of them very well illustrate the value of attention to institutional influences on justices. Part 2 addresses specifically the "internal" institutional influences of judicial norms, processes, and leadership on justices' decision making. David O'Brien begins by charting the changing institutional norms regarding the writing of Supreme Court opinions, and he emphasizes the increasing replacement of singular "opinions for the Court" with disparate individual opinions in the last fifty years. Charles Sheldon identifies parallel trends toward "dissensus" in state supreme courts. Sue Davis's essay explores the many facets of leadership by the chief justice beyond opinion assignment from the perspective of both rational choice and interpretive institutionalism; she emphasizes the incongruity rather than continuity of the approaches. The last two essays in the section—by Elizabeth Bussiere concerning Warren Court resistance to recognizing basic welfare rights, and by Ronald Kahn concerning Rehnquist Court constructions of constitutional law regarding abortion and religion—aim to demonstrate how legal doctrine and logics both structure and constrain judicial policymaking options.

Part 3 considers the influence on supreme court justices of "external" institutions and actors: Congress, the president, litigants, lawyers, interest groups, lower courts, and so on. Lawrence Baum's excellent essay explores how the recruitment and selection processes along with other institutional factors end up "freeing" justices to undertake their distinctively legal modes of policymaking. The last four essays then signal a return of sorts to the paradigms introduced earlier. Lee Epstein and Jack Knight expand on their well-known rational choice analysis to demonstrate that increasing amicus curiae advocacy provides critical information to justices regarding how key political actors will respond to various policy decisions. Jeffrey Segal combines concerns of the previous two essays to "test" contending positivist models of decision making, which not surprisingly concludes in a vigorous defense of the attitudinal model against strategic approaches. As if to respond to Segal, Charles Epp marshals data and theory in service of the compelling argument that the Supreme Court's policy agenda is the result of an interaction between the justice's policy preferences and two institutional constraints—the institutionally transmitted responsibility to resolve policy issues that divide the lower courts and the organized "support structure for legal mobilization" by groups, patrons, and cause lawyers. Melinda Gann Hall and Paul Brace end the collection with a plea for developing rational choice analysis of institutional context through explicit comparative studies of state supreme courts as a way to "resolve some of the most perplexing problems in judicial politics scholarship" (p. 282).

Overall, this is an outstanding book. The featured authors are highly accomplished contributors in the field, most (but not all) of them in mid-career, which confirms the generational categorization. The essays are, not surprisingly, generally of very high quality. Indeed, the collection is unusually consistent in terms of sophistication, clarity, and significance. Moreover, most of the essays engage one another to some degree. A notable achievement is that many nicely clarify conceptual differences, politely acknowledge the contributions of rivals, and even make small accommodations in their own approaches. At the same time, neither the editors nor the contributors seek to promote any grand new syntheses. Indeed, a few authors state flatly that the different ap-

proaches are simply irreconcilable. This is probably wise, but it also signals the limits of engagement. What is striking about the debate represented here is the degree to which commitments to sustaining particular paradigms preclude taking seriously criticisms about paradigm limits as a motivation for fundamental conceptual innovation or reconstruction.

The present volume achieves its clearly stated goals quite effectively. It begins with quite traditional questions about how judges make decisions, but the featured contest among perspectives substantially widens the vista of inquiry, introduces a host of new variables, and opens the way for many new secondary questions. Perhaps the most important achievement of the new institutionalism is that its proponents have revitalized concerns about contextual complexity, political conflict, contested power, and normativity in the study of courts. Together with a companion volume by the same editors (*The Supreme Court and American Politics: New Institutionalist Interpretations*, 1999), which locates the Supreme Court even more broadly and provocatively within a divided American polity, Clayton and Gillman have at once successfully represented, elevated, and intensified contemporary scholarly dialogues about judicial politics.

Regulating the National Pastime: Baseball and Antitrust. By Jerold J. Duquette. Westport, CT: Praeger, 1999. 154p. $59.95.

Arthur T. Johnson, *University of Maryland, Baltimore County*

Major league baseball, unlike other professional sports in the United States, has been exempt from antitrust laws for nearly a century. The reason lies with early state and federal court decisions, of which the most frequently cited is the Supreme Court's *Federal Baseball Club of Baltimore* v. *National League* opinion, authored by Justice Oliver Wendell Holmes in 1922. Baseball's legal status has been the subject of numerous law review articles and commentaries, historical narratives, and scholarly analyses. Nevertheless, Jerold Duquette claims that there has been no integrated and comprehensive examination of "baseball's unregulated monopoly."

Duquette employs an "historical institutionalist" method to explain the "persistence of the baseball anomaly." He wishes to explore not only the institutional and ideological but also the political and economic facts that have contributed to it. He uses the conceptual framework of Marc Allen Eisner (*Regulatory Politics in Transition*, 1993). Baseball's status is examined in terms of four periods identified by Eisner: the Progressive era, or the market regime, from 1880 to the late 1920s, when baseball's commercial character went unacknowledged; the New Deal era, or associationalist regime, from the 1920s to the early 1960s, when baseball's unregulated monopoly thrived; a new era of regulation, or the socialist regime, in the 1960s and 1970s, when baseball was severely challenged but still avoided regulation; and the efficiency regime of the 1980s and 1990s, when a shifting consensus on regulatory politics emerged and allowed baseball to remain the only professional sport exempt from antitrust law.

Duquette argues that major league baseball's leaders during the Progressive era "exploited both the cultural significance of the game and the institutional frailties of the federal government in their development of baseball law" (p. 22). During the New Deal, "the stability inside the game together with the instability outside of it both secured major league baseball's standing as the national pastime and insulated it from government interference," which resulted in baseball becoming a "cultural icon" (p. 43).

Baseball had lost icon status by the 1980s, challenged by shifting ideology nationally, a strong leader who led players' unionization efforts, the emerging popularity of other professional sports, nomadic franchises, and the growing influence of television on various aspects of sports. Despite growing fan cynicism in the 1980s, baseball exploited the dominant anti-government regulation philosophy to protect its status, and in the 1990s, despite much turmoil, it successfully coopted the major league players and won the support of the minor leagues (and host cities) as well as their political representatives, with the signing of the Curt Flood Act. This legislation, adopted in 1998, allows players to call upon the antitrust laws, but only after decertifying as a union, and maintains baseball's antitrust exemption on issues related to minor league players, franchise relocations, and the minor leagues.

Duquette concludes that although baseball interests are "succeeding in the political defense of their terrain, they are losing ground on the institutional and ideological fronts where the courts, the Congress, and the country are increasingly willing to reconsider" baseball's status (p. 127). That status, cultural and legal, is no longer what it used to be, and baseball's monopoly faces an uncertain future.

The book is well written and very readable. The author effectively and accurately provides a summary description of baseball's legal and political history, and he offers an insightful analysis of recent legislative events related to baseball. For example, he argues that Holmes's 1922 decision was consistent with Progressive jurisprudence regarding treatment of "incidental" interstate transportation, which is ignored or not recognized by most commentators. Duquette owes this observation, however, to G. Edward White (*Creating the National Pastime: Baseball Transforms Itself*, 1996). Also, Duquette may be the first academic to put into a political and legislative context the role of the minor leagues in organizing support for major league baseball's lobbying efforts on the antitrust issue in the late 1990s. He introduces to the academic community a political analysis, brief as it is, of the Curt Flood Act. Although the book is successful as a brief review of the history of public policy toward baseball, overall the analysis and conclusions are not surprising or different from what already has been written.

At a superficial level, Duquette presents a persuasive argument that "the unique place of the business of baseball in America—its regulatory anomaly—is a function of the game's historical development as encased in the interplay of institutions and ideas, as well as politics ... and a fuller understanding of it is gained by focusing on ... changing regulatory regimes" (p. 135). Yet, one is left wanting more than Duquette provides. At the macro level, the relationships he describes do exist, but does that close the argument? The maxim "correlation is not causality" applies. The argument is incomplete without a more rigorous analysis of events and issues.

Duquette identifies key issues and events, but little detail is given to demonstrate his primary thesis. This is because he builds his analysis primarily from secondary sources. Only a few interviews were conducted, and most were not with the primary figures in the more recent events. Numerous congressional hearings are referenced, but more basic research is needed to support the conclusions that Duquette draws. For example, he discusses the Sisk hearings in 1976 and ties the outcome (no action) to his analysis of the 1980s. He barely addresses what was considered at the time the prime reason for those hearings—the attempt to force major league baseball to place a team in the nation's capital—and he fails to explain in any detail why a successor committee to Congressman Sisk's Select Committee on Professional Sports was never convened. He fails to recognize the use of congres-

sional hearings as theater or bargaining tool. Despite his reliance on congressional hearings as a primary source of information, Duquette ignores other congressional sources. Also, he does not provide sufficient detail to help the reader understand how representatives of the professional sports industry influenced policymakers, whether as "vocal advocates of the progressives' social, cultural and political agenda" or as lobbyists in the 1990s, even though much of his discussion is about that phenomenon.

Absent such details and any quantitative data, the author makes generalizations that are open to question, or at least in need of greater explanation. For example, he places the beginning of fan cynicism in the 1990s (p. 74), whereas many would trace it (as related to baseball) to as early as 1957 (the Dodgers and Giants move to California) or to the 1960s (CBS purchases the New York Yankees, and the Braves move to Atlanta). Duquette refers to the mobilization of fan discontent in the 1990s, when any evidence of an effective organized fan movement at any time is scarce, even Ralph Nader's Fight to Advance the Nation's Sports (FANS) in the 1970s, which survived one year.

Duquette refers to baseball as "big business" throughout the book but fails to provide any financial information relative to other industries. Although this is true today, was it true in the early twentieth century? In the 1950s or 1960s? For example, we are told that baseball "was the only industry of its size and scope that was not subjected to regulation by the federal government" during the Progressive era (p. 21). What were the total revenues of baseball compared to other business of the time? No data are offered to support the economic status of baseball as big business, which is an important question, given the subject of the book.

In sum, Duquette provides at one level a well-crafted argument that offers insight into the historical status of baseball. At a more demanding level of explanation, Duquette misses the opportunity to persuade with detailed analysis and empirical evidence.

Money Matters: Consequences of Campaign Finance Reform in U.S. House Elections. By Robert K. Goidel, Donald A. Gross, and Todd G. Shields. Lanham, MD: Rowman & Littlefield, 1999. 215p. $62.00 cloth, $19.95 paper.

Candice J. Nelson, *American University*

In an era when campaign finance reform is widely discussed by politicians, political scientists, and journalists, *Money Matters* provides a timely and thoughtful analysis of several reform proposals. The central focus is on the effect that spending limits, matching funds, full public funding, and partial public funding with spending limits would have on electoral competition, voter turnout, and voter involvement in elections to the House of Representatives.

A brief chapter introduces the approach of the book, and chapter 2 provides a comprehensive history of the relationship between money and elections from the earliest days of the country. It does an excellent job of showing how the linkages between wealth and campaigning were established and continued to be a thread throughout the nineteenth and twentieth centuries. The chapter also succinctly describes the characteristics of the current campaign finance system and sets the stage for the discussion that follows.

In chapter 3 the authors raise questions that challenge each of the five arguments often advanced by opponents of reform. For example, some say that spending limits are not needed, because not all that much money is spent on politics. This argument is usually advanced by comparing candidate

spending to commercial product advertising. The authors make a credible case as to why choosing a candidate and buying toothpaste differ, but they are less successful in articulating a standard by which to measure how much spending is too much or not enough. Similarly, regarding the argument that reform always fails, the authors point to the Australian ballot and civil service reform as two successes. Opponents would likely counter that such examples hardly point to the success of campaign finance reform specifically.

In chapters 4 through 7, the authors simulate the effect that varying levels of spending limits, matching funds, full public funding, and partial public funding combined with spending limits would have on competition, turnout, and voter information in House elections. By far the most interesting, and the chapter most likely to contribute to the debate surrounding spending limits and public funding, is chapter 4. The simulations assume spending limits, matching funds, and full public funding at levels from $100,000 to $1 million, and the combination of partial public funding and spending limits within the same range. The findings support the argument by opponents of reform that spending limits, without some public funding, would hurt challengers, although not to the degree one might expect. The simulations also suggest that some form of public funding, either through matching funds or full or partial public funding, would help candidates of the minority party, except at the very lowest levels of funding.

Chapters 5 through 7, which examine the effect of the four proposals on voter turnout and voter involvement in the election, find that campaign spending is much less important to turnout than is contact with political parties and campaigns. Spending limits might marginally reduce turnout, and partial or full public funding might marginally increase turnout, but the increases or decreases would be less than 2%. Confirming the earlier work of Steven J. Rosenstone and John Mark Hansen (*Mobilization, Participation, and Democracy in America*, 1993) the authors find that individual turnout is increased through contact with the party or the candidate, not by the amount of money spent. Yet, because the authors look at aggregate candidate spending, not where the dollars go, it is not clear whether certain campaign spending would increase turnout. Their findings suggest that spending on field and get-out-the-vote programs might stimulate turnout, since these programs involve direct contact. Due to filing requirements of the Federal Election Commission, it is not possible for scholars to break down campaign spending into categories, unless campaigns choose to submit detailed information to the commission.

Chapter 7 finds that voter contact with the party or the campaign is more important than spending in a voter's familiarity with incumbents and challengers, placement of the candidates on an ideological spectrum, and concern with the outcome. The authors speculate that in some cases increased spending may in fact blur differences between candidates, but the data does not allow this hypothesis to be explored in any depth.

The last two chapters outline the most common loopholes in the Federal Election Campaign Act—soft money, issue advocacy, independent expenditures, and bundling—that have essentially eviscerated the contribution limits of the act and analyze three degrees of campaign finance reform: minimal, modest, and comprehensive. The authors clearly state the objectives they think reform should meet and evaluate them in terms of the three degrees. For both proponents and opponents of reform, this chapter holds no surprises.

The authors acknowledge the political difficulties in passing comprehensive campaign finance reform—full public

funding, bans on soft money and independent expenditures, and limits on issue advocacy similar to those in an earlier version of the McCain-Feingold bill—but they support such reform and predict that increasing abuses will eventually strengthen pressure for comprehensive change. The authors have a normative bias in favor of reform as defined by good government groups over the years, but this bias generally does not come through in their analysis. For anyone unfamiliar with the issue, this book provides an excellent framework from which to understand the discussion. Its strongest contribution is the analysis of public funding, spending limits, and electoral competition, which provides very specific evidence to support the argument that spending limits and public funding would help level the playing field in House elections. *Money Matters* advances our understanding of the issues surrounding the debate over campaign finance reform.

Passages to the Presidency: From Campaigning to Governing. By Charles O. Jones. Washington, DC: Brookings, 1998. 224p. $39.95 cloth, $16.95 paper.

Ryan J. Barilleaux, *Miami University (Ohio)*

The American political system has many features that set it apart from other governments of the world, but not all are equally apparent. One distinctive aspect is the length and importance of the transition period from one presidential administration to another. In most countries the passage of power occurs almost as soon as the election results are known (consider, e.g., the rapid assumption of power by President Kostunica after Slobodan Milosevič admitted defeat in the September 2000 Yugoslav election), but in the United States roughly ten weeks elapse between the election and inauguration. The American approach, as Charles Jones puts it in this outstanding book, is to transfer power at a "leisurely pace."

That pace is nevertheless deceiving. For the new president and incoming personnel, life is anything but leisurely. Indeed, a common theme among those involved in presidential transitions is their sense of being gripped by urgency and haste: The political clock ticks relentlessly. In an environment dominated by the need to act quickly, there is ample opportunity for trouble.

The importance of the transition period has concerned practitioners and scholars for some time, although many close observers still do not appreciate how influential these ten weeks can be in launching a new administration on the right foot. Not many years ago, a veteran political journalist went so far as to assert that the transition should be shortened to days instead of weeks, as if a change in leadership necessarily improves with speed. Perspectives such as this do not appreciate the differences between the American system and those of other nations, where only a small number of top personnel change after the election of a new leader. In most other democracies, government departments and even much of the executive branch are staffed by civil servants, so few people have to be found to fill these positions. In contrast, our system is distinguished by the large numbers of posts that have to be filled by a new president, ranging from the Executive Office through several layers in numerous departments and agencies. In 1960–61, John Kennedy lamented that he had once thought he knew everyone he needed to know in Washington. Now that he had to fill so many high-level jobs, JFK remarked, it seemed that he did not know anyone. In the decades since then, the magnitude of the problem has only grown.

The presidential transition has been an increasing focus of

scholars in recent years, and their work has tended to follow two paths. One is the concerted effort to document the record of presidential transitions, at least during the past four decades. The purpose is twofold: Discover what happened and determine the activities or strategies that make transition more or less effective. The other path has a didactic purpose: Extract lessons for future presidents to follow in managing their own assumption of power.

Charles Jones follows the dual approach of other authors but makes a unique contribution. As one would expect from the scholar who almost single-handedly restored respect for the constitutionally separated system among students of the presidency, Jones illuminates the subject of presidential transitions in a way that few of his colleagues can match. In addition to the conventional topics—organization, personnel, agenda setting—he introduces other perspectives that are especially valuable for understanding how transitions fit into presidential politics specifically and the American system more generally.

Jones begins with a discussion of transitions and democracy, noting that peaceful transfers of power are at the heart of democratic government: "Representative democracy is about transitions. Citizens are chosen in free elections to become agents ... every election is a time of transition, with first-time winners experiencing the challenge of assuming another's stead and incumbents reading the results as a measure of their performance" (p. 4). Transitions are about something much larger than changing the names on the doors.

Jones interviewed an array of people involved in presidential transitions over the decades he examines. One focus is on "the biggest mistake" made during a transition, or what might be termed "if I knew then what I know now." These mistakes are familiar to those versed in transition studies and often result from the sense of urgency: time and opportunities wasted, misjudged or mishandled appointments, failure to establish a good working relationship with Congress, poor organizational discipline. Jones's summary comment on these mistakes points to the most essential task of transition: "being prepared to complete the shift from campaigning to governing" (p. 176). Presidents-elect and their aides must learn to think in a governing mode rather than a campaigning mode.

At the end of his study, Jones poses an intriguing idea: The transition typical of the post–World War II era may be fading into history. Ruminating on the failures and activities of the Clinton transition in 1992–93, Jones notes that it did not fit the mold of the "conventional transition." Although Clinton and his people could have done many things more effectively, Jones suggests that a more campaign-oriented style of governing may be a permanent feature of the presidency for the foreseeable future. If that is so, then the Clinton case may be something other than a failed conventional transition. Jones finds much to fault in the Clinton transition, as do many of the participants, but he believes future transitions may contain several similar elements, such as use of outside political consultants, interest-group mobilization, and heavy reliance on polling. In the future, the Clinton case may itself be seen as the transition to a newer, campaign-oriented model.

Jones introduces a new concept—voice—that is a major advance in our understanding of how the contemporary presidency operates. As he explains it, "voice features constant monitoring of the interests and concerns of ordinary Americans, sympathetic exposure of these matters in a 'family values' setting, exhortation for a community solution ... liberal use of executive orders and other presidential prerogatives ... and little deference to jurisdictional bound-

aries between public and private or levels of government" (p. 193). Voice not only helps explain the Clinton presidency but also illuminates trends that presidential scholars have sensed but not examined directly.

Few scholars of American government other than Jones could pack as much into a book of this size, and do so in a highly readable way. He provides another major advance in our understanding of the nation's highest office, and he raises issues that cry out for a sequel. The material in his conclusion, on a new model of transitions and the use of "voice," deserve greater attention. Jones and other presidential scholars have their work cut out for them.

Entangling Relations: American Foreign Policy in Its Century. By David A. Lake. Princeton, NJ: Princeton University Press, 1999. 332p. $60.00 cloth, $17.95 paper.

Glenn Hastedt, *James Madison University*

David Lake provides a theoretical framework for understanding the security choices made by the United States in the twentieth century. He grounds his work in the metaphor that polities may be understood as firms producing security. The fundamental choices before states are unilateralism and cooperation. The former is equated with production within a single firm, and the latter can take several forms. Principal among these are alliances, in which polities act as if they were separate and independent firms entering into joint production agreements, and empire, which is similar to the integration that takes place in the modern multidivisional corporation. Alliances and empires form the end points of a continuum of security relationships. Alliances are at the anarchy end, as each polity retains full decision authority. Empire is at the hierarchical end, because one decision maker is dominant over others. Spheres of influence, protectorates, and informal empires are intermediate points.

The choice between unilateralism and cooperation is determined by three primary factors: joint production economies, the expected costs of opportunism, and governance costs. Couched in more political terms, this means that American foreign policy security choices are dependent upon the following considerations. How large are the gains from cooperation? Are partners reliable, and what are the risks of opportunism on their part? How costly are possible security relationships? According to Lake, alliances are most likely to be formed when the gains from cooperation are considerable, there is little risk of opportunism, and only slight costs are entailed in monitoring and enforcing the relationship. Empires are most likely when the gains from cooperation are great but so are the risks of opportunism. Hierarchy reduces these risks. Governance costs under empire are low and increase slowly with the move toward establishing hierarchical relationships.

Lake identifies three eras in twentieth-century American security policy. The interwar years (1919–39) were a period of aborted cooperation and reaffirmed unilateralism. The Cold War (1945–89) and the new world order (1990–present) are defined as periods of successful cooperation. Relatively anarchic cooperation marked many of America's dyadic relationships in these periods, but examples are also given of protectorates and empire. As to the future, Lake identifies two contradictions that need to be managed successfully. First, American foreign policy facilitates cooperation among partners by reducing the risk of opportunism, but at the same time it sets limits on the gains from cooperation by providing a disproportionate share of the forces used in joint military operations. Second, through its leadership the

United States subordinates other states, which limits the burdens they are willing to shoulder and raises the governance costs of cooperation.

Lake indicates that he is not attempting to formulate a new theory of American foreign policy. Instead, rational contracting is an attempt to bridge theories rooted in neorealism, neoliberal institutionalism, and constructivism. Bridges are not easy to construct. Their ultimate success depends upon the strength of their foundation and the ease with which they allow one to move from one theoretical perspective to another. Rational contracting is most closely related to neoliberal institutionalism. An important question for future research is whether Lake's analysis is rooted firmly enough in the other two perspectives to permit easy movement among all three, or is yet another island in international relations theory.

Two types of assessment are needed to determine the strength of its foundation. First, how broad is it? What types of research agendas can it support? Lake notes that a potentially important extension of his study is the examination of international economic policy. Such an extension intuitively is plausible because, as Lake observes, international economic relations range from relatively anarchic (each state retains full control over economic policy) to hierarchic (decision-making power is transferred to another polity). Left unaddressed by this formulation is the longstanding debate over free trade, protectionism, and economic sanctions. These questions are as much about the content of policy choices as they are about unilateral versus cooperative action, but they are central to the study of American foreign policy.

Second, can the methods used to construct this theoretical foundation be applied to other polities? In addition to assessing evidence on the actual choices made by policymakers, Lake examines the surrounding policy debates to see whether policymakers and the public were indeed interested in the types of concerns identified by his theory. This adds texture and richness to his presentation. Yet, with all research strategies that focus on societal conditions, domestic politics, or political culture, the challenge becomes one of replication in settings in which these data are not as readily available.

Bridges are needed in a discipline characterized by islands of research that often do not communicate well. *Entangling Alliances* holds great potential for serving as such a bridge. It is firmly rooted in theory and addresses concerns raised by those who stress both systemic and domestic causes of foreign policy decisions. In addition, the key concepts help us look at longstanding research questions in a new light, such as the reasons for alliance formation.

The Presidency and Domestic Policy: Comparing Leadership Styles, FDR to Clinton. By William W. Lammers and Michael A. Genovese. Washington, DC: CQ Press, 2000. 383p. $28.95 paper.

Peri E. Arnold, *University of Notre Dame*

Can presidency research do more than probe idiosyncratic cases? Our knowledge of the American presidency and the status of presidency studies in political science both hang on the answer to that question. Indeed, the current research agenda in this field exhibits a confidence that its findings are amenable to generalization and deserve the discipline's approbation.

The most prominent contemporary scholarship on the presidency stimulates further theorizing and invites general-

ization. The current concept of presidential behavior emphasizes factors of political context or personal style and character. For example, Stephen Skowronek (*The Politics Presidents Make*, 1993) argues that presidents' possibilities are defined, on the one hand, by their sequence within a regime cycle and, on the other hand, by the developmental state of American politics and institutions in their era. Alternatively, the line of theorizing begun by Neustadt in *Presidential Power* (1960) finds current expression in Fred Greenstein's (*The Presidential Difference*, 2000) examination of executive performance through variations in presidential styles.

Most books aimed at the undergraduate market convey little about the analytic issues in presidency research and do little to advance undergraduates' analytic capacities. *The Presidency and Domestic Policy* is a welcome exception. It was written primarily by William W. Lammers, and Michael A. Genovese took up the task of bringing it to print after his former teacher's death.

The book contains excellent profiles of modern presidents' influence on domestic policy. It also offers a theoretically informed explanation of the leadership performance of these presidents, from Franklin Roosevelt through Bill Clinton. The central argument is that leadership performance in domestic policy is an expression of "the styles and strategies presidents employ in their efforts to govern" (p. 3). They formulate, effect, and implement domestic policy through decision making, administrative work, public leadership, and relations with Congress. Therefore, the way they address tasks in these four dimensions should affect their efficacy in domestic policy. Yet, an assessment of relative efficacy requires an assessment of the domestic policy opportunities present within each context.

Opportunities for domestic policy innovation vary over time. These depend upon a number of contingencies, including winning margin, popularity while in office, electoral fortunes of the president's party in Congress, and budget deficits or surpluses. The authors do not consider broader historical perspectives in conceptualizing the opportunity context, and they might fruitfully have extended their thinking to include work such as Skowronek's or Lowi's (*The Personal President*, 1976).

Lammers and Genovese propose that the opportunity context can be ranked as high, moderate, or low. The first section treats the three high-opportunity presidents: Roosevelt, Johnson, and Reagan; the second covers the moderate-opportunity presidents: Truman, Eisenhower, and Kennedy; and the third examines the four low-opportunity presidents: Nixon, Carter, Bush, and Clinton.

The chapters on individual presidents synthesize existing scholarship and provide an overview of their styles, strategies, and effects on domestic policy in light of their opportunities. Although the profiles are sound and use the salient literatures well, they are constrained by their high level of generality. Theoretically informed case studies would give added weight to suggestions of causal relationships between elements of a president's style and the political consequences of his actions.

The concluding chapter compares the ten modern presidents' domestic policy leadership. Holding opportunity contexts constant by comparing presidents within each of the three categories, the authors observe that "some presidents have used their political opportunities effectively, while others have played their hands rather poorly" (p. 331). They consider Roosevelt the most efficacious domestic policy leader among high-opportunity presidents, Truman among the moderate-opportunity group, and Nixon among the low-

opportunity group. A notable finding is that presidential efficacy seems time bound. Mid-century presidents appear more likely to be efficacious domestic policy leaders than later presidents, whether their opportunity context is high, medium, or low. Lammers and Genovese ask: "Are present-day presidents hampered by new forces in demonstrating their skills and taking advantage of their opportunities?" They conclude: "Presidents serving since 1973 have indeed been affected by a series of limiting influences" (p. 351).

Unavoidably, the focus on domestic policy limits the analysis of presidential leadership. Indeed, the resonance between foreign and domestic policy since 1945 makes it difficult at times to distinguish precisely leadership in the two arenas. Consider, for example, Johnson's hesitancy to choose between "guns and butter" in the mid-1960s. Yet, the authors give the Vietnam War only passing reference in discussing Johnson's domestic policy leadership. To place foreign policy in the background results in a less than full analysis of these presidents' leadership skills or opportunity contexts.

On balance, this book is quite successful. I recommend it for undergraduate course use. More specifically, its pedagogical payoffs are twofold. First, it is an engaging, thoughtful, and well-written narrative of the modern presidents' initiatives and influence on domestic policy. Second, the conceptual focus on presidential performance and leadership style can strengthen the analytic focus of presidency courses.

Pitiful Plaintiffs: Child Welfare Litigation and the Federal Courts. By Susan Gluck Mezey. Pittsburgh: University of Pittsburgh Press, 2000. 209p. $45.00 cloth, $19.95 paper.

— William T. Gormley, Jr., *Georgetown University*

In 1988 the American Civil Liberties Union filed a class action lawsuit against the Illinois Department of Children and Family Services (DCFS), on behalf of B. H., a 17-year-old youth in foster care, and nearly 20,000 other children forced to live outside their home because of abuse and neglect. Attorneys accused the DCFS, responsible for protecting and placing such children, with violations of Illinois statutes and the due process clause of the Fourteenth Amendment. B. H., for example, had been placed in ten different settings despite clear evidence that children require continuity of care for their emotional growth.

In *Pitiful Plaintiffs*, Susan Gluck Mezey focuses on the *B. H. v. Johnson* case in an effort to illuminate child welfare litigation: its origins, evolution, and consequences. The result is a rich and textured case study that sheds considerable light on how the process works. It also reaffirms the old adage that politics, like sausage, should not be viewed in the making by those with weak stomachs or faint hearts. Many Illinois children were uprooted from troubled homes, only to be placed in equally troublesome foster care settings. Although the judicial system responded to their plight, it took years for positive consequences to be discerned.

The *B. H. v. Johnson* case is a particularly good choice for an in-depth analysis. During the 1990s, Illinois had the third highest substitute care population in the United States. Problems with foster care in particular were rampant and acute. The consent decree that ultimately resulted from the B. H. litigation was unprecedented in magnitude among child welfare cases. Moreover, the DCFS was subject to as many as eight separate consent decrees, including the B. H. decree. Clearly, this case involved many interesting legal and political developments and high stakes: It represented nothing less than an effort to restructure one of the largest child welfare systems in the United States.

·To better understand the B. H. case, Mezey interviewed a diverse group of 57 people, including attorneys for both sides, federal district court judges, and juvenile court judges. Many agreed to let at least some of their remarks be quoted, which enabled Mezey to be precise about the sources of numerous observations. In addition to the interviews, Mezey reviewed the relevant case law, not only Illinois cases but also key federal cases in other states and relevant decisions by the Supreme Court.

In constructing and organizing her narrative, Mezey blends approaches developed by Phillip Cooper, Robert Wood, and Wayne Welsh. The resulting analytic framework highlights five phases of litigation: stimulus, accountability, adjudication, implementation, and response. Although these stages are not altogether discrete, and the accountability stage might be more aptly named, the framework is serviceable enough. Like similar frameworks long used in ·the public policy field, this one enables the author to offer a chronological account enriched to some degree by political science concepts.

The most significant contribution of this book is that it highlights the extraordinary difficulty of reforming a child welfare system that can only improve if numerous public. officials respond favorably to interest group pressure. The catch is that a favorable response in one institutional arena does not guarantee a favorable response elsewhere. Moreover, it is difficult to sustain the zeal of a reformist movement, due to personnel turnover in the executive branch and smoldering resentments within the legislative branch. For these reasons, federal district court judge John Grady may have been wise to promote a negotiated settlement rather than a bitter, protracted court trial. Even within the less adversarial setting of a consent decree, mutual recriminations made it difficult to achieve significant progress.

An attractive feature of the book is that it combines legal, political, and policy analysis. The legal analysis is excellent and lucid enough for advanced undergraduates to follow. The political analysis is wide-ranging and instructive, with good insights into ·each branch· of government and occasional references to federal-state relations. The policy analysis is the weakest link of the triad, perhaps because of the paucity of good evidence on which of several factors best explains eventual improvements in the Illinois child welfare system. In chapter 7, Mezey lists several possible explanations, but neither she nor her sources do an effective job of disentangling them.

A weakness of the book is that Mezey's own viewpoint on key questions sometimes remains submerged. For example, in chapter 6, Mezey summarizes the views of the respondents on whether Judge Grady was too timid and whether the monitor he appointed was ineffectual. Here, Mezey is scrupulous about reporting the opinion of respondents but does not directly answer these questions herself. In chapter 8, Mezey offers a glimpse of her thinking on these issues but does not fully present her own point of view.

Another limitation of the book is that it suffers from the usual weaknesses of the case study genre. Mezey.argues that it took interest group litigation to achieve systemic change in Illinois. But is litigation necessarily the best strategy in a state with a more sympathetic legislature or a more competent bureaucracy? On issues such as this, a comparative case study offers striking advantages over the single case.

Throughout the book, Mezey is critical of Donald Horowitz and others who assert that federal judges overextend themselves by playing an aggressive role in state institutional reform cases. She nicely demonstrates that Judge Grady did not exhibit unbridled judicial activism in the B. H. case.

Rather, he prodded the parties to reach a negotiated settlement and avoided the micromanagement that conservative critics decry.

It is less clear that the American Civil Liberties Union chose the right strategy in launching a massive legal assault on the DCFS. In a tantalizing reference to *R. C.* v. *Hornsby*, an Alabama foster care lawsuit with impressive outcomes, Mezey hints that a somewhat narrower focus in Illinois might have yielded swifter progress. Without an in-depth analysis of the Alabama case, however, we will never know for sure.

More broadly, the circumstances under which litigation on behalf of children ought to be undertaken also need to be better defined. For example, we are beginning to see lawsuits filed against day care centers, family day care homes, and resource and referral agencies that refer parents to child care providers. Because children cannot speak for themselves, it is tempting to turn to the courts to seek relief. But even if judges behave with circumspection, the judiciary may not be the best venue for a resolution of these disputes. Judges lack expertise in these matters, as Judge Grady openly admitted. Also, judicial intervention, at best, is a blunt instrument for reform. In the B. H. case, it made possible a significant reallocation of resources to the Illinois child welfare system, but it could not guarantee a change in the culture of the DCFS bureaucracy. Even supporters of the lawsuit concede that it fell short of its stated goals.

Although *Pitiful Plaintiffs* does not answer some important questions, it offers considerable insight into an important lawsuit and its aftermath. It also helps correct an imbalance in the judicial politics literature by focusing on the federal district courts, as opposed .to the Supreme Court or the circuit courts of appeals. The book is well written and tells a compelling story. It is likely to be well received by students in courses on judicial politics, public law, interest groups, and possibly social policy. Although it will not end debates on the appropriate role of federal litigation in reforming state bureaucracies, it does advance our understanding of the underlying issues.

Defending Government: Why Big Government Works. By Max Neiman. Upper Saddle River, NJ: Prentice Hall, 2000. 260p. $27.00 paper.

John C. Pierce, *University of Colorado at Colorado Springs*

Max Neiman provides a concise, well-written, and comprehensive critical analysis of "the conservative attack on the public sector, especially its explanation for and evaluation of the size and growth of the public sector in the United States" (p. viii). In doing so, however, he only partially fulfills what is promised in the subtitle, namely, explaining why big government works. Rather than explicitly assess the reasons for goal achievement in a variety of policy areas, as the title implied to me, Neiman focuses on why we have big government and on the various critiques of that size. To be sure, the book is appropriate for upper division and graduate courses in political science, public policy, or public administration. Indeed, the clear organization of the analysis, the lucid and economical writing, and the political salience of the substance make it accessible ·to good students. But Neiman clearly has a broader audience in mind, including the scholarly community, in both political science and related disciplines. For that audience Neiman also performs a significant service by the broad sweep of the analysis and the precise focus on key elements in the major arguments over big government, both intellectual and political.

Neiman begins by confronting a fundamental point of

contention: Has there been substantial growth in government size? He concedes that growth in absolute dollars "provides some astonishing figures" (p. 21), but he argues that an accurate assessment involves size relative to GNP. "In recent years there has been a decline and then a steadying of the percentage representing government taxing or spending as a proportion of GNP" (p. 24). Neiman then turns to alternative macrodeterminants of government growth, apart from the deeply embedded sociocultural memories produced by the "pain and deprivation" of the Depression (p. 47), which supported an activist government. Those explanations are mostly Marxist in content, and they are more about "why government" than about why it works.

Another set of explanations is found in the public choice literature. "In public choice theory, then, rational, self-interested economic roles prevail in both private markets and government sectors" (p. 69). The economic basis of these theories of government growth is attractive because of its wide applicability—to voters, to groups, to government bureaucracies themselves. Even so, according to Neiman, theorists are not uniform in their conclusions about rational choice and government size. Some suggest rational choice leads inevitably to more government as it competes for supporters in the public, and others (Neiman names Anthony Downs) believe it leads to restraint in government spending.

Neiman looks at the evidence and concludes that war-making and preparing for war "stand out as the premier causes of government growth" (p. 114). The reason is that the threats and dangers attendant to external conflict remove or minimize the consequence of other barriers to growth. Does that explanation fit with the major theories Neiman reviews? The answer is that war is a displacement factor, "supplanting existing notions of what is acceptable in the way of government involvement" (p. 93). Neiman then looks at some of the other dimensions of the dispute about size, such as whether big government is necessarily bad, leading to oppression; he concludes not, claiming that "democracy sometimes requires the public to use the tools of government to liberate individuals from the constraints of bigotry and unfairness" (p. 133).

What does Neiman not consider that he should have? Apart from the need to come to terms with the subtitle, three themes could have received much greater attention: technology, globalization, and social capital. These are not ignored completely but receive scant discussion, even though they are major policy arenas of dispute, both in public forums and scholarly journals. First, the implications of technology for government growth as well as the rapidly increasing penetration of personal lives and economic structures by that revolution deserve more than a paragraph or two (p. 46). Microsoft and Al Gore notwithstanding, the issues of e-commerce, market monopolies, surveillance, electronic democracy, volatile NASDAQ levels, and national security initiatives, among others, all beg for much greater coverage here, even if forced within the theoretical structures Neiman lays out and critiques so carefully.

Second, Neiman touches on the potential effect of globalization, but the review is skimpy (pp. 217–8). The possible implications of that powerful disturbance of economic and political structures for the role of government, both in the United States and elsewhere, should be examined in great detail, especially in the context of the other explanations for government growth covered in the book.

Third, few objects of analysis have received more attention in recent years than the notion of "social capital," broadly seen as the social and political trust-based sets of relationships among individuals that provide them with the foundation to work collectively to achieve shared goals (James S.

Coleman, *Foundations of Social Theory*, 1990; Francis Fukuyama, *Trust: The Social Virtues and the Creation of Prosperity*, 1995; Robert Putnam, *Bowling Alone: The Collapse and Revival of American Community*, 2000). The concept attempts to link economic analysis to social analysis of the relationship between government and the individual. Neiman does not use that particular terminology but is aware of this connection: "The language and tactics of contemporary criticism of public policies and the scope of government [are] designed to underscore the lack of communal objectives and shared interests among Americans" (p. 211). But it also is important to consider the role of social capital in the control of big government, which may exceed the acceptable bounds of its legitimate activity. The trust base of collective activity can both lead to big government and constrain it. Trust is both source and sanction.

Defending Government is strongly recommended to those in search of a clear, concise, and intelligent review of arguments about the growth of government, but it self-consciously originates in a particular political perspective. Even so, scholars and students alike will benefit from a careful reading of this book, as well as from a careful consideration of its implications for a rapidly changing political, social, and economic environment.

Good Advice: Information and Policy Making in the White House. By Daniel E. Ponder. College Station: Texas A&M University Press, 2000. 244p. $39.95.

Andrew Rudalevige, *Dickinson College*

That the amount and nature of the information reaching the president matters for the choices he makes is hardly disputed, but translating this insight into analysis has been slow work. This is true especially in comparison to other subfields (e.g., legislative studies), which in making use of the new institutionalism have stressed information by highlighting the roles institutions play in ameliorating the uncertainty rampant in political decision making. Daniel Ponder's new book, then, is particularly welcome. *Good Advice* asks some critical questions: What did the president know, and how did he come to know it? Equally important, how did that matter?

According to Ponder, presidents do not face a dichotomy between responsiveness (politics) and competence (administration) but a continuum linking the two. They must choose a point along this axis to invest resources for policy formulation in a given field. Should they rely on the White House staff? On the various departments? The investment metaphor (pp. 30f) is used to suggest that presidents select among numerous sources and vast amounts of information, with an eye toward minimizing their risk and maximizing their payoff from any given policy choice.

In theory, presidents may call on any number of advisers. In practice, it is often argued, modern presidents have moved the locus of advice out of the wider bureaucracy and into the White House: they have "centralized" (a term coined in Terry Moe's influential essay, "The Politicized Presidency," in John Chubb and Paul Peterson, eds., *The New Direction in American Politics*, 1985). Ponder is sympathetic (perhaps even at times too generous) to this argument. To his great credit, however, he goes beyond a straightforward application of its logic and seeks to refine its theoretical grounding. Using the institutionalist language of rule-driven behavior (in the broad sense of individual action in a context of formal and informal incentives and constraints), he posits that the White House staff will indeed always play an active role in policy development. Yet, that same emphasis on rules compels the

conclusion that "although policy making is centralized in the White House, it. is not of one kind"·,(p. 35). Because presidential resources, incentives, and constraints, are dynamic—and because policy proposals differ along many dimensions—"the character of centralization differs ... in both degree and kind," even within a single administration (p. .6).

The result is a theory of "staff shift" (p. 6). Ponder suggests that under some circumstances the president will follow a pure centralizing strategy, with the White House staff serving as policy "director." In other cases the president may use the White House as "facilitator," to coordinate but not dominate the work of the departments. In still other cases, policy formulation may be largely delegated to the departments, with the White House serving merely as "monitor" (pp. 35–8). A proposal will succeed or fail depending in part on whether the president has appropriately matched policy to advising process. Again, the question is not responsiveness or competence, but how the two can be most effectively melded.

Ponder explores staff shift by tracing six domestic policy proposals in Jimmy Carter's administration. To maximize the variation in cases, he selects on both the independent·and dependent variables, that is, both on the advisory process and on its degree of success. He chooses two examples each of policy direction, facilitation, and monitoring, one of which is deemed a failure and one a success. A successful advising process does not necessarily produce a "win" in Congress but, rather, a feasible proposal in terms of substance, politics, and presidential preference. Ponder deems Carter's 1980 youth employment plan, 1979 energy plan, and civil service reform successful; the creation of a Department of Education, welfare reform, and national health insurance are denoted failures. He finds some interesting commonalities: For example, failed cases tend to assess inadequately the political landscape, and successful processes provide the president with an effective "honest broker" in substance if not in name.

·The cases are well buttressed by archival material from the Carter Library and are handled deftly. The conscientious exposition of the Carter policymaking process convinces the reader that staff shift is an empirical reality. But this approach, of course, brings its own advantages and disadvantages. On the plus side, Ponder can trace changes in the president's management environment in some detail, and he controls for the vagaries of individual personality that often prompt the "yes, but ..." critique of institutional approaches to the presidency. On the negative side, Ponder cannot explicitly generalize this result to other presidencies. Was the Carter administration different from others? Certainly, it distrusted the bureaucracy less than the presidencies before and after it; Moe, in fact, views Carter as a "pause" in the politicized presidency. Ponder clearly disagrees, but .a broader approach would be needed to cement this point. Furthermore, as he is quick to note (p. 56), can six handpicked cases "prove" the broader patterns of staff choice or that the strategies Carter chose were linked to systematic shifts in the incentives and constraints he faced?

· *Good Advice* should be seen as illustrative, not definitive. Its great contribution is in. moving the theoretical debate forward, infusing presidency studies with information-grounded institutionalism. In this respect it could perhaps be clearer in two areas. First, readers not already familiar with the jargon of new institutionalism may find this part of the discussion (e.g., the distinction, if any, between institution and organization) somewhat opaque. Likewise, Ponder generally presents his assumptions and definitions with admirable clarity, but it is sometimes hard to flesh ,out the set of conditions under which he argues'presidents might appropri-

ately choose one advising structure over another. A careful read bears insightful fruit (e.g., proposals that cross departmental jurisdictions but require a good deal of technical expertise are probably best handled by a mixed, "facilitator" approach); a clearer synthesis of expectations versus findings would make these stand out.·

Overall, *Good Advice* is a valuable addition, well thought out and well researched, to what we know about how presidents obtain and use information and advice. It moves the study of the institutional presidency in a very welcome direction. ·

Public-Private Policy Partnerships. Edited by Pauline Vaillancourt Rosenau. Cambridge,· MA: MIT Press, 2000. 256p. $45.00 cloth, $19.95 paper.

Jonathan P. West, *University of Miami*

Recent calls for public sector reform advocate reinvention, deregulation, reengineering, outsourcing, and privatization to address deficiencies in the classic bureaucratic model and to improve government performance. Reform efforts that seek to capitalize on the advantages offered by 'the three sectors—public, private, and voluntary—include experiments in public-private policy partnerships. Experience with cooperative undertakings between the state and for-profit or third-sector service providers spans the last three decades. This book examines the pros and cons of these public-private policy partnerships, isolates the determinants of success or failure across policy spheres, and identifies the circumstances under which cross-sector partnering should be promoted or avoided.

The book consists of thirteen chapters written by accomplished policy analysts. In the introduction, Stephen Linder and Rosenau effectively present the analytical framework and guiding questions addressed by chapter authors. These essays were published in a special issue of *American Behavioral Scientist* (vol. 43, no. 1, 1999). The contributors offer richly diverse perspectives linked to their various policy focuses and disciplinary specializations. Chapters assess partnerships in health, education, welfare, prisons, criminal justice, the environment, technology, nuclear power, transportation, and public infrastructure. Contributors include specialists in political science, public policy, law, economics, public administration, and public health. The qualitative and quantitative analyses draw on U.S. and Canadian experiences. Collaborative arrangements in various policy arenas range from minimalist to authentic partnering; the former type is most common, but . the latter is becoming more frequent. Rosenau's concluding chapter, rather than advocate or criticize partnerships in the abstract, carefully sifts evidence and integrates material from the policy chapters to evaluate the effectiveness of partnerships. .

Each of the well-written essays contains valuable insights, but some more directly· address the book's central purpose as outlined by the editor. If the book is read selectively rather. than sequentially, I suggest reading the introduction and the chapter by Stephen Linder ("Coming to Terms with the Public-Private Partnership") for a general discussion of the topic and the ,multiple meanings of partnerships, followed with the four chapters by Sheldon·Kamieniecki, David Shafie, and Julie Silvers on the environment, Harry Levin on education, Michael Sparer on health,· and Anne Schneider on prisons. Together, these four most directly and completely examine the various policy choices and forms of public-private collaboration, the evolving roles and tasks of the three

sectors, the criteria for evaluation, and the evidence of partnership successes and failures.

Some conclusions from these four chapters are general and not surprising. Market-based approaches to environmental protection can be efficient and effective, but may fall short in addressing environmental justice and public participation issues (pp. 123–4). Support or opposition for educational vouchers is "premised on ideology and values rather than evidence" (p. 141). Devolution in health policy is "more rhetoric than reality" (p. 157). Prison privatization is driven by value orientations: "a ... conservative, antigovernment, law and order ideology" (p. 207). Rosenau provides a more useful and overarching set of conclusions in the final and most instructive chapter. She offers a balanced, lucid assessment of the strengths and limitations of cross-sector partnerships, summarizing and synthesizing crucial lessons learned from the ten policy chapters. Her criteria include cost and quality performance, equity, access, citizen participation, and democratic processes. She also considers the implications of partnerships for government regulation, accountability, conflicts of interest, cost shifting, managing risk and uncertainty, and the likelihood of success.

Evidence is mixed on key evaluative dimensions, notably cost efficiency, equity, and access. Kamieniecki, Shafie, and Silvers find cost efficiency gains are likely in an environmental partnership program, Levin finds private school voucher programs to be more cost effective, and Schneider identifies slight cost reductions in private prisons; but these gains may be offset by externalities and diminished performance on other criteria (e.g., quality, equity, democracy, accountability). In many instances public-private partnerships increase the costs to society due to additional public spending on monitoring, regulation, assessment, and transaction costs, and these factors seldom are carefully weighed when calculating anticipated partnership costs. Evidence about the efficacy of partnering remains cloudy in some policy areas (see Mark Rom's chapter on welfare services for the poor) and is much more positive in other spheres (development of fuel-efficient vehicles, discussed in James Dunn's analysis of transportation partnerships).

The chapters on environmental protection, health care, and education partnerships suggest that gains in equity through increased choice and competition may involve tradeoffs that adversely affect vulnerable populations. Rosenau correctly observes that in such cases social values and quality considerations should trump cost calculations and profit maximization.

Considerable diversity is found with respect to the influence of intersector partnerships on public participation. In some policy areas (environmental protection) public-private partnerships reduce citizen input in the policy process, but in others, as Nicholas Lovrich notes regarding criminal justice, public/not-for-profit partnerships may be overly receptive to citizen involvement. Walter Rosenbaum's analysis of the lessons learned from experience with commercial nuclear power facilities provides examples of how institutions can be designed to provide a more appropriate balance with stakeholder access, including access to public interest concerns.

Despite considerable strengths, this book shares the deficiency of many edited volumes in that quality is somewhat uneven and a uniform chapter format is lacking. Furthermore, since the bulk of the book was published previously, it is unclear why the material was republished, other than to increase its accessibility. Also, more careful editing would have eliminated multiple, distracting references to "this article" or "this issue."

Aside from these relatively minor criticisms, the book is valuable because it provides fresh, up-to-date, and compelling evidence regarding public-private partnerships, the risks and rewards associated with them, the ingredients affecting their success, and recommendations for their refinement and reform. In this way, it makes a substantial contribution to the ongoing dialogue among policy scholars and practitioners about the prospects and pitfalls of collaboration among the three sectors and charts a direction for future initiatives.

Language Policy and Identity Politics in the United States. By Ronald Schmidt, Sr. Philadelphia: Temple University Press, 2000. 250p. $65.50 cloth, $21.95 paper.

Anna Sampaio, *University of Colorado at Denver*

In the late twentieth century, language conflicts became not only a central battle in public policy but also an essential medium for political expression among traditionally marginalized groups. This is clearly reflected in a host of policy initiatives (from antiimmigrant propositions such as 187 in California, to English-only and antibilingual education movements in such states as Arizona, California, Colorado, and Florida) and the proliferation of studies linking language with Latino and Asian American politics (e.g., Antonia Darder and Rodolfo D. Torres, eds., *The Latino Studies Reader: Culture, Economy and Society*, 1998; and Louis DeSipio and Rodolfo de la Garza, *Making Americans, Remaking America: Immigration and Immigration Policy*, 1998).

Ronald Schmidt provides a timely guide in this historical overview, with a particular focus on the competing discourses among assimilationists and liberal pluralists. Through his "value-critical" approach, Schmidt artfully weaves political history and theory with statistical analyses and policy reviews while centering on three primary themes: (1) educational policy for language minority children, (2) access to civil and political rights and government services by non-English speakers, and (3) the establishment of English as the sole official language of the U.S." (p. 11).

The book begins and ends in much the same place; that is, chapters 1 and 2 review language law and public policy in recent decades, and chapter 8 presents the author's own articulation of a language policy that builds on the "English-plus" initiative, with enhanced immigrant settlement and economic restructuring measures. Along the way, Schmidt provides a thorough examination of dominant theories and discourses in contemporary policy debates (chaps. 2–4); a comparison of these competing theories on foundational questions of justice, equality, and the common good (chaps. 5 and 6); and a critique of each of these philosophical trends (chap. 7). The result (spelled out in chaps. 7 and 8) is a thoughtful and sophisticated defense of a language policy that "will promote justice for language minority group members and facilitate the common good" (p. 180).

Moreover, the author successfully frames this debate in the context of "identity politics" (here defined broadly in both material and symbolic terms), and he uses the policy issues to ask and answer larger questions about immigration and discrimination/racism in U.S. history in terms of citizenship and national unity. This is best articulated in chapter 7: "Here, as we have seen the primary issue is not language per se, but social identities and their relationship to justice and the common good.... Language, then, is an important ingredient in the political conflict, but the central issue motivating that conflict is the nature of the American people and their relationships with one another" (p. 224).

Schmidt achieves remarkable balance, both methodologically and ideologically, without compromising his commit-

ment to defending language minority communities. Regardless of political dispositions, one is drawn into this timely topic by Schmidt's careful discussion of policy initiatives, law, and statistical data in the first few chapters. These lay a clear foundation for more complicated philosophical tensions between assimilationists and pluralists. Moreover, rather than positioning these competing discourses in a simple dichotomy (with all language activists as advocating clearly for one side or another), Schmidt elucidates the range of similarities and differences between assimilationists and pluralists, particularly around questions of public versus private speech, equality, and national unity (see specifically chaps. 5 and 6). As such, this book offers thoughtful insights into foundational questions of political theory, and it is a useful tool for public policy analysts. In using various chapters of the book in my class on Latino politics, I found that students were moved to develop their own positions, beyond simple speculation or political gesticulation, and to grapple with the difficult weaknesses in even their most favored bilingual education programs. Moreover, the thoroughness of the statistical and historical information provided students with the necessary material to defend their opinions.

In addition to thoroughness and balance, Schmidt provides distinct clarity on complicated issues, not only through his accessible writing but also by focusing on the competing politics of language policy rather than endless cases of success/failure in bilingual education programs. Readers come away with a more comprehensive understanding of the larger state and national issues and a clear framework for situating other studies in this field. The author also brings a new perspective by comparing various components of U.S. language policy (e.g., how individuals communicate with the federal government and vice versa) with policy in other parts of the world. To the extent that the book has any shortcomings, one is the limited information provided in these comparisons and about the relationship between U.S. language policy and the changes to federal politics introduced by globalization. The author touches on these topics in chapters 2 and 8, but it is hoped the information will be expanded in future editions.

Ultimately, this research will serve as a compelling intellectual exercise for students, scholars, and policymakers alike. It is highly recommended to anyone interested in understanding more about contemporary language policy and its relationship to issues of immigration, identity politics (with a particular emphasis on Latino politics), and theories of national unity.

Culture Wars and Local Politics. Edited by Elaine B. Sharp. Lawrence: University Press of Kansas, 1999. 250p. $35.00 cloth, $16.95 paper.

Barbara Ferman, *Temple University*

Although political conflict is certainly no stranger to U.S. cities, the contributors to *Culture Wars* suggest that a new kind of conflict, heavily embued with moral overtones, is surfacing with more frequency on the urban landscape. Battles over abortion, gay and lesbian rights, hate crimes, and the like, are taking their place along side the more traditional disputes associated with service delivery, economic development, and redistribution of resources. The morality-based nature of these new culture wars has, according to the contributors, created a new type of politics that is evidenced in the way issues are presented, debated, and resolved. These differences are a function of the passion associated with moral claims, the involvement of religious organizations, and

the use of nonconventional protest tactics that can be fairly aggressive. These differences between how culture wars play out and politics as usual may render existing theories of local politics insufficient.

With this as their vantage point, the contributors to this excellent and thought-provoking collection seek to understand how local governments respond to the challenges implicit in culture wars and the factors that shape and influence the varied responses. Borrowing from earlier research conducted by the editor (Elaine Sharp, "Culture Wars and City Politics: Local Government's Role in Social Conflict," *Urban Affairs Review* 31 [July 1996]: 738–58), the contributors apply a typology of possible government responses: evasion, responsiveness, hyperresponsiveness, entrepreneurial instigation, repression, and unintentional instigation. The two major variables examined are institutional structure (both formal and informal) and political culture. The latter is taken directly from Daniel Elazar's typology of individualistic, traditionalistic, and moralistic cultures (*The American Mosaic: The Impact of Space, Time and Culture on American Politics*, 1994).

The book has two major objectives. The first is to assess the accuracy and usefulness of the above typology of responses, and the second is to examine the roles of institutional structure and political culture in shaping governmental responses. The objectives are pursued through nine case studies that examine morality-based controversies in more than twenty-five cities. These controversies include gay rights legislation, sexual orientation in the schools, hate crimes, abortion conflicts, and needle exchange. As a reality check of sorts, a tenth case study examines how urban policymakers see their role and responsibility in these culture wars.

The case studies reveal the usefulness of the response typology employed (i.e., all were observed in the cases) but suggest the need to add an additional category, "nonresponsiveness." Evasion was the most likely response and was found within all political cultures and across different institutional settings. By contrast, hyperactive responsiveness and unintentional instigation were extremely rare. Variation in formal institutional structure did not have much effect on how cities approached their roles in the controversies. Political culture, by contrast, did play a critical part in shaping governmental responses. Responsiveness and entrepreneurial instigation were associated with the presence of an individualistic culture, whereas repression was more likely to surface in traditionalistic and moralistic cultures.

Culture Wars is an extremely ambitious effort on several fronts. First, comparative case study research that involves numerous contributors is difficult under the best of circumstances. The authors in this collection were operating in largely unchartered territory, since this is the first attempt to examine culture wars across various cities. Second, they were seeking to develop a new framework for examining this relatively recent feature of urban politics. Third, the specific issues in the case studies all fall under the general heading of "culture wars," but there was significant variation in the nature of the issues, their developmental history, and the constituencies they attracted. Despite these challenges, the book presents a coherent body of analysis, breaking new ground in urban theory and comparative case study examination. As such, it makes several major contributions to the literature.

First, this is the first systematic look at culture wars at the local level. As such, the cases and the analytical chapters demonstrate that these conflicts are not isolated fringe phenomena that occur sporadically but are recurring phenomena that require systematic investigation. In pioneering

such an approach, the book provides subsequent scholars with a firm foundation, promising methods, several interesting hypotheses, and strong analytical frameworks. This work will no doubt lead to many follow-up studies.

Second, the volume broadens urban analysis to include social movement theory. Given the preoccupation of social theory with collective action issues, group dynamics, and coalition formation, it has much to offer urban scholarship.

Third, the authors provide a necessary corrective to the traditional "dependent city" perspective. The cases reveal the reciprocal relationships between local activities—including those of local governments and their officials—and state and federal activities. The San Francisco case, in particular, in which local government policy forced corporate giants to change some of their practices vis-à-vis domestic partner benefits, brings this message home loud and clear.

Fourth, the book promotes the idea that culture wars constitute a fourth arena of city politics (the traditional three are developmental, allocational, and redistributive). There are enough similarities between the political and institutional dynamics surrounding culture wars and those surrounding the other three arenas to make the case that culture wars are definitely within the realm of local politics. At the same time, the differences are significant enough to warrant a separate category.

Finally, the reader is forced to think about, in a normative way, the role of moral issues in local politics. Although this was not a primary objective of the contributors, there is no escaping the fact that moral issues are becoming increasingly enmeshed in political discourse and activities. The continuing activities of protest groups, such as those chronicled in this fine collection, combined with the fallout from the Charitable Choice Act will ensure that politics and morality continue their uneasy dance. The cases in this book provide many angles from which to view the intersection of politics and morality and much rich detail in which to root the larger perspectives. This excellent book should appeal to anyone interested in the issues that comprise the new culture wars, as well as to students of conflict theory, social movements, urban politics, public management, and public administration.

Civic Engagement in American Democracy. Edited by Theda Skocpol and Morris P. Fiorina. Washington, DC: Brookings, 1999. 528p. $52.95 cloth, $19.95 paper.

M. Margaret Conway, *University of Florida*

Concern about the health of civic life in the United States has generated academic debate, journalist commentaries, several study commissions, and publications by the score. Is civil society in decline, or is it reinventing itself? That is the question addressed from a variety of perspectives by the sociologists and political scientists who contributed to this volume. The chapters, initially prepared for presentation at a conference in September 1997, examine issues at the center of the debate from several theoretical perspectives. The editors organize the contributions under three headings: the roots of civic engagement, civic life in a changing society, and the ironies of contemporary activism.

In an introductory essay, Skocpol and Fiorina review three theories that posit alternative causes for ongoing transformations in civic life: the social capital approach, which emphasizes socialization into the norms, networks, and cooperative actions seen as necessary for solving social problems; the rational choice approach, which focuses on incentives for individual action; and the historical-institutional approach,

which emphasizes changing organizational patterns, the resources available for collective action, and relationships between elites and the mass public.

Whereas Robert Putnam emphasizes the role of social trust in fostering democracy, Skocpol points out that the creation and evolution of democratic regimes also is fostered by conflict and distrust. Research in this volume makes clear that scholars who use different theoretical perspectives may reach differing conclusions about how voluntary associations affect civic life, social capital formation, and the operations of political institutions.

A core issue in the social capital debate is whether the associational life of American communities, so central to Putnam's argument, has increased or decreased over time or changed in other ways that significantly influence social capital formation and its effects on civic engagement. Several contributors address that issue. Focusing on the community level, Peter Dobkin Hill analyzes trends in the population of organizations from 1850 to 1998 in New Haven, Connecticut, and the implications of organizational change for patterns of civic engagement. Chapters by Skocpol and by Elisabeth Clemen indicate that women's voluntary associations have had significant effects on social welfare policy. As Skocpol points out, this contrasts to Putnam's conclusion that these associations had few effects on policies during the Progressive era. Clemen provides an historical analysis of the role of women's groups in the transformation of American politics between 1890 and 1920. She reveals their role in political mobilization and shows how their structure and internal procedures affected both external perceptions of the organizations and patterns of interactions among them. Multiple models of organizations—an "organizational repertoire"— enabled challengers of the established political order to employ nonpolitical models of organization.

Jeffrey Berry addresses post–World War II patterns of citizen advocacy groups through an examination of their participation in congressional hearings on domestic social and economic policy as well as media coverage of group activity. His research suggests that growing membership in groups based in Washington reflects a shift from local voluntary organizations to national groups that focus on policy solutions at the national level. Because such membership often entails little or no activity other than writing a check, the creation of social capital may be weakened.

Several chapters evaluate long-term changes in American society and their consequences for voluntary associations and civic engagement. Steven Brint and Charles Levy consider cultural and organizational changes among professionals. Susan Crawford and Peggy Levitt use the Parent Teachers Association for a case study, Marcella Ridlen Ray discusses the effects of changes in communications technology for group formation, and Robert Wuthrow explores the effects of religious involvement on patterns of civic engagement. Skocpol examines change over time in the universe of voluntary associations, with a focus on the withering of national membership federations and the development and growth of advocacy groups arising from the social movements of the 1960s. She attributes alterations to changes in the political opportunity structure, new methods and models for building and maintaining organizations, shifts in social class relationships, and evolving race relations and gender roles.

An alternative approach for examining social capital formation is provided by Wendy Rahn, John Brehm, and Neil Carlson. They use survey data to examine how social capital may be generated through participation in a national election.

Patterns of civic engagement are generally assessed as a

positive contribution to society, but negative consequences may flow from political participation. Fiorina points out possible negative consequences from activism by extremists. Kay Schlozman, Sidney Verba, and Henry Brady examine inequalities in civic participation and consider the biases that these bring to the political system.

This book is recommended reading for scholars interested in the social capital controversy. It will stimulate further research on change over time in patterns of civic life and the consequences for social capital formation and civic engagement. Also, it illustrates the advantages of approaching research problems from a variety of theoretical perspectives.

It's Our Military, Too!: Women and the U.S. Military. Edited by Judith Hicks Stiehm. Philadelphia, PA: Temple University Press, 1996. 309p. $69.95 cloth, $22.95 paper.

Francine D'Amico, *SUNY Cortland and Syracuse University*

This anthology opens with several compelling first-person narratives of life inside the U.S. military institution, followed by analyses by both military and civilian researchers. The academic contributors approach their topics from different disciplines, including political science, history, sociology, and literature/film. The volume seeks to bridge the gap between those inside the institution and those on the outside (p. ix). The effort to build this bridge began with a series of specialized conferences and workshops on gender and military culture spanning the 1990s. I participated in several of these and heard some of the contributors to this collection present their research. Other volumes that developed from the conferences are Mary Fainsod Katzenstein's *Faithful and Fearless* (1998), Mary Fainsod Katzenstein and Judith Reppy, *Beyond Zero Tolerance* (1999), and Francine D'Amico and Laurie Weinstein, *Gender Camouflage* (1999).

It's Our Military, Too! is aimed at readers new to the study of U.S. military culture and policy. All save one of the chapters are original publications. In the opening chapter, Army Lt. Rhonda Cornum criticizes the U.S. media for its obsessive focus on her brief experience as a POW during the Gulf War; she prefers to assess that experience in the context of her 13-year military career (pp. 3–23). Although this contribution provides a compelling hook, especially for the student reader, questions raised in succeeding chapters about women's efforts to "fit" into a resistant institutional culture should prompt the reader to reassess Cornum's uncritical essay on military service. Using pseudonyms to protect their careers, two active duty officers critique the military's hetero/sexist culture, Virginia Solms through the frame of her experiences as a lesbian Army officer, and Billie Mitchell through an analysis of cadet training at West Point.

The second part of the volume focuses on history and contemporary policy issues. In a meticulously detailed history of military nursing from 1775 to the Vietnam War, retired Army officer Connie L. Reeves argues that military nurses' struggle for acceptance was the vanguard for women's entry to other military occupations. Sociologist Brenda Moore (also a veteran) uncovers the history of African American WAAC/WACs who served overseas during World War II and analyzes contemporary military race/gender relations, expanding upon her earlier work (*To Serve My Country, To Serve My Race*, 1998). Disappointingly, many of the other contributors neglect the race/gender dynamic Moore so brilliantly exposes.

Advanced readers and policy researchers will no doubt find the chapter on gender and weapons design, by Department of Defense program analyst Nina Richman-Loo and Cornell

researcher Rachel Weber, most intriguing. They analyze the role technology plays in facilitating or limiting women's military service through a case study of the Navy/Air Force Joint Primary Aircraft Training System (JPATS) and its proposed redesign of the next generation of aircraft cockpits (p. 136). Beginning students will appreciate the concise and organized brief on changing military mission and personnel models by sociologist and Vietnam veteran M. C. Devilbiss, but advanced readers should also benefit from her insights.

Chapters in the final section of the anthology consider how society's ideas about gender are reflected in and sustained by military traditions and popular representations of war. Carol Burke explores the "pernicious cohesion" sustained by gendered folk traditions at Annapolis, where she taught from 1984 to 1991. Susan Jeffords analyzes the telling of "the war story" in American popular culture, and Miriam Cooke examines gender and military paradigms through a case study of the photographic framing of the Lebanese civil war. Along with Billie Mitchell's chapter on West Point, these are the theoretically richest contributions in the volume.

This text will be useful for a range of introductory college courses on contemporary social and political issues and perhaps also for high school social studies courses that address citizenship. Three dozen black-and-white photographs illustrating nearly every chapter are sure to engage student attention. Chapter notes and bibliographies and a cumulative index provide useful tools for academic readers.

Unfortunately, the editor provides no substantive introduction to identify the different projects and perspectives of the contributors or to offer theoretical threads to help make connections across the chapters. This omission may have been intentional, so as not to force the very different contributions to bend to a predetermined framework, but more might have been done to prepare the reader for what lies ahead. For example, what central questions shape or inform the collection? The brief preface poses none. Stiehm merely decries civilian inattentiveness/deference to the military and challenges the civilian reader to accept responsibility "for what it is and what it does" (p. ix). She elaborates on this obligation in the final chapter, "The Civilian Mind," but does not engage the theoretical literature on democratic citizenship.

Also missing is a discussion of how this text fits with other literature on military history, sociology, and policy or gender theory/politics. The literature has grown so vast and diverse that perhaps such a review would have been superficial at best to cover the breadth of disciplines represented. Still, locating the text within the narrower field of military gender studies would have been useful not only for student readers but also for instructors considering the volume for course adoption.

The anthology is something of a hybrid, a cross between a textbook and a citizen handbook. A chapter on rank structure and insignia provides a laundry list of "facts" about the military institution. This may be useful information for beginning readers, but these "facts" require greater organization, contextualization, and discussion for that audience. For example, how does the persistence of four service branches with different uniforms, insignia, organizational structures, and cultural traditions affect gender integration? Would a restructuring such as occurred recently in the Canadian military remove some of the obstacles to women's participation that contributors describe? Student readers will need additional guidance to understand the unspoken connections among chapters and across the three sections of the book.

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The Movers and the Shirkers: Representatives and Ideologues in the Senate. By Eric M. Uslaner. Ann Arbor: University of Michigan Press, 1999. 218p. $44.50.

William Bianco, *Pennsylvania State University*

The Movers and the Shirkers is a critique and extension of a well-cited and important research program: attempts to measure the degree to which legislators shirk, or advance their own policy goals at the expense of those held by their constituents. Such analyses (e.g., Joseph P. Kalt and Mark Zupan, "Capture and Ideology in the Economic Theory of Politics," *American Economic Review* 74 [June 1984]: 279–300; John R. Lott, "Political Cheating," *Public Choice* 52 [1987]: 169–86) typically assume a principal-agent relationship between constituents and elected representatives, and they specify a regression analysis with roll-call behavior as a left-hand side variable and various measures of constituency interests and legislator ideology as right-hand side variables. Previous work (John E. Jackson and John W. Kingdon, "Ideology, Interest Groups, and Legislative Votes," *American Journal of Political Science* 36 [August 1992]: 805–23) shows that these analyses are bedeviled by measurement and estimation issues. Eric Uslaner highlights a more fundamental flaw: By ignoring important and well-understood mechanisms that tie legislators to their constituents, these analyses assume what should be tested.

Uslaner's argument will ring true to anyone familiar with the political science literature on representation. Rather than emphasize regular elections as the only mechanism that compels incumbents to behave as their constituents demand, Uslaner focuses on recruitment—the decision to run for office. Simply put, potential candidates who would face large incentives to shirk, because their policy concerns are crosswise to those held by constituents, generally decide against running because they assume, correctly, that they would have little chance of winning. The result is that a sizable fraction of incumbents are well matched to their constituencies, so shirking is not an option. In the main, doing right by their constituents furthers their own policy concerns.

Uslaner also argues for a more nuanced definition of the constituency in models of shirking. As he notes, it would be no surprise to find that when disagreements exist, legislators are more responsive to the preferences of their reelection constituency over the interests of their entire district. Thus, apparent shirking—a mismatch between a legislator's behavior and constituent interests broadly defined—may reflect the essence of democracy: incumbents who try to hold the support of the people who elected them.

The core of the book is a reanalysis of the Kalt-Zupan data on voting in the U.S. Senate. Uslaner faces a classic dilemma: Use suspect data in order to facilitate a comparison with well-cited findings, or use better data and abandon comparability. In the main, he opts for comparability, which is a defensible choice, although my preference would have been to emphasize Poole-Rosenthal NOMINATE scores as an alternate measure of legislator ideology. Yet, Uslaner moves beyond contemporary analyses of shirking by estimating a multiple-equation model of roll-call behavior, and he also exploits some additional sources, including a CBS poll of Senate incumbents.

Uslaner finds little systematic shirking. When it does occur, it is more directed at satisfying reelection constituencies than at furthering an incumbent's policy goals at the expense of constituents. Moreover, the policy preferences of Senate incumbents are strongly related to those of the people who elected them—their geographic and reelection constituencies. The point is not that Senate incumbents are well

controled; rather, to paraphrase John Kingdon, they just reflect where they came from.

Uslaner's results do not preclude shirking on proposals of little interest to constituents, or isolated, idiosyncratic shirking that is masked by aggregate measures. What the book confirms is that contemporary analyses of shirking, by largely ignoring recruitment, overlook a critical mechanism that binds legislators to their electorate.

An additional strength of this book is the author's self-consciousness about method. The analysis chapters emphasize how Uslaner constructed measures and arrived at specifications, as well as the effect of alternate variables and models. In this sense, the book would be ideal for discussions in a graduate methods class. Readers may disagree with some of Uslaner's assumptions, but these areas of disagreement are easy to find.

In sum, *The Movers and the Shirkers* is an important correction to contemporary studies of representation. Uslaner moves the debate away from expectations based on simplistic principal-agent models and toward a more realistic specification of the ties that bind incumbents to constituencies.

Choosing Equality: School Choice, the Constitution, and Civil Society. By Joseph P. Viteritti. Washington, DC: Brookings, 1999. 284p. $29.95.

Jerome J. Hanus, *American University*

The contours of the school choice debate are by now familiar to public policy students, but a lack of agreement about the appropriate weights to be given to the variables affecting the subject continues to splinter their ranks. On the surface, there appears to be a consensus that the latest scores on standardized tests will resolve the uncertainty as to which type of education, public or private, is most effective, but a dip into the literature quickly dispels any such hope. The only thing clear is that nonpublic schools, even with one financial hand tied behind them, do not perform any more poorly than public ones. Consequently, Viteritti wisely gives short shrift to the byzantine methodological distinctions made by researchers and, instead, focuses on the normative questions.

Unlike most writers who favor school choice (e.g., Milton Friedman, *Capitalism and Freedom*, 1962) because it will contribute to an increased dimension of individual freedom, Viteritti justifies it because its outcome will increase equality among classes and races. In a comment reminiscent of John Rawls (*A Theory of Justice*, 1971), Viteritti argues that "education policy must be designed to benefit the most disadvantaged members of society—those who are underserved by the current system" (p. 211). He does not sharply distinguish between equality of opportunity and equality of result because he expects the former to evolve straightforwardly into the latter. If lower income families in the inner cities can choose to attend a school other than their local public one, then they will have a greater probability of attaining the same quality education as middle-class children. This would help fulfill *Brown* v. *Board of Education*'s desire that all races receive not just an education, but a decent one.

Viteritti focuses on three particular issues in the school choice debate: whether lower income parents are competent to choose among schools, whether aid to religious schools is constitutionally permissible, and whether school choice can contribute to greater equality in America. He quickly dispatches the first issue by recounting the experiences with school choice (both public and private) in Harlem, Milwaukee, Cleveland, and several other cities. In all of them,

despite onerous legal restrictions imposed by the state, substantial numbers of low-income parents did make a conscious decision to opt for a charter, magnet, or private school. Of course, most simply stayed in the local public school, either because they were satisfied with it or, perhaps, because they lacked the initiative to leave. In either case, experience has disproved the charge that low-income parents are incapable of distinguishing among schools or are unconcerned with the education of their children.

The constitutional question is more complex because it involves both federal and state constitutions and because the Supreme Court has unnecessarily complicated the. issue. Viteritti rightly zeroes in on *Everson* v. *Board of Education* as the culprit that distorted the legal and social history of religion in the United States and introduced thirty years of textual incoherence into the law of church-state relations. By defining religion to include both "religion and non-religion," the Court left itself open to the charge that when it required that government aid must support a secular (i.e., nonreligious) purpose it was promoting a secularist religion over sectarian religion. Beginning in the 1970s, the Court has taken a more accommodationist position, but it has done so more on public policy grounds rather than on constitutional principle, thus resting the law on the current political orientation of its members.

An important contribution to the church-state question is Viteritti's analysis of the stringent state "Blaine amendments." He does a public service in describing their anti-Catholic origins, direct and indirect, and points out that if the Court were logical, it would strike these down because they violate free exercise of religion by reason of their unconstitutional intent. To see the import of his reasoning, one need only compare the Court's decisions to strike down laws intended to discriminate against racial minorities with those intended to discriminate against religious minorities.

The third issue in the school choice debate, and the one that most interests Viteritti, is the opportunity it offers low-income pupils to participate effectively in America's dedication to providing a decent education for all. Observing America's commitment to freedom of choice in so many facets of life, Viteritti finds it anomalous that the core government institution—education—is the least pluralistic and the least likely to acknowledge the importance of religion in people's lives. This is even more the case with regard to racial minorities, because black churches have historically played such a crucial role in unifying their members and offering leadership for social advancement. As he observes, "the church is a special source of strength in poor communities" (p. 208). For these groups, an education devoid of a proper concern for their religious heritage and respect for their religious values and teachings will not provide these children with the sense of empowerment necessary for alleviating their political inequality. Thus, Viteritti draws together the elements of democratic freedom of choice, religious instruction, and social (and spiritual) capital to justify offering a plurality of educational options to the poor.

An argument for equal government funding for poor children to attend any kind of accredited school—public, charter, magnet, private, religious—has much to be said for it, and Viteritti deals effectively with the most frequently raised objections. Nevertheless, his philosophical commitment to equality does give rise to some misgiving. Education in America has ideally been assumed to be directed toward the individual student, not racial, social, or income groups. Critics of school choice often object to it because "the rich" might receive a government subsidy if they opt for a private school. Despite the fact that most private school pupils come

from middle- and lower-middle-class families, Viteritti is inclined to accept this form of class analysis. Instead of offering a justification for increasing freedom for all parents, he would restrict it to the currently poor and, presumably, would take it away from those who manage to work their way into higher income levels. Thus, Viteritti's argument for school choice will still result in a certain amount of exclusiveness and bureaucratic entanglement, as the state must determine each year who is and who is not still poor enough to qualify for school choice. Because Milton Friedman avoids this rather distasteful prospect by including all parents in a policy of school choice, he provides a much more compelling intellectual foundation for creating a nation of thriving educational communities.

The Politics of the Minimum Wage. By Jerold Waltman. Urbana: University of Illinois Press, 2000. 172p. $24.95.

Byron W. Daynes, *Brigham Young University*

Upon receiving Jerald Waltman's book, the first thing I read was the dedication to the author's "coworkers at A. & W. Root Beer, Ruston, Louisiana, 1963–67." I like root beer, but I wondered whether a mistake had been made in sending me this book to review. It did not take long to realize no mistake had been made. This is a book that treats minimum wage as symbolic politics, but, more important, it entirely reexamines public policy.

Waltman begins by crediting Theodore J. Lowi with originating public policy research by identifying four basic policy types—distributive, regulatory, redistributive, and constituent policy—and alluding to the distinctive political process that attends each (Theodore J. Lowi, "American Business, Public Policy, Case Studies, and Political Theory," *World Politics* 16 [July 1964]: 677–715, and "Four Systems of Policy, Politics, and Choice," *Public Administration Review* 32 [July/August 1972]: 298–310). It soon becomes clear, however, that Waltman is disturbed by Lowi's definition of public policy, which views coercion as the core identifying ingredient (Theodore J. Lowi, "Foreword: New Dimensions in Policy and Politics," in Raymond Tatalovich and Byron W. Daynes, eds., *Social Regulatory Policy: Moral Controversies in American Politics*, 1988, p. x).

Waltman believes that a "more interesting theory can be generated" if public policy is defined as "any statute or administrative act adopted by the appropriate legal authority" (p. 4). This definition allows Waltman to separate policy into coercive and noncoercive categories. The first encompasses distributive, regulatory, and redistributive policies, and the second includes Lowi's "constituent" policies, James L. Anderson's "suasive" policies ("Governmental Suasion: Refocusing the Lowi Policy Typology," *Policy Studies Journal* 25 [Summer 1997]: 266–82), and Waltman's own "symbolic" policies (p. 4). Thus, coercion becomes the organizing variable Waltman uses to construct an entirely new definition of policy, and his revised typology is a primary contribution to the literature.

The remainder of the book explores all possible perspectives of minimum wage as symbolic politics: ideological roots, political history, public support, sociology, place in the policy process, and economic consequences. In chapter 1, Waltman looks at the roots of minimum wage in Progressivism and at the ideological cleavages it has caused. The political history examined in chapter 2, which begins with the New Deal and ends with 1996, shows convincingly how much more sympathetic Democratic administrations have been to the issue than have Republican administrations. Waltman argues that

the Supreme Court and Congress have vacillated over time in their support of minimum wage.

In chapters 3 and 4, the author reveals the importance of public support to the success of minimum wage politics, even though public opinion has been ambivalent over the years. Also, unlike some other issues, minimum wage appears constituency specific, of interest particularly among women, African Americans, Hispanics, and young people.

In the chapter on policymaking (chap. 5), Waltman introduces a new agenda-setting model, characterized by "instigators," who introduce policy, and the more visible policy "enablers," who facilitate the policy process. Since public support for minimum wage is so uncertain, the issue does not always make it to the formal policy agenda but remains viable enough to secure a place on the less visible political agenda (p. 107).

Waltman does not see minimum wage as having serious economic consequences on employment, inflation, business failures, or the reduction of poverty, but such concerns remain significant enough to influence debate.

In his concluding section, which ought to have appeared as a separate treatise, Waltman attempts to link the importance of work to citizenship in a discussion of what he calls "civil republicanism" (pp. 135–45). This was not a satisfying conclusion for an otherwise thought-provoking treatment of policy theory. Of course, this evaluation could be due to my own preferences for policy theory.

Waltman's argument forces us to choose between his own policy perspective and Lowi's framework. His treatment goes far beyond my own experience with the Lowi policy scheme. When Ray Tatalovich and I proposed that social regulatory policy should be a distinct policy type, Lowi labeled our attempt to add a fifth category "valiant" but indicated he preferred to subsume our efforts into his scheme, creating an enhanced version of his model that would still preserve the "simplicity and, more importantly, the logic of the analysis" (Tatalovich and Daynes, eds., *Social Regulatory Policy*, p. xii).

In contrast, Waltman alters the way of looking at policy, as he both rejects Lowi's definition of policy and disassembles his fourfold scheme, even in its more elaborate format. I would like more extensive evidence as to why the rejection of Lowi and the acceptance of Waltman would enhance my understanding of policy. Although Waltman's argument is fairly tightly drawn, it is ultimately less convincing than Lowi's assertions in support of his framework because Waltman does not fully explain how an economic regulatory policy such as minimum wage can be totally divested of all its coercive elements, despite the symbolism that defines responses to it (p. 1). But even if one does not subscribe to Waltman's ambitious policy revision, the questions he asked are important.

Checkbook Democracy: How Money Corrupts Political Campaigns. By Darrell M. West. Boston, MA: Northeastern University Press, 2000. 220p. $47.50 cloth, $16.95 paper.

David A. Breaux, *Mississippi State University*

Darrell West argues that elections, rather than stimulate debate over ideas and policies, have degenerated into contests in which candidates try to raise more and more money. The cost of campaigns has skyrocketed, the level of cynicism and apathy among voters has increased, and the perceived accountability of elected officials has been placed at risk.

Checkbook Democracy begins with a discussion of how the abuses of Watergate helped spawn the Federal Election Campaign Act of 1974. Its key features were strong disclosure

laws designed to end secrecy in fundraising; caps on contributions at $1,000 per individual and $5,000 per political action committee; voluntary spending limits for presidential races in exchange for public subsidies; and the creation of a new federal agency, the Federal Election Commission (FEC), which was charged with enforcing the new law. As a result of these provisions, the ability of wealthy individuals to fund individual candidates was sharply curtailed. In the subsequent chapters, West provides several case studies that illustrate how the quest for more and more money has led candidates, political parties, and interest group organizations to take advantage of loopholes in campaign finance laws.

Chapter 2 examines the independent expenditures that funded the Willie Horton advertisement during the 1988 presidential contest. Although the FEC collected extensive evidence that documented the Bush campaign's role, the FEC lawyers were not able to persuade the courts. This set the tone for the flood of independent expenditures in subsequent elections. Chapter 3 focuses on the rise of issue advocacy and details how the Christian Action Network spent $2 million on advertisements to defeat Bill Clinton, whom they saw as undermining traditional family values by advocating "radical homosexual rights."

Chapter 4 looks at what West calls the "schizophrenia" of current campaign finance rules. The focus is on the case of Simon Fireman. He and his company, Aqua-Leisure, were fined $6 million for making contributions above the $1,000 limit to Republican presidential candidate Bob Dole. At the same time, individuals, unions, and corporations donated hundreds of thousands of dollars to political parties, but because these funds were not given directly to candidates, they were exempt from campaign finance restrictions.

Chapter 5 examines the role of donations from foreign nations to the Democratic National Committee in 1996. This was an embarrassing scandal for the Democratic Party and raised serious issues of national security. Chapter 6, which illustrates how powerful groups can help candidates evade campaign finance rules, looks at the role of the Teamsters Union in Clinton's 1996 reelection campaign.

Chapter 7 examines how tobacco companies helped defeat policy proposals that had the overwhelming support of the American public by waging a $43 million lobbying campaign and making large donations to the Republican National Committee. Chapter 8 explores how candidates rely on nonprofit, tax-exempt organizations to help them evade campaign finance law. These organizations are not subject to disclosure requirements or contribution limits and can accept money from anyone who desires to contribute.

The concluding chapter notes the various forces working against serious campaign finance reform. Thirteen reforms are suggested (including on-line weekly disclosure reports, a ban on soft money, an increase in contribution limits, and restructuring of the FEC) that would bring about open, fair, and equitable elections.

Checkbook Democracy is a lively read because of anecdotal information on specific instances of campaign finance abuses, but the effect of money on campaigns and elections is an extremely complex issue, and West does not provide a rigorous analysis. His claim that the increase in voter cynicism and apathy is a product of campaign finance abuses is not supported by evidence and therefore is mere speculation. Although West puts forth some intriguing prescriptions for campaign finance reform, his suggestions are not only untested but also politically unviable.

This is an easy-to-read book concerning specific abuses of the campaign finance system and how these undermine the electoral process. For more than anecdotal information,

however, one needs to look elsewhere, such as David Magleby, ed., *Outside Money: Soft Money and Issue Advocacy in the 1998 Congressional Elections* (1999). The contributors to *Outside Money* employ a common method that allows them to present and analyze hard data on actual spending by parties and groups.

The Market Approach to Education: An Analysis of America's First Voucher Program. By John F. Witte. Princeton, NJ: Princeton University Press, 2000. 221p. $29.95.

Michael Mintrom, *Michigan State University*

John F. Witte has been prominent in the academic discourse surrounding market-like reforms of public education. In the two-volume *Choice and Control in American Education* (1990), Witte and his coeditor, William S. Clune, assembled an impressive collection of analytical essays and literature reviews that highlighted relevant theoretical concerns and showcased the insights from practice available at the time. In September 1990, Witte was appointed the state's evaluator of the pioneering Milwaukee, Wisconsin, voucher program. From 1991 through 1995, he and his collaborators produced annual reports analyzing key aspects of the program. The salience of school choice as a policy idea and the political interests at stake inevitably meant that all eyes were on Milwaukee, and the question in everyone's mind was "does it work"?

Although Witte found much to be commended about the program, he uncovered little evidence that voucher students in private schools were doing better with respect to test score gains than a control group in the city's public schools. Others disputed those findings. In particular, in a reanalysis of Witte's data, Paul E. Peterson and colleagues suggested that, when compared to a theoretically more appropriate control group, voucher students were making significant gains. The Peterson study went to unusual lengths to discredit Witte's work, and what might have been a productive scholarly exchange turned sour. Witte published many scholarly articles on school choice issues throughout the 1990s, typically grounding his arguments in evidence he had gathered in Milwaukee.

The Market Approach to Education is the capstone of Witte's effort to make sense of Milwaukee's voucher program, not only its internal dynamics and local outcomes but also, and more important, its broader political significance. The result is impressive. This is undoubtedly the best analysis of the details of school choice and its social implications since Jeffrey R. Henig's *Rethinking School Choice: Limits of the Market Metaphor* (1994). Both books are essential reading for those interested in this issue. Witte's book is fascinating for two reasons. First, as one would expect, it provides many vital insights into the major debate taking place over the future of public education in this country. Second, it raises some serious questions about the relationship among political science, policy analysis, and political advocacy.

Chapters 1 and 2 provide an introduction to the study and to the enduring controversy over educational choice. Witte returns to the broader politics of school choice in chapters 7 and 8, and he covers a range of key issues. Chapter 3 provides background on Milwaukee's public schools and creation of the voucher program. In chapter 4, Witte discusses participation in the voucher program, which was deliberately targeted at low-income families; the choice students came from poor, predominantly minority, and mostly single-parent female-headed households. Compared with other low-income families in Milwaukee, those of choice students were typically

smaller, and the parents (especially mothers) were more educated, were more involved in schooling before coming into the program, and had higher educational expectations for their children. Witte argues that participation rules matter greatly in determining who enters voucher programs. Furthermore, based on analysis of admissions to voucher schools and how they compare with admissions in other (public and private) choice programs in Milwaukee, Witte concludes: "Absent explicit restrictions, schools will be as selective as permitted" in determining which students they will admit (p. 81). The implication is that, when asking "who chooses?" under school choice, we should pay attention to the schools as well as the families. If unchecked, the actions of both can exacerbate social stratification.

In chapter 5, Witte discusses the handful of private schools involved in the voucher program, noting both their successes and their failures (several schools closed during the period of the study). Witte emphasizes the freedom of private schools to devise their own programs and the variety that results, although he also notes that many of the practices in these schools seem to be similar to those found in traditional public schools.

The issue of student performance outcomes is addressed in chapter 6. Witte points out that this is only part of what should interest us when thinking about the outcomes of voucher programs. He also acknowledges—as would most analysts—that student test scores are a frustratingly narrow measure of achievement. Nonetheless, these are the performance outcomes most easily quantified and hence most readily amenable to analysis using standard forms of social scientific hypothesis testing. Witte presents his original approach to determining the comparative test score performance gains of students in choice schools and traditional schools; he also replicates the analyses of others who have considered the Milwaukee data. As in the past, Witte's analysis indicates that the choice students showed no significant, systematic improvement in performance compared with nonchoice students, no matter how the control group is chosen. The analysis is thorough and persuasive. It is also fair to conclude that the analysis of test score data associated with the Milwaukee program and other voucher programs has benefited from the Witte and Peterson controversy (even though its tone was unfortunate). There is much in this chapter of interest to anyone concerned with selection bias problems and how these might be handled when modeling complex social processes.

In several places Witte expresses concern that reasoned discussion about the future of American education is being crowded out by "choice theater." "What is implied is that the images, symbols, characters, and dramatic flourishes are just as or more important than legislative and administrative details, votes, or budgetary considerations. They certainly are more important than policy evaluations, especially when such evaluations provide complex and mixed findings" (p. 173). Witte believes that few evaluations of school choice programs will be commissioned by policymakers in the future. (Incredibly, all data collection for the Milwaukee program ceased in 1995.) For politicians, the operative question is: Why fund evaluations of your pet programs when the findings will be used against them? For Witte, who comes from the positivist tradition in political science, this apparent negation of objective policy analysis is troubling. It is also enormously troubling for what it implies about the value (or lack of it) that some contemporary politicians accord to informed political debate.

For political scientists interested in policy questions (and, in one way or another, that means most of us), Witte's

broader discussion of school choice politics invites reflection on our own practices. On the one hand, scholars must seek to produce work that seriously advances the discipline's knowledge of politics in general. On the other hand, scholars often have knowledge and skills that can be usefully employed to cast new light on contemporary social phenomena and, perhaps, contribute to practical problem solving. In practice, these two roles are often inextricable, especially for those who seek to illuminate aspects of politics in general through their study of contemporary concerns. With respect to the rise of school choice, the work of political scientists has not been inconsequential for shaping the terms of the debate. The best of this work also has made important contributions to disciplinary knowledge. But still we must ask: Whose interests are advanced when we choose our research topics and publish our findings? And when do we become complicit with the "theater"? Increasingly, questions of this sort are being raised within the discipline, and they deserve to be asked more often.

In *The Market Approach to Education*, Witte draws our attention to the ways that voucher programs (whether publicly or privately funded) have been established with the immediate purpose of providing an escape for poor students from woefully inadequate urban public schools. Yet, he notes that the broader intention of many voucher advocates is to secure subsidies for middle-class families who desire to send their children to private schools or who do so now at considerable cost. If this is the goal of voucher advocates, then those seeking a lifeline from poor urban schools can kiss goodbye to the promised land.

Witte also goes to some length to demonstrate how private foundations and business interests have worked in the background to support a variety of school choice efforts, setting agendas through their quiet power. To the extent that scholars play into these private agendas while masking themselves as objective social scientists, they become complicit with the "theater." Does this promote the discipline? Witte's efforts to reflect on such questions are laudable. They make this book highly relevant and recommended reading for all political scientists, not just those who care about education policy and the details of voucher programs.

Comparative Politics

Prosecuting War Crimes and Genocide: The Twentieth Century Experience. By Howard Ball. Lawrence: University of Kansas Press, 1999. 288p. $35.00.

Richard Falk, *Princeton University*

There is little doubt that an abiding feature of international relations in the current period is the struggle to extend the rule of law to crimes of state. The 1998 detention in Britain of former Chilean dictator Augusto Pinochet, in response to a request for extradition issued by a Spanish judge, gave prominence to this quest. This development was further reinforced by the campaign to establish a permanent international criminal court, which eventuated in a treaty signed in Rome by about 120 countries two years ago and is on its way to securing the 60 ratifications needed to bring it into force. Organized international society is far from the end of this journey; powerful governments, including our own, are not ready to submit their citizens or leaders to international procedures of accountability.

It is against such a background that Howard Ball's carefully researched *Prosecuting War Crimes and Genocide* needs to be considered. Ball does an excellent job of narrating the main steps taken over the last several decades, especially since the end of World War II, in relation to wartime criminality, genocide, and severe violations of human rights. Separate chapters discuss the Nuremberg experience and the trials held in Tokyo to assess the responsibility of Japanese military and civilian leaders for atrocities committed during their period of rule. There are also separate chapters on each of the three major genocidal experiences of the last part of the twentieth century: Cambodia, the Balkans, and Rwanda. The book concludes with chapters on the Rome treaty as a belated fulfillment of the Nuremberg legacy and on an assessment of the obstacles that remain, especially the unwillingness of the United States to commit itself to the accountability that it did so much to bring about.

Ball gives an excellent and generally reliable overview of this complex and controversial story. His concluding words indicate his belief that a positive trend is under way: "The

world community's quest for an effective enforcement response to these horrendous crimes will be realized sooner than later and ... the twenty-first century will be the initial millennium where justice for victims of human rights abuses, genocide, torture, war crimes, and crimes against humanity becomes the norm rather than the exception" (p. 240). Ball poses the underlying issue of policy as a collision between "realpolitik" and "solemn cries for utopian justice" (p. 6). He makes no secret of his own predisposition: "The culture of impunity must end" (p. 6). The impunity generally enjoyed by perpetrators of such terrible acts deprives victims and survivors of "justice" (p. 10) and serves as the most dramatic indication of the extent to which the realities of international relations and domestic governance remain games of power.

Most tellingly on this issue for Ball was the cynical response of the United States to the Cambodian ordeal perpetrated by the Pol Pot regime in the late 1970s. Ball notes that the Carter administration, with special prodding from National Security Advisor Zbigniew Brzezinski, gave strong diplomatic and indirect economic (and even military) support to the Khmer Rouge. The U.S. government gave priority to opposing the Vietnamese invasion that toppled the genocidal regime and to playing "the China card" against the Soviet Union. Such a blatant embrace of geopolitics by a liberal American president who had gained world notoriety for his support of a human rights diplomacy should serve as a wakeup call for those who claim that the United States exercises moral leadership in world affairs. Belatedly, the Clinton administration, despite Pol Pot's recent death, has called for the creation of a special international criminal tribunal mandated to bring to justice those remnants of the Khmer Rouge that survive. So far, the Cambodian government has resisted the call, suggesting its own suspect willingness to proceed. Ball tells this complex story without much editorializing, although he does rely upon the Cambodian case to make his central argument that realpolitik contributes greatly to impunity for notorious perpetrators of crimes of state.

The weaknesses of Ball's book relate to its lack of analytic depth. Because the difficult theoretical issues are addressed rather superficially, the book seems more suitable for the

general reader or undergraduate than for an advanced student of this subject. The narration does provide a coherent account of the international efforts to overcome impunity through prosecution and punishment, but it does not explain very well why the glass is even half full, or more tellingly, why it is only half full. Ball does not recount at all the saga of heroic individuals of conscience who kept the Nuremberg idea alive during the long period of intergovernmental silence of almost fifty years. This silence started almost immediately after the Nuremberg experience and lasted until the tribunal dealing with the crimes associated with the breakup of Yugoslavia was established in 1993. He does not mention the important commentary on accountability that occurred during the Vietnam War when Telford Taylor, a prosecutor at Nuremberg, wrote a widely discussed book, *Nuremberg and Vietnam: An American Tragedy* (1970). In other words, the role of civil society in the struggle against impunity is virtually ignored. As a result, there is an inadequate presentation of the successful collaboration between a coalition of 200 or more NGOs and a series of moderate governments that eventuated in the Rome treaty, despite the objections by such powerhouses as the United States and China. If an effective international criminal court ever sees daylight, it will be largely the result of the continuing transnational pressure mounted by civil society organizations and dedicated citizens; although their role is briefly acknowledged by Ball (pp. 196–8), its political relevance is not sufficiently assessed.

There is also little insight into both the willingness to embark on the path of international accountability in the first place and the subsequent reluctance to proceed farther. On the one side, there were the peculiar circumstances that existed in 1945, including the guilt of the victorious liberal democracies that they had turned such a blind eye to Nazi policies so long as these were directed internally. Furthermore, there was a widely shared sense that the outcome of World War II was a victory in "a just war," which made it appropriate to punish the unjust side so as to reinforce the political importance of defeating fascism. There was, as well, the pedagogic impulse to tell the full story of Nazi and Japanese wrongdoing both as a way to acknowledge past victims and to build support for a more humane politics in the future. On the other side, from the outset, was the problem of "dirty hands," the extent to which the Allies had themselves waged the war in a manner difficult to reconcile with the laws of war, especially the strategic bombing campaigns against German and Japanese cities and the use of atomic bombs against Hiroshima and Nagasaki. In the Cold War era, the bipolar structure of the world order and the recourse by both sides to interventionary wars created a mutual vulnerability to charges of Nuremberg-like criminality. Quietly, without public discourse, major states abandoned the pursuit of accountability for crimes of state.

Ball also does not give much insight as to why the moves against impunity were revived in the 1990s, and he does not give the Latin American experience of transition to democracy the attention it deserves. It was the fall of military dictators in leading countries in Latin America more than a decade ago that revived the sense that crimes committed by a government need to be acknowledged, even if domestic conditions do not permit the full imposition of accountability on the perpetrators. The UN creation of tribunals to address the crimes emanating from the breakup of Yugoslavia and the genocide in Rwanda in the 1990s reflected a special set of circumstances that centered on the ambivalence of the United States and its allies, especially their own unwillingness to take timely risks to protect the victims. Refocusing attention on criminality seemed without risk and usefully diver-

sionary, but at the same time responsive to increasing public and media pressures to enforce international law against its most willful violators. At the same time, the reluctance to pursue high-profile indicted war criminals, such as Karadzic and Mladic in Bosnia, discloses again the degree to which political opportunism (in this case the fear of retaliatory casualties among the peacekeepers) leads to ill-deserved impunity.

All in all, Ball has contributed a useful, if limited, book to the growing literature on this subject. It does not have the scholarly originality or conceptual elegance of either Martha Minow's fine *Between Vengeance and Forgiveness: Facing History after Genocide and Mass Violence* (1998) or Gary Jonathan Bass's challenging *To Stay the Hand of Vengeance: The Politics of War Crimes Tribunals* (2000). At the same time, *Prosecuting War Crimes and Genocide* not only tells the main story in a generally helpful and accurate way but also engages itself in the "good fight" for criminal accountability as an integral dimension of global humane governance.

Warriors in Politics: Hindu Nationalism, Violence, and the Shiv Sena in India. By Sikata Banerjee. Boulder, CO: Westview Press, 2000. 207p. $62.00.
Ethnicity and Populist Mobilization: Political Parties, Citizens, and Democracy in South India. By Narendra Subramanian. Delhi: Oxford University Press, 1999. 371p. $59.95.

Sanjib Baruah, *Bard College*

These two books are about two powerful regional political forces in India—the Shiv Sena of Maharashtra (with a focus on the city of Mumbai) and the Dravidianist parties of Tamil Nadu. Many readers of this journal may know these places by their older names: Mumbai is Bombay, and the state of Tamil Nadu and its capital city were once known as Madras.

Both books, not coincidentally, have much to say about the rise of Hindu nationalism in India, which is perhaps the most dramatic change in the Indian political landscape in recent years. That, indeed, is the central theme of Banerjee's book, which investigates the Hindu-Muslim riots in Mumbai in 1993. Banerjee argues that the politics of Hindu nationalism provides the context for the riots. In Mumbai, the major political force articulating a Hindu nationalist agenda is the Shiv Sena (literally, the warriors of Shivaji, a legendary Maharastrian Hindu hero).

In Tamil Nadu, by contrast, Hindu nationalist political forces have not fared well. According to Subramanian, it is largely because the Dravidianist parties occupy that ideological and political space. In his words, parties that appeal to caste and language in Tamil Nadu have "inhibited the growth of other visions of community" (p. 32). The idea of Dravidianism comes from the fact that languages in southern India are of the Dravidian family, whereas most in northern India are of the Indo-European family. The ideologues of Dravidianism consider non-Brahmin Tamil speakers—indeed, speakers of all southern India languages—to be descendants of a Dravidian race.

Subramanian's theoretical framing helps bring out the broader implications of both books. The official nationalism of many postcolonial societies, he writes, emphasizes secularism and seeks to make the state a neutral arbiter among competing religious and ethnic groups. The goal is to create inclusive political communities, but the problem is that by keeping the cultural component "thin"—because emphasizing particular cultural symbols would seem exclusionary to

some—secular nationalism has also proven to be a rather fragile foundation for nation-building projects.

In India that project, it now seems, did not strike deep roots in many regions. Secularist pan-Indian political parties are losing ground to parties that employ "culturally thicker appeals to community" (p. 323). But at what cost? The two books together throw light on the good, bad, and ugly sides of the politics of ethnic and religious political mobilization in India.

Subramanian makes creative use of the notion of populism to elaborate the complex story of the ethnic mobilization by Dravidianist parties. In many situations ethnic appeals work because they are mixed with populist notions of "a common people," as opposed to a privileged elite (p. 8). This can heighten ethnic antagonisms in some situations, as occurred in Srilanka and could have occurred in Tamil Nadu, but it also can temper ethnic hostilities. In Tamil Nadu, he argues, the populist turn of Dravidianist parties contained the exclusionary and violence-unleashing potential of Dravidianist discourse. Rather than police ethnic boundaries, Dravidianist parties celebrated mass culture and created entitlements for less privileged groups.

The ascent of Dravidianist parties in Tamil Nadu has been accompanied by a remarkable afflorescence of associational life. Even though these associations were formed under the auspices of the Dravidianist political leadership, there was significant room for pluralism and for competing political projects within the Dravidianist framework. Important differences arose over time that led to splits in the party and to leadership change, which accompanied shifts in the party's platforms and mobilizational styles. Subramanian's interesting analytical distinctions between assertive populism and paternalistic populism, for instance, roughly coincide with what are now two distinct parties—the Dravida Munnetra Kazhagam (DMK) and the All India Anna Dravida Munnetra Kazhagam (AIDMK).

The "All India" was added to the AIDMK's name in 1976 to emphasize its acceptance of pan-Indianism. Dravidianism by then had come a long way. In the early stages it advocated secession, a platform it abandoned in the 1960s. This is remarkable in view of the havoc that secessionist politics have caused in Kashmir, Punjab, and the northeastern states. Subramanian gives the credit to the Dravidianist parties, not to the supposed capacity of the pan-Indian political system to accommodate diversity and ethnic dissent. Although populist Dravidianism is in many ways one of Indian democracy's success stories, Subramanian does not ignore its occasional authoritarian propensities. The AIDMK, for instance, has attacked trade unions, has been intolerant of oppositional newspapers, and has showed more than a fleeting fondness for repressive laws.

Subramanian combines interesting theoretical insights, drawn from a wide range of readings, with painstaking empirical research. He interviewed hundreds of local leaders in five carefully selected electoral constituencies of the state assembly. The book is almost a dazzling piece of scholarship. Certainly, it is the most important work to date on the politics of Dravidianism and is an original contribution to the study of Indian politics.

One misses in Banerjee's book a sense of what goes on inside the Shiv Sena organization. For instance, there is reference to one crucial ideological turn, but one does not fully understand why it happened. From being a party of Maharashtrian regionalism in the late 1960s and 1970s, the Shiv Sena became a party of Hindu nationalism. Banerjee refers to the earlier period, when the Sena clashed with Hindu migrants from southern India and Uttar Pradesh over

access to jobs. Since 1984, she suggests, "because of an alliance with the more nationally based BJP" (the Bharatiya Janata Party), the Sena has "broadened its Maharashtrian Hindu vision to include a pan-Hindu focus" (p. 41). Most readers would like to know more, however.

The strength of Banerjee's book is the focus on the structural factors that contributed to the Mumbai riots of 1993. Most notable is the role the author assigns to the failures of the Bombay textile workers strike in 1983. Historically, the hundreds of textile mills and the ancillary industries—machinery, dyes, chemicals, and the marketing and transportation of textiles—had provided employment to a very large part of Mumbai's population. The strike hastened the closure of many mills and permanently displaced millions of workers in these industries.

This economic devastation, according to Banerjee, provided the backdrop for the Shiv Sena's aggressive politics of Hindu nationalism. The 1983 strike shifted textile production to the powerloom sector in small towns. Powerlooms use less complex technology and are owned by small-scale entrepreneurs who employ very few workers. Thousands of workers who were making a living wage in large industries—albeit only enough to live in the slums of one of the world's most expensive cities—now had to make do with less pay in the less predictable world of the informal sector. Furthermore, there were somewhat differential effects on Hindus and Muslims. Many workers in the textile mills were Hindus, but the powerlooms in small towns mostly employed Muslims. Many Hindus, Banerjee suggests, turned to the Shiv Sena. She points out that slums like Jogeshwari, which saw the worst violence in 1993, were home to a very large number of displaced mill workers.

The relationship Banerjee posits between the decline of Mumbai's textile industry and the Hindu-Muslim riots is complex. The decline of this key industry, along with a stagnant labor market and the manifest erosion in the Congress Party's capacity to govern, "conspired to create a dawning awareness among many voters that a viable political alternative was necessary" (p. 111). The Shiv Sena filled that void. Despite this complex formulation, to explain riots one has to take another crucial step. As her theoretical point of departure, Banerjee searches for an alternative to the either-or choice between primordialist and rational choice explanations. To her credit she emphasizes context: the "economic and political crisis that creates a sense of anxiety" (p. 5). But it seems to me that one must take into account the logic of crowd behavior—"the heightened psychic states and convulsive behavioral impulsions"—to which anthropologist Stanley J. Tambiah has drawn attention (*Leveling Crowds: Ethnonationalist Conflicts and Collective Violence in South Asia*, 1996, p. 266).

Given Banerjee's lucid criticism of binary categories, it is surprising that in drawing out the implications of her study for Indian politics she gives such primacy to the distinction between ethnic and cultural nationalism. She qualifies these categories significantly, to be sure, but she describes India as standing at the crossroads of a "struggle between cultural and civic nationalism." The outcome will determine whether "constitutional guarantees of civil liberties and protection of minorities enshrined in its Constitution" will survive (pp. 175–6).

Which way will India go? The optimists would turn to Subramanian's thesis that "culturally rooted, yet tolerant, visions of community" not only can emerge in Indian politics but also can acquire hegemony. At India's present political juncture it is hard to disagree with him that such forces are

more likely to curb intolerance than a "culturally vacuous" notion of Indian citizenship (p. 326).

Incentives and Institutions: The Transition to a Market Economy in Russia. By Serguey Braguinsky and Grigory Yavlinsky. Princeton, NJ: Princeton University Press, 2000. 282p. $39.50.

Stephen Wegren, *Southern Methodist University*

After a decade of market reforms, it is obvious to most observers that something has gone wrong in Russia. Some economic indicators suggest modest successes in instituting a market economy, but most of the evidence is clear that the economic and social crisis has far exceeded what might be expected in terms of a "transformation recession." Industrial and agricultural output is down substantially, capital renewal is not keeping pace with the retirement of old machinery, the nation is deindustrializing, real living standards have plummeted, and investment capital is fleeing the country. The authors of this book conclude that "the Russian privatization program was nothing but a grandiose failure" (p. 6).

In the political realm, new institutions have been used to benefit a few vested interests. Elections are competitive, but electoral rules are routinely violated to benefit incumbents. Russia did not choose democracy but stumbled into it. The notion of a benevolent government is far from Russian reality. The authors assert that "if Russian authorities could afford to continue the totalitarian rule, they would most definitely do so" (p. 204). Overall, the political institutions being constructed in Russia are unlikely to lead to a Western-style political system based upon democratic political values.

The purpose of this book is to examine the causes of these outcomes. The answer, according to the authors, is that "the structure of incentives built into the current transition environment is leading to the consolidation of a system that is almost as remote from a free market economy and democratic state as the previous communist system was" (p. 10). Arguing that post-Soviet institutions were built on the foundation of Soviet institutions (not from scratch), the authors examine how old institutions were used by carry-over elites. They also examine how new institutions were manipulated and created dysfunctional incentive structures.

An important aspect of the argument is the attention drawn to incentives. Western lending agencies and advisors concentrated heavily on creating (duplicating) Western-type institutions in Russia with the rather naive expectation that the construction of incentives would or could, by themselves, lead to desired policy outcomes. This view was naive because it ignored both the cultural context in which these institutions operated and the incentive structures derived from them. If outcomes did not meet expectations, the putative reason was "reform was not implemented" or "the state is too weak." Few policymakers or specialists understood that the real reason lay in the way actors responded to the incentive structures they confronted in the new institutional configuration.

Any reader, whether or not specialist or policymaker, will benefit from this book, but three principal shortcomings deserve mention. First, it is argued that the "wrong" incentives emerged from Western-style institutions, which implies that institutions caused these incentives, which in turn led to behaviors that undermined the logic and intent of reform. But why? Why did institutions that were "successful" in other previously authoritarian countries—such as Germany and Japan—lead to different outcomes in Russia? The authors

suggest the reason is cultural, but the cultural aspect is neither explicit nor rigorously examined.

Second, if the nature of reform institutions is the independent variable, causing reform outcomes that will lead neither to democracy nor capitalism, then what can be done? What could have been done? What were the options? What alternative models existed? The authors are very critical of the institutions introduced to Russia and are persuasive in demonstrating the consequences, but they do not present alternatives that might have avoided the fate Russia has endured for the past ten years. If their argument is accepted, not only was the reform path misguided, but also the prognosis for Russia's future is fundamentally pessimistic.

Third, the authors maintain that their analysis is relevant for other transitional nations in the former Soviet bloc, yet they indicate that the reform outcomes are culturally specific to Russia. The way in which Russians respond to incentive structures is not being duplicated throughout the Eastern bloc, and in several nations reform outcomes are entirely different. For example, the Czech Republic, Hungary, and even Poland have not shared Russia's economic fate. The Baltic states, although beset with energy problems that affect their economy, have not experienced the same political deformations in their advance toward democracy. Thus, the analysis seems to be Russia-specific, not broadly applicable to other nations.

Overall, the strengths greatly outweigh the shortcomings. The book is very insightful and persuasively argued. It benefits from the rich detail and well-illustrated examples used to substantiate larger points. This would be expected, since one of the authors is a notable political figure, leader of a political party, and a candidate for president in 1996 and 2000. Without doubt, this book should not simply be read, but should be pondered and studied by policymakers and specialists.

Ending the LDP Hegemony: Party Cooperation in Japan. By Ray Christensen. Honolulu: University of Hawaii Press, 2000. 228p. $52.00 cloth, $27.95 paper.

Kenji Hayao, *Boston College*

The Japanese party system has been in flux in recent years. In 1993, two groups defected from the Liberal Democratic Party (LDP) and joined with the opposition to form a broadly based coalition government. A year later, the LDP regained power by creating a coalition government with its ideological opponent, the Japan Socialist Party (JSP). Both events shocked virtually everyone at the time. The LDP had been in power for so long—almost 40 years—that it seemed almost inconceivable that it could lose power. For just as long, the JSP had been the main opposition. By the 2000 election, a dozen parties had come and gone, the JSP's strength dropped to a very small fraction of what it was a decade earlier, and the LDP had to turn to various coalition partners to maintain its control of government. All this is quite puzzling to even close watchers of Japanese politics, because party politics, especially the role of opposition parties, has been a relatively understudied area. For those who want to make sense of how these events came to pass, Ray Christensen's *Ending the LDP Hegemony* will be very helpful.

According to conventional wisdom, opposition leaders were either too incompetent, too complacent, or too ideologically rigid to be much of a threat to LDP dominance. Christensen shows that the two transforming events of the early 1990s had roots in countless earlier attempts by moderate opposition leaders to expand their numbers in the

National Assembly and take control of the government. The book outlines three major strategies pursued by these leaders in their quest for power.

First, opposition leaders worked to unify the centrist forces. In 1970, the Democratic Socialist Party (DSP), the Clean Government Party (CGP), and the JSP announced a merger plan. Although this did not reach fruition, they began to coordinate electoral campaigns in order to run the optimal number of candidates in each district and to divide the opposition vote evenly, so as to minimize the number of "wasted" votes. Christensen's careful study shows that, surprisingly, the opposition did better than the LDP in coordinating their efforts.

Second, opposition leaders repeatedly tried to entice various LDP faction leaders to join them. As early as 1966, the DSP's Sasaki made plans with Nakasone Yasuhiro "to dissolve the Diet and create a new centrist coalition" (p. 137). In 1974, opposition leaders collaborated with Miki Takeo. In 1983, they worked to support Nikaidō Susumu for prime minister against the incumbent Nakasone. In 1990, the JSP's Ishibashi and the LDP's kingpin, Kanemaru Shin, discussed plans for Ozawa Ichirō and his followers to defect from the LDP, three years before the split actually occurred.

Third, the leaders of the centrist forces explored ways to become a junior partner with the LDP in a coalition government. As Christensen points out, the LDP had very good relations with all the major opposition parties, with the notable exception of the Japan Communist Party. In 1979–80, for instance, the DSP and CGP both worked with Prime Minister Ohira on legislative and budget bills with the hope of becoming coalition partners in the near future.

These efforts brought some success. The electoral alliances helped whittle down the LDP majority to razor-thin margins in the 1970s, and the LDP lost control of the upper house in 1989. The opposition parties often negotiated with the LDP to gain its support in local elections and force compromises on legislative bills. Nevertheless, at least until 1993, the opposition was unable to bring down the LDP or force it into forming a coalition. Even when the LDP lost control in 1993, it soon divided the opposition again and took it back.

Christensen argues that campaign laws and the party system put the opposition at a strategic disadvantage. Campaign laws (e.g., bans on candidates from advertising in the media) hindered the parties from reaching voters not tied to parties or politicians through organizational networks, such as labor unions, agricultural cooperatives, business groups, or kō enkai (personal support groups). Thus, parties had difficulty expanding beyond their base of support. Instead, the opposition was forced to depend mostly on building alliances with one another or enticing defectors from the LDP. In addition, because the opposition was fragmented, it faced "strategic dilemmas of cooperation" (p. 7). As Christensen points out, the LDP maintained remarkably close ties to all the opposition parties, with the exception of the communists, so it often exploited their divisions to entice one of them to break from a united anti-LDP front.

The book would have been even stronger had it pursued the analysis a bit farther. Given the importance of electoral systems in electoral cooperation, more could have been done to analyze the effect of the electoral system on parties and the party system. For instance, Christensen notes that the fragmented opposition has put these parties at a strategic disadvantage, but he does not mention to what extent the old electoral system helped create a fragmented party system, given its semiproportional nature. More also could have been done to analyze the effects of the new electoral system on the development of electoral alliances. The new system allows

candidates running in single-member districts also to appear on proportional representation party lists. Potentially, this encourages parties to run candidates in single-member districts in order to raise the profile of the party, regardless of whether such candidates have a chance to win. This seemed to be much more of a problem in the 2000 election for the opposition parties than for the ruling coalition.

Ending the LDP Hegemony is a welcome addition to the literature, especially because little has been written about opposition parties in Japan in recent decades. With the continuing changes in Japan's party system and among its political parties, Christensen's study will be valuable to anyone interested in understanding these puzzling developments.

Revolutionizing the Family: Politics, Love, and Divorce in Urban and Rural China, 1949–1968. By Neil J. Diamant. Berkeley: University of California Press, 2000. 458p. $55.00.

Vivienne Shue, *Cornell University*

This is a thoroughly revisionist study, in the best sense of the word. Starting from the conviction that a close look at marriage and divorce in China can open "a wide window onto what might be called the 'interface' between state and society" (p. 14), Diamant sets out to capture a better sense of the quality of "everyday interactions between citizen and state" (p. 15). He uses these observations to shed light on larger questions about the degree to which citizens in different social strata may have regarded the state as legitimate or illegitimate, as well as the extent to which state interventions designed to alter power relations in both rural and urban society were effective.

Diamant first lays out the dominant findings and interpretations in the literature on marriage and divorce in Maoist China. This literature teaches that the effect of the 1950 Marriage Law's guarantee of the right to seek divorce was generally limited. Especially in rural areas, where traditional patriarchal family relations held stable after the revolution, women bold enough to demand a divorce were most often frustrated by community norms and power structures or foiled by the firm opposition of local state officials, who (mostly male) sympathized more with the interests of husbands than with the ideals of freedom expressed in the Marriage Law. Divorce in the 1950s and 1960s was more an urban than a rural phenomenon, pursued more by better educated and more cosmopolitan elite groups than by ordinary working-class or rural people. Diamant takes these widely accepted hypotheses and interpretations, tests them against some fascinating new sources of data, and delivers a radical reinterpretation of who sought divorce and how different levels and institutions of the state behaved in the handling of divorce cases.

Among the first wave of Western political scientists to gain access to local government archives in China, Diamant rests his arguments on painstaking data collection from several different municipal, district, and county government archives in the Beijing and Shanghai areas as well as in the southwestern province of Yunnan. This unprecedented access, and his own prodigious scholarly effort, allow him to compare Women's Federation documentation, Bureau of Civil Affairs records and statistics, labor union reports, and court materials in several urban, periurban, and rural localities. In these rather candid, unedited, and colorful new sources, originally compiled by local officials and staff for internal use only, Diamant finds evidence that divorce was far more common in

the countryside than has been believed and that rural women, when they did face family and official opposition, nonetheless frequently pursued divorce with blunt and combative determination. Some went on foot when necessary to a number of different government offices to pour out their grievances and demand relief, threatened suicide, or feigned madness if it would help gain them sympathy.

In urban working-class districts, marriages were quite often quickly entered into and just as often hastily dissolved; many spouses clearly regarded marriage more as a matter of pleasure and convenience than a sober and binding lifetime commitment. Furthermore, in the urban districts where the more educated elites resided, divorce was rare and shunned as an unacceptably face-losing option. Diamant argues that educated elite families, influenced by residual Confucian styles of belief and behavior, held to stern codes of propriety and rather prudish values where sex, marriage, and divorce were concerned. They also manifested some more modernist notions concerning "privacy" and were reluctant to air their dirty linen to outsiders, especially since involving state officials in their domestic disputes could have what they regarded as fearful political repercussions. In contrast, Diamant argues, peasants and workers sustained a less fussy, less modernist, more frank, and even "Rabelaisian" sexual culture as well as a more tenaciously "feisty" legal culture. Because they lived in communities where the neighbors were likely to know their business already, they had less "privacy" to lose by drawing the state into their affairs, and they had much to gain in the way of income or personal satisfaction by switching partners.

Diamant stresses the bumbling ineptness of state officials, on the one hand, and women's determined "agency," rather than their victimization, on the other. He shows how, when refused a divorce by one local office, women frequently pressed their cases in other offices, often higher up the administrative system, to secure the outcome they desired. He develops this observation into a general hypothesis that local state institutions were often regarded by peasants as less "legitimate" than offices closer to the center, a plausible and promising beginning for future projects that would disaggregate not only the state but also the very concept of state legitimacy.

Yet, just here there is an apparent weakness in Diamant's argument. After laboring long to persuade the reader that Chinese peasants enjoyed a rough and frank, not a prudish, sexual culture, the author offers as the main reason for the relative illegitimacy of local officials the common knowledge peasants often possessed of those officials' own moral weaknesses and sexual improprieties (e.g., pp. 234, 318). Perhaps Diamant is right that these delegitimated local officeholders, but if peasants are so accustomed to a culture of sexual transgression, then there are some missing steps in the logic. Were local officials held to a "higher" moral standard? If so, why?

It is possible to point to a few loose ends such as this, but loose ends may be inevitable in a highly original study that boldly generates hypotheses. Diamant's book is interestingly structured from a methodological point of view, and it is written with arresting insight into the Chinese social condition. It lays out a coherent, strikingly original vision of family-state relations, of contrasting class-based sexual and legal cultures coexisting within Chinese society, and of the frustrating dynamics of state-led social reform during the Maoist era. *Revolutionizing the Family* concludes with an afterword that usefully relates the author's findings to trends in the post-Mao period as well.

Can Democracy Take Root in Post-Soviet Russia? Explorations in State Society Relations. By Harry Eckstein, Frederic J. Fleron, Jr., Erik P. Hoffman, and William M. Reisinger, with Richard Ahl, Russell Bova, and Philip G. Roeder. Lanham, MD: Rowman & Littlefield, 1998. 420p. $64.00 cloth, $21.95 paper.

Jeffrey Kopstein, *University of Colorado at Boulder*

A decade after communism's collapse, what do we have to report? For one thing, some states certainly have it easier than others. A handful of the postcommunist states located in close proximity to the West have made admirable progress in constructing viable market economies and meaningful institutions of democratic representation, but a much larger group has yet to taste the fruits of what Western politicians in the 1990s called "market democracy." Among this latter group, some retain at the time of this writing a formal commitment to democracy, but others have never moved very far from the authoritarian cronyism where they started.

On a continuum between meaningfully democratic and outright autocratic, in 1999 Russia rests about half way between the two extremes. The authors of this volume start with two questions, one substantive and the other methodological. First, which way will Russia go—democratic or authoritarian? Second, how should we go about studying postcommunist Russia? Both are important questions, but the authors concentrate most of their attention on the methodological one.

Is such a concern justified? Have our choices of theoretical models and methods skewed our research so that the things we have learned about postcommunism are not the things we need to know? What are we missing? A great deal, the authors contend. Approaches to the study of postcommunism, they claim, have been one sided, concentrating either on the state (as in much of the transition and institutionalist research) or on society (as in all survey research), but never on both simultaneously.

Philip Roeder gracefully takes on the role of sacrificial lamb by contributing an interesting and carefully researched article that uses a model of elite bargaining to explain variation in the constitutions of the 15 post-Soviet states. Russell Bova provides a stimulating chapter on the conditions under which institutional choice either matters or makes little difference to outcomes. He argues that from the outset Russia chose precisely the wrong institutions (semipresidentialism with mixed proportional representation and single-member-district electoral rules), ones that were sure to hinder party development, foster an irresponsible grandstanding parliament, and isolate a presidency from the society over which it rules.

But that is the kind of work the primary authors of this volume do not like or at least believe needs to be grounded in an approach more sensitive to society. What is the alternative offered? Given the list of contributors, it should not be surprising that the preferred approach is Eckstein's congruence theory, which is really a theory of political stability. A polity will be stable to the extent there is congruence between the authority patterns of the state and those of other social units. Polities that work best (are the most stable) have a mixture of authoritarian and democratic features, which in the jargon of the theory is called "balanced disparities." Too much hierarchy produces autocracy, and too little produces chaos.

What such a theory requires is very careful study of primary social units (such as the family and schools) or adjacent social units (such as parties) as well as a comparison of the authority patterns within the state. Thereafter, a

judgment must be made about the congruence among the patterns. But, as William Reissinger notes in his insightful summary of congruence theory, such a research design is extraordinarily demanding. It also may be indeterminate. The call for fieldwork is laudable and important, but how does one go about judging whether authority patterns are congruent? Does this not lead us back into the older (and not very productive) debates about the nature of historical Russian culture (how democratic it is/was)? One imagines teams of fieldworkers eating breakfast with families, dashing off to attend classes, and then accompanying politicians in their daily routines in search of congruence. Left unclear is the procedure for determining when congruence is there or not. The chapters that attempt a preliminary application of congruence theory (preliminary because none involve primary fieldwork) underscore this concern.

Notwithstanding these criticisms, the volume is a valuable contribution to the field of postcommunist studies. By explicitly attempting to identify what an established theory in the social sciences would have to do in order to be tested adequately in the Russian context, the authors challenge the rest of the field to provide an alternative. Furthermore, although doubts remain about whether such a demanding approach as congruence theory can yield definitive results, the call for more fieldwork in Russia is timely and important. The sad fact is that we still know very little about how Russian political and social institutions work. What comparativists need at present is a whole range of detailed institutional and social studies on postcommunist states and societies. If the inspiration for them is congruence theory, then the authors of this volume will have accomplished a great deal.

Reconstructing Citizenship: The Politics of Nationality Reform and Immigration in Contemporary France. By Miriam Feldblum. Albany, NY: SUNY Press, 1999. 227p. $54.50 cloth, $17.95 paper.

Mark J. Miller, *University of Delaware*

As recently as a quarter-century ago, virtually no one knew or cared about French nationality law and policy. But in the wake of the May-June events of 1968 there was a *prise de conscience* about international migration and its effects upon French society and the immigrants themselves. New Order, the then quite obscure extreme rightist group, began to organize protest rallies against illegal immigration. In 1974, the post of Secretary of State for Immigration was created to symbolize the growing concern accorded international migration by the French government. Successive governments, whether rightist or leftist in orientation, pledged to recover control over migration.

Miriam Feldblum examines the growing saliency of laws and policies concerning French nationality and citizenship as a political process, drawing from the contentious politics approach championed by Doug McAdam and Charles Tilley, among others. She critiques structuralist and institutionalist analyses of citizenship politics in Europe as pointing to the need "for differentiated but dynamic" (p. 9) understanding of citizenship politics. She thereby situates her case study in the broader comparative sweep of citizenship politics, which emerged only in the late 1980s, with a few notable exceptions, in the pioneering works by Rogers Brubaker and Tomas Hammar.

Feldblum endeavors to correct what she views as a flawed understanding of the politicization of citizenship that privileges the role played by the extreme Right. The immigration issue provided the window of opportunity for the long

discredited, marginalized, and splintered extreme Right to recover electoral appeal, but immigrant organizations also played a key role in politicizing citizenship issues. Although there is an extensive French-language literature about immigrant associations and their campaigns and activities in the 1980s, very little has been written about them in English. Immigrants emerged as political actors in France in the 1970s with their factory strikes, the long SONACOTRA rent strike, and their protests against racist violence and for legalization. By the 1980s, organizations comprised largely of French citizens of North African Muslim background began to press for acceptance and equality of rights and mobilized against prejudice and racist attacks. They frequently asserted a right to be different. In so doing, they challenged the longstanding, almost sacrosanct, Jacobin republican tradition that expects immigrants to become French, not only legally but also culturally.

Feldblum is at her best in explaining why the call for acceptance of difference challenged Jacobin ideals. From the period of the bitterly contested French Revolution, regional or hyphenated identities were associated with subversion and disloyalty to the Republic. In the twentieth century, various institutions, including the Roman Catholic Church, trade unions, military service, and the French Communist Party, long functioned to assimilate immigrants. By the 1980s, however, the waning influence of these institutions was palpable. The National Front burst onto the national scene after stunning municipal elections in Dreux in 1983. In the 1986 parliamentary elections, it won 30 seats in the Chamber of Deputies. Feldblum acknowledges that the National Front's emergence helped trigger the mid-1980s debate over national identity, but all parties to the debate used the language of nation and national identity to articulate positions on immigration, pluralism, and the extreme Right. She cites René Gallisot's assessment that the antiracism of the Left was "caught in the trap of national identity" (p. 54).

In 1985, revision of the French Nationality Code was proposed by a moderate conservative deputy to restrict access to citizenship to those born on French soil of noncitizen parents. Feldblum provides an exhaustive account of the politics surrounding this measure, which ultimately was scuttled by mass protests against proposed reforms of the French university system. She concludes that the failure of restrictive citizenship politics in the mid-1980s was due to "institutional and political process constraints that limited the Right's attempt to revise nationality code" (p. 99). She contrasts her analysis to explanations that rely on national models and culturalist notions.

In 1987, Prime Minister Jacques Chirac appointed the Nationality Commission, headed by Marceau Long. This highly respected body held a series of televised hearings in 1987 and 1988 before issuing recommendations, which in Feldblum's view reconstructed French citizenship in a manner consistent with the Jacobin tradition. The work of the commission coincided with the drama created by the adoption of the Single European Act in 1986, which seemed to challenge French national identity from above, as it were, by creating a truly single market among member states of the European Community.

The commission's recommendations reiterated such traditional republican ideals as national integration, secularity, and limited universalism (p. 128). There was much more continuity then discontinuity. The moderate Right and the Left endorsed the recommendations, which were rejected by the National Front. Many of the immigrant associations also embraced the recommendations and in so doing distanced themselves from the earlier calls for a right to difference. The overall tenor of immigrant-background politics changed, as

many French Muslims openly affirmed their allegiance to French republican ideals.

The final chapters of the book analyze the controversy over the *foulard*, the headscarf worn by Muslims, and nationality law reforms in the 1990s. Since 1989, French educational establishments have been roiled by recurrent debates about whether Muslim girls should be permitted to wear a scarf to class. For many French, it signifies a repudiation of French culture and society as well as the secularity of state-provided education. The scarves evoke fear of Islamic fundamentalism within France. The government response was complicated by the fact that there was no institutionalized representation at the national level for France's burgeoning Islamic community. Feldblum's analysis reveals how limited is the space for American-style ethnic politics in republican France. Indeed, the Socialist government of Prime Minister Michel Rocard explicitly rejected that approach to handling the affair. Disputes over headscarves continue to erupt from time to time, but the outcomes in the 1990s were generally consistent with the Nationality Commission's vision of a tolerant, inclusive France anchored in republican tradition. There can be pluralism and difference, but within carefully delimited republican bounds.

In the 1990s there were first conservative and then Socialist nationality law reforms. Indeed, a centerpiece of the Socialist electoral campaign in 1997 was the call to rescind the nationality law of 1993. As soon as Prime Minister Lionel Jospin's government took power in 1997, a procedure was begun that resulted in legal residency for 80,000 technically illegal aliens. For the Socialists, most of those who qualified had been unfairly treated under the unduly restrictive 1993 law. A principal focus of that reform had been a provision that conferred citizenship upon the children born in France to non-French citizens who had been born in Algeria (and certain additional areas) before 1962, when it was part of the French Republic. The nationality and immigration laws of 1993 were intended to coopt electoral support for the National Front. By 1999, in the wake of the Socialist victory in 1997, the National Front had splintered and lost electoral support.

Feldblum provides an authoritative account of a complex subject that has become an important focus of inquiry only in recent years. This book is highly recommended for graduate-level classes on European politics and comparative migration policies.

A Place in the Sun: Marxism and Fascism in China's Long Revolution. By A. James Gregor. Boulder, CO: Westview, 2000. 231p. $45.00.

William A. Joseph, *Wellesley College*

Few scholarly observers of Chinese politics today would disagree with A. James Gregor's assessment that "little remains of the Marxism of Communist China" (p. 215) other than its use as an ideological subterfuge by the ruling party to bolster its claim to a monopoly on political power in that country. But few would agree with the central thesis of this book that the People's Republic of China (PRC) is best classified by scholars and responded to by Western policymakers as "a variant of contemporary fascism" (p. 166).

Much of the book is a taxonomic exercise that Gregor undertakes in order to dispel what he sees as the "great confusion" exhibited by social scientists in trying to "categorize the system that emerged out of the wreckage of what had been Maoist China" (pp. 163–4). The book is structured to build the case that Chinese communism, which killed millions of people and brought economic and social ruin to the

country under Mao, has given way to Chinese fascism. In the author's view, post-Mao China has achieved considerable economic success but still maintains an iron-fisted grip on the nation's political life while exhibiting prototypically fascist irredentist tendencies in its international behavior.

Gregor places fascist China within the "genus" of political movements he labels "reactive developmental nationalism," which he sees as the key to "Understanding the Twentieth Century" (the sweeping title of the opening chapter). These movements and the regimes they have spawned reflect the common reactions by revolutionary parties and leaders in countries around the globe to the common challenges of economic "retardation" and humiliation, bullying, or outright imperialist aggression by stronger powers. All reactive developmental nationalist systems share political traits, such as a "single, elitist, hegemonic party ... led by a 'charismatic' and 'inerrant' leader" (p. 65), mass mobilization to build regime support, and a political army. They also pursue similar objectives for their beleaguered nations, namely, rapid industrialization and, most important, "a place in the sun" that will establish or recapture the country's security and status in the international system.

One "species" of the reactive developmental nationalism genus is comprised of totalitarian regimes, which in turn incorporate several "subspecies," including fascism, national socialism, and communism in its many national variants, such as Bolshevism, Stalinism, and Maoism. These regimes are ruthlessly dictatorial and exhibit a "bitter anti-imperialism" (p. 92) that often leads to an irredentist and aggressive foreign policy. In contrast, the nontotalitarian species of the genus are not implacably hostile to democracy and combine a staunch antiimperialism with an eagerness to engage the world on the basis of mutual respect and benefit. The examples of this species that receive the most attention in Gregor's analysis are prefascist Italian nationalism and, particularly, Sun Yat-Sen's "Three Principles of the People" ideology and his Kuomintang (Nationalist Party).

At several points Gregor praises the foresight of the nineteenth-century theorist, Friedrich List, for his early formulation of ideas about the dynamics of world politics that are very similar to those of reactive developmental nationalism. He also argues that fascist theory, because it recognized the central role of nationalism in motivating political movements, has provided a particularly accurate and astute guide to understanding the twentieth century. The perceptive insights of both List and the fascists are sharply juxtaposed to the "theoretical incompetence" and "howling implausibility" (p. 68) of Marxism's emphasis on misguided class analysis and spurious internationalism.

How does Gregor fit contemporary China into this taxonomy? He emphatically contends that the reforms introduced by Deng Xiaoping beginning in the late 1970s completely severed the even tenuous ties with Marxism that the PRC had during the Mao years and led to the establishment of a paradigmatically fascist system in China. He notes that Maoist China had some fascist traits, but the socially divisive insistence on class struggle and the imposition of a command economy ran counter to fascism's core value of national unity and the role of the market (under the supervision of a strong state) to promote economic development. Deng remained committed to political dictatorship, but he renounced class struggle and rallied the nation to unite—under the leadership of the Chinese Communist Party—around the goal of modernization via a revived semimarket economy.

Furthermore, post-Mao China under both Deng and his successor, Jiang Zemin, has embarked on a foreign policy marked by aggressive irredentism. Gregor's evidence in-

cludes Chinese policy toward Taiwan and in the East and South China Seas; steps by the PRC to build up and expand the reach of its naval forces; and a regime-sponsored effort to construct a racially based "myth of descent" (p. 162) to prove that China is the birthplace of the earliest human civilization. Such systemic and policy changes lead Gregor to conclude that "the regime on the Chinese mainland has taken on those critical features that have always been employed to identify fascist rule everywhere in the world" (p. 142). He musters further support for his argument by drawing on the works of "the best Fascist theoreticians in the interwar years," whose own "defining traits of fascism" (p. 216) provide a near-perfect description of post-Maoist China.

Another major thread of Gregor's book is the analysis of Sun Yat-Sen's legacy in China's long and still unfolding revolution. According to Gregor, Sun was a classic proponent of the most positive subspecies of reactive developmental nationalism. He was prescient in his belief that China's salvation could come only through capitalist-friendly economic growth and an antiimperialism that both safeguarded Chinese sovereignty and welcomed foreign trade and investment. In fact, Sun so clearly emerges as the paragon of Gregor's analysis that it is very tempting to read an intentional or unintentional pun into the book's title.

Gregor sees considerable consonance between the ideology of Sun Yat-Sen and that of Deng Xiaoping—but with at least two fundamental exceptions. First, Sun's "affable nationalism" has little in common with the "exacerbated and aggressive nationalism" (p. 95) of fascists like Deng. Second, although Sun recognized the need for a protracted period of political tutelage under a single party, he never wavered from his "unqualified commitment to ultimate democratic rule" (p. 165) for China. Gregor refutes the view that Chiang Kai-Shek's Kuomintang or one of its most powerful and influential factions, the Blue Shirt Society, turned toward fascism following Sun's death in 1925. The party was thwarted in its efforts to implement all or part of Sun's mandate on the mainland by communist insurrection and Japanese invasion. But the full democratization of Taiwan under the auspices of the Kuomintang in recent decades is cited as proof that the party has been "inspired throughout its history by Sun's democratic, non-totalitarian, reactive and developmental nationalism" (p. 217).

Besides offering his taxonomic exercise as a guide for scholarly analysis of Chinese politics, Gregor has an explicit second purpose in identifying the proper classification for the regime that governs the PRC: to sound a warning to the Western world about the impending threat posed to its vital interest by a fascist China. He notes that fascist systems "have been singularly hostile and aggressive" and "have been prepared to employ massive violence" (p. 217) in pursuit of their irredentist objectives. In his view, China will be no exception.

The greatest strength of this book is its thought-provoking, erudite, and eloquent—if often redundant—analytical survey and classification of some of the great political movements of modern times. Students of comparative revolutions will find much to contemplate in Gregor's observation that, whatever their ostensible ideological differences, these movements share many fundamental features as variants of reactive developmental nationalism.

But Gregor is on somewhat thinner ground in his attempt to fit modern Chinese political history and contemporary politics into this framework. Many China specialists will, for example, his find his depiction of the Kuomintang under Chiang Kai-Shek to be naively benign and question the validity of citing Taiwan's recent political transition as evi-

dence for Chiang's adherence to Sun Yat-Sen's untested commitment to eventual democratization.

As one of the world's leading scholars of fascism, Gregor certainly has the bona fides to know a fascist regime when he sees one. But his unflinching portrayal of the PRC today as a diehard fascist regime seems a rather injudicious conclusion to reach about a political system that is still very much in a state of flux. No doubt there are important and troublesome features of the regime that point in the direction of fascism. But there are many countervailing trends in China that a more nuanced effort to classify the nature of the current regime would have taken into account. Gregor would have made a much stronger case had he noted, for example, the Communist Party's increasing inability to exert control over or even maintain an organizational presence in various sectors of society; the rapidly expanding influence of the Internet and other modern means of communication; and the spread of at least quasi-democratic elections on the village level.

The last three chapters read, in large part, like a scholarly tract intended to support the views of the so-called Blue Team of conservative politicians, think tank types, journalists, and others who see it as their mission to alert Americans to the "China threat" and steer the country away from the delusional policy of constructive engagement and toward strategic containment. In these chapters, the polemic ultimately overwhelms the scholarship and detracts significantly from what is otherwise a very stimulating book.

Elections in Japan, Korea, and Taiwan under the Single Non-Transferable Vote: The Comparative Study of Embedded Institution. Edited by Bernard Grofman, Sung-Chull Lee, Edwin A. Winckler, and Brian Woodall. Ann Arbor: University of Michigan Press, 1999. 495p. $74.50.

Tun-jen Cheng, *The College of William and Mary*

Under an electoral system of single nontransferable vote (SNTV) with multiple-seat districts, each voter can cast only one vote and only for one candidate, surplus votes cannot be transferred between candidates, and seats go to those candidates with the plurality of votes. Initially crafted by Japanese oligarchs in 1900, this unique system was continuously employed for Japan's lower house elections till 1995, with a brief interlude during the Allied occupation. The SNTV system has been in use in Taiwan since World War II and was adopted in Korea during the Fourth and the Fifth Republic (1973–88). It is ironic that academic interest in this electoral system should increase just when it is being abandoned in its birthplace, Japan, in a fin-de-siecle political act that also ended political dominance of the ruling Liberal Democratic Party (LDP).

Whereas previous studies have focused on single cases or particular aspects of SNTV, this volume vows to compare the system across time and nations as well as with other types. It also attempts to situate the SNTV system in a broader institutional context in order to reveal linkages among various political games that actors may play simultaneously. Thus, aside from delving into the choice, nature, and consequences of the SNTV system, the four editors (especially the chief editor) are also eager to present a theory of institutional embeddedness. This ambitious volume begins with an excellent introduction of the three cases and an exhaustive formulation of hypotheses, and it ends by refining the findings of various chapters into ten propositions that can be retested in the future. It not only comes close to being a definitive study of the SNTV system but also duly directs our attention to the

integrated research on electoral rules, forms of government, and regime types, which is arguably a new frontier for the next round of research on political institutions.

The volume is thorough in answering the question of what SNTV entails (the use and consequences of this system). There is a near consensus among contributors that the system is conducive to money politics, personal voting, and factionalism. For a political party, optimal nomination as well as even distribution of votes among its candidates are crucial to maximizing seats. Overnomination, undernomination, and lopsided vote distribution lead to either wasted votes or collective defeat. For any individual candidate, factional membership enhances the probability of party nomination. Moreover, given that s/he is competing not only with other parties but also with candidates from the same party, party label does not matter that much, whereas personal support base (*koenkai*) is vital to victory. Maintaining *koenkai* is expensive, and money is indeed the milk of politics. Money politics is also influenced by campaign finance regulations, and the selection process for party leadership and the rules for nomination can reinforce or alleviate factionalism under SNTV.

There is an interesting dialogue on the disproportionality between votes and seats under SNTV. Does this system benefit large or small parties? It facilitates the entry of small and opposition parties in Korea and Taiwan, but it also gives a seat bonus to large parties in Japan and Taiwan. District magnitude (the number of seats to be elected from a district, or M) seems to be a critical variable: The smaller the M, the more discriminatory is an SNTV system toward smaller parties; the larger the M, the more it functions as a proportional representation (PR) system. The other factor is the organization strength and skill in solving the vote-distribution problem. As I-Chou Liu points out, party identification and institutional devices help a party segment its supporters into discrete voting blocs and help maximize seats. Gary Cox and Emerson Niou are equally convincing in arguing that all parties, large or small, face the possibility of error in nonoptimal nomination and unequal distribution of votes. "Seat bonuses" accrue to whichever party can alleviate these problems.

There is also a fascinating exchange among country-specific chapters on the fragmentation of the party system under SNTV. Steven Reed and John Bolland extend Durverger's law to SNTV and argue that the number of parties at the district level is M plus one, and the number of factions in larger parties is the same. As the average size of district in Japan was four, the country had a five-party system, and the largest party, LDP, had five factions. The reason for this is the pervasive fear of everyone losing (*tomodaore*) if nomination is out of control, hence the impulse for opposition parties and LDP factions to restrict the number of their candidates in each district (the so-called downward rachet effect). Edwin Winckler argues, however, that the SNTV's logic can be overridden by issue structure, social cleavage, and public preference. Given the deeply entrenched identity cleavage, Taiwan essentially has two main parties, although district magnitude is generally larger than that in pre-1995 Japan. Three chapters on Korea allude to regionalism (versus electoral system) as the principal maker of the party system there. As Grofman aptly summarizes, the same electoral rules lead to different consequences, which illustrates the importance of institutional embeddedness in shaping the outcome.

The volume is also deft in comparing across types. Arend Lijphart shows that SNTV, if controlled for malapportionment and district magnitude, does not create more dispro-

portionality than the list PR systems. He proposes to classify SNTV, and for that matter limited vote systems, as PR, eliminating the intermediatory category between plurality and PR. The chapter by Kathleen Bawn, Gary Cox, and Frances Rosenbluth also connects the study of SNTV to the intellectual stream of the American voters.

Cross-time comparison is neglected (Winckler being a notable exception). Woodall (pp. 48–9) makes an interesting prediction on the change of party system in post-SNTV Japan, but space prevents him from pursuing a comparative static study. Another weakness is the lack of discussion on possible institutional diffusion from Japan to Taiwan and Korea. The origin of SNTV in Taiwan is left unexplained, and adoption of the system in Korea is attributed to Park's power ambition only, as if it had nothing to do with his learning from the Japanese experience. In addition, several chapters allude to but do not systematically address the relationship between SNTV and regime type or the form of government. Finally, the three Korean chapters overlap considerably, as do three chapters on campaigning in Japan under SNTV. These minor criticisms should not detract from the tremendous value of this astutely integrated volume, which is unquestionably the definitive book on SNTV.

States and Power in Africa: Comparative Lessons in Authority and Control. By Jeffrey Herbst. Princeton, NJ: Princeton University Press, 2000. 280p. $55.00. cloth, $17.95 paper.

Donald Rothchild, *University of California, Davis*

In this important reappraisal of Africa's political evolution over an extended period, Jeffrey Herbst engages in a dialogue with scholars of comparative and African politics on the causes of state weakness and the possibilities for state redesign. Herbst argues that state consolidation in Africa has been complicated by the problem of extending authority over its distant territories. Low population densities have been a long-standing obstacle encountered by precolonial, colonial, and independent African rulers alike. It has proven expensive for leaders at the political center to project power over peoples and territories far from the capital city. As a result, rural areas have been neglected, particularly by colonial regimes, and urban areas have been favored in terms of public services and amenities.

Independent African governments largely embraced the state system inherited from colonial times. They maintained the boundaries established by the Berlin Conference of 1884–85 and in doing so accepted limits on central administration of rural areas nominally under their control. "How power was actually expressed was often similar to the precolonial model of concentric circles of authority. States had to control their political cores but often had highly differentiated control over the outlying areas" (p. 134). Despite limited state capabilities, borders proved valuable in strengthening the ability to affirm sovereign authority within them and to deny any ethnoregional claims to self-determination.

The boldness of Herbst's inquiry and analysis is clear to see. The conventional wisdom is challenged on a wide variety of controversial issues: colonialism, neocolonialism, the role of chiefs, ethnicity, boundaries, the state, and international conflict in Africa. For example, rather than view colonial rule as "chang[ing] everything" (p. 29), Herbst stresses the continuities between the colonial period and those that preceded and followed it. On neocolonialism, he raises doubts about the common wisdom regarding multinational control over independent African governments in the contemporary pe-

riod; among other factors, he notes that currency controls by African states dampened the climate for foreign investment by greatly overvaluing exchange rates.

Herbst rejects the common conclusion that national boundaries have been a hindrance to state consolidation in Africa. "It is not that they are alien to current African states but that African leaders have been extraordinarily successful in manipulating the boundaries for their own purposes of staying in power rather than extending the power of their states" (p. 253). A contradiction is evident, then, between the state's claims to sovereignty and the incompleteness of its control of the hinterlands. This contradiction becomes painfully evident in the worst cases, when public order declines, autonomy leads to secessionist attempts, refugees seek sanctuary within their state or across the border, and state collapse occurs.

Inevitably, perhaps, the author's analysis of the contradiction between claims to sovereignty and actual control is more compelling than the solution presented. Arguing that the inherited state system is artificial and static, Herbst calls for alternative designs to reflect the political realities of the post–Cold War era. His recommendations include dealing with African problems on a regional basis, donor support for regional integration, the decertification of states that do not exercise physical control over their hinterlands, the recognition of viable breakaway states, and increased interaction by international organizations with autonomous towns and subregions. These recommendations require the willingness of the great majority of states to work together in an unconventional manner to cope with the challenges of weak states. Because many states are not prepared to deal effectively with the reality of genocide, it seems unlikely that these cautious and uncertain countries can be mobilized anytime soon to address Herbst's suggestions regarding incomplete state control.

Herbst deserves enormous credit for breaking with current approaches to African studies, using broad strokes to paint a vivid and highly original canvas. Because he does not shrink from challenging the conventional wisdom on many critical questions, his book will become a centerpiece of the dialogue on African politics for years to come. Even so, several points deserve further discussion. First, the focus on political geography points to constraints on the state but offers a limited insight into the nature of conflict. As Herbst puts it, "geography is only a given" (p. 159). Such an approach tells us something significant about the limits on central political and military control of hinterlands, but it offers little information about the ambitions of leaders or the uncertainties of patrons or groups that give rise to state-ethnic conflict.

Second, there is little attention to the options for accommodating ethnic conflict through various types of formal and informal power-sharing measures. Herbst concentrates throughout on the elite's preference for the "nation-state," but many African countries are in reality multinational states that have frequently negotiated stable relations on their own. The choices are broader than Herbst examines. Thus, weak states have often resolved their problems politically, forming relations with powerful ethnoregional actors in the hinterlands to compensate for their low level of capability there. In particular, they have included subregional leaders in the cabinet, legislature, and high party positions, making up for their lack of control through the use of such noncoercive incentives as purchase, insurance, and legitimation.

Third, Herbst stresses the strength of boundaries but tends to obscure the extent to which weak African states fail to control the movement of people, arms, and finance across international borders. Diamonds found in Sierra Leone, the

Congo, and Angola are carried across borders and sold on international markets to finance insurgencies; illegal arms are shipped in bulk across international boundaries, fueling such terribly destructive wars as that between Ethiopia and Eritrea; and military forces from seven foreign countries have interceded in the fighting in the Democratic Republic of the Congo. Rather than "escaping the brutal history of continued war" (p. 112), Africans are increasingly brutalized by a variety of international forces beyond their control. The African state, which is often responsible for much of this brutality, lacks the capability to protect its citizens from this externally sponsored havoc.

As with any broadly ranging study, *States and Power in Africa* has problems of approach and analysis, but this is a book of very considerable significance. To his credit, Herbst faces up to many of the major problems confronting Africa in a direct and unflinching manner. This volume will undoubtedly become part of the future intellectual dialogue on comparative and African-related issues and deserves wide attention from both academics and practitioners.

Of Centaurs and Doves: Guatemala's Peace Process. By Susanne Jonas. Boulder, CO: Westview, 2000. 299p. $64.95 cloth, $24.95 paper.
Dictating Democracy: Guatemala and the End of Violent Revolution. By Rachel M. McCleary. Gainesville: University Press of Florida, 1999. 297p. $49.95.

Edelberto Torres-Rivas, *Harvard University*

Bibliography dealing with the armed conflict in Central America, aptly called crisis literature, was abundant until the latter part of the 1990s, although North American academicians paid little attention to Guatemala. Now two books have been published, both with titles that refer to the same phenomenon. The peace process that concerns Susanne Jonas corresponds to the termination of the violent revolution in Rachel McCleary's work.

McCleary's explanation for the end of the violent conflict rests on the role played by the elite principals of Guatemalan society, the business sector and the military. The central argument is that both elites were in continual disaccord, which at times became open conflict, about the authority of the state to make economic policy and to participate in the allocation of resources for mutual benefit. McCleary bases her analysis of the game of the elites on the well-known views of Richard Gunther, John Higley, and Michael Burton (*Elites and Democratic Consolidation in Latin America and Southern Europe,* 1992, and "Types of Political Elites in Post-Communist Eastern Europe," *International Politics* 34 [June 1997]: 153–68) and on Seymour Lipset's research on democracy and economic development ("Some Social Requisites of Democracy," *American Political Science Review* 53 [March 1959]: 69–105, and *Political Man: the Social Bases of Politics,* 1981). She then analyzes Guatemala's recent history with this theoretical instrument. She describes the transition to democracy, introducing a useful distinction between the first transition (1982–86) and a second one (1986–93), and argues that during the first period the military was in power; not until 1986 was Vinicio Cerezo, the first civilian president in thirty years, elected. She calls the first transition the period of formal democracy.

Since McCleary is interested in relating events that could lead to the consolidation of democracy in a country that does not have much democratic background, she examines the game of the military vis-à-vis the government, especially in the 1980s, and the crisis that divided them and forced them to

cede power to the political parties and the civilians. In the advent of democracy, capitalism is important as a means to ensure that the means of production are private property as well as to safeguard the fulfillment of contracts and open competition.

Most of McCleary's work focuses on a transcendent moment in recent political history: the auto-coup of President Jorge Serrano Elías on May 25, 1993. The rivalry between the elites finally ended when they joined forces in defense of constitutional rights and produced a lengthy agreement, the Instancia Nacional de Consenso (National Law of Consensus), which united all the factions of society in favor of institutionalism, law and order. More than political players, they were social players headed by the CACIF (the Coordinating Committee of the Agricultural Commercial Industrial and Financial Associations) which is the highest authority of the business sector, and the military high command. They managed to achieve a unity and consensus that would have been difficult to bring about during any other time in the country's history. In its narrative, *Dictating Democracy* finds the root of its theoretical explanation and its empirical justification: the important agreement of the elites, whose convergent strategies lend stability to the democratic regime. This agreement opens the way for new opportunities for economic change and, more important, for an end to the armed conflict that punished this country for many long years.

The Instancia Nacional de Consenso is considered a civilian movement that arose in response to the collapse of the fledgling democratic order. It was organized by the elites but had the backing of popular organizations that succeeded in representing the political will of Guatemalan civilian society. Because it was sheer coincidence, it cannot be given historical dimension because of the consequences McCleary enthusiastically attributes to it: the end of the conflict between the military and the business sector; the unanimity of the players that lead society; and democratic stability. May 1993 was a decisive moment in the process of democratic construction, and this process has continued with some of the same players and some new ones, without serious setbacks. To be sure, a step forward was accomplished with the election of Ramiro de León Carpio to the National Congress, but new difficulties are still to be encountered along the path to the consolidation of democracy.

Of Centaurs and Doves analyzes the same period but through a different lens. It is not the elite sectors that explain history but rather the fights, agreements, and failures of the multiple players who move about a stage markedly influenced by what Jonas calls a prolonged civil war. Her interest in the democratic transition revolves around what happened at the end of the counterinsurgency. The first part of her book is a noteworthy account of how the peace process developed between the guerrilla forces and the government and the military. It is a journey that took a sinuous, ten-year path, and it leads Jonas, correctly, to question why at the end there were negotiations and how the dialogue between enemy forces led to the end of the war.

Susan Jonas is without a doubt the North American academician most familiar with the Guatemalan political scene. She has dedicated more than a quarter-century to studying this society carefully and at close range, a study that has resulted in five books. *Of Centaurs and Doves* is the end of a long, personal intellectual cycle that coincides with the end of a tragic period in the history of this country. The peace process is analyzed as a movement to define a new society, a better society. The difficulties and most especially the successes are emphasized. In fulfilling the peace agreements

setbacks arose, and the second part of the book deals with the negative role of the players Jonas calls the "dinosaurs."

Jonas analyzes the constitutional reforms as a decisive element in the construction of the peace, which she does with her customary precision. As for the "no" vote in the Popular Assembly in May 1999, she attributes more negative political effects than there were in actuality. This part of the book provides a careful, although somewhat pessimistic, analysis of the democratic possibilities, especially as concerns the army, comparing the situation to the experience of El Salvador. The rejection of the reforms is evidence of the antidemocratic character of the parties that constructed the democracy (the National Advancement Party and the Guatemalan Republican Front) and the racist culture of Guatemalan society. Jonas makes a comprehensive analysis of this entire process, including the role of North America, which promoted the war and then favored the peace. The author also describes the important part played by United Nations Verification Mission in Guatemala, the murder of Monsignor Gerardi, and the shadows thrown on the guilty ones, who enjoy impunity. The book ends by pointing out that the less drastic and more probable alternative is the institutionalization of uncertainty.

These two books are important because of the information they contain, but their different theoretical focuses are of diverse heuristic value. Both use personal interviews extensively. McCreary draws heavily on business people and others in the private sector, so the narrow vision of the CACIF ends up being hers. Jonas has a wider viewpoint; she interviewed all the opposing factions and did not allow herself to be won over by any. The interpretation she offers is more comprehensive. Her distinct conceptual perspective and the diverse sources of her data explain why, although the subject studied is the same, the results of the two books are different. Democracy has not been consolidated in Guatemala, as McCreary claims, but neither are we on the road to uncertainty, as Jonas proposes. We are in a period of contradictory learning, but we are making progress.

Creating the Zhuang: Ethnic Politics in China. By Katherine Palmer Kaup. Boulder, CO: Lynne Rienner, 2000. 221p. $52.00.

James E. Seymour, *Columbia University*

For communists, the "nationalities problem" has always been the major stumbling block to the realization of their social goals. Mao Zedong believed that it would take the longest to solve. "First, classes will be eliminated, then nations, and finally nationalities. Throughout the world it is the same" (p. 113). Inasmuch as policies on nationalities have been the Achilles' heel of the various Chinese communist experiments, it is surprising that it took so long for scholars, especially those in the West, to examine the mainland's largest ethnic minority, the Zhuang. Kaup's fine little book goes a long way to filling the void.

The Zhuang are especially interesting to political scientists because, to a considerable extent, this nationality is a result of political engineering. There have always been Zhuang, but before the 1950s they never loomed large on China's ethnographic landscape. How this was changed is interesting enough. How it all worked out, with unintended but understandable consequences, is simply fascinating.

In the discourse on nationality formation, Kaup places herself, or at least the Zhuang, largely within the formulation best articulated by David Laitin, according to which ethnic groups are constructs of (often hegemonic) state authority. "The Chinese case," Kaup argues, "is not simply one of the

government's creation of an incentive program for various nationalities. It is one of the state mandating which individuals would be considered part of which bounded and non-overlapping nationalities categories" (p. 20). Of course, it is not quite as simple as this, and Kaup employs various other theoretical approaches to explain the Zhuang phenomenon.

In 1958, when the province-level Guangxi Zhuang Autonomous Region was created, very few people thought of themselves as "Zhuang." As in Xinjiang, the communists found it in their interest to break down local loyalties and forge a fragmented population into a "nationality." The purpose has always been to weaken local loyalties, as opposed to the divide-and-conquer approach applied to the more robust, troublesome, and real Tibetan nation. Unlike other commentators, Kaup believes that pressing political considerations made it imperative to create this region: "Special policies were ... needed to integrate them into the greater Chinese state" (p. 53).

Such policies have suited communists well, but what have they done for the minorities? The illiteracy rate has declined but is still high, exceeding 50% in many Zhuang counties. The literacy problem is complicated by the many Zhuang dialects. "Despite the impressive figures on the number of books published in Zhuang, I did not once see a Zhuang book for sale the entire time I was in Guangxi and Yunnan" (p. 143). With little in their own language to read, literacy for the Zhuang means literacy in Chinese rather than their mother tongue.

Although data are difficult to obtain, it is well known that economic achievement among China's ethnic minorities is (with the conspicuous exception of Koreans) far below the national average. Guangxi is in the lowest 10% of provinces, and Guangxi's Zhuang are far poorer than the region's Hans (who are Chinese and are in the majority). Zhuang officials now complain that the new loosening of restrictions leaves the Zhuang simply "free to be poor" (p. 178).

There may not have been much reality to Zhuang nationality originally, but it became a sort of self-fulfilling prophesy, and there must be moments when the communists feel that they have created a Frankenstein's monster. "As we ... move into the new millennium, Zhuang elites are forming action groups to solidify ethnic solidarity and to pressure the government for a great number of preferential policies" (p. 21).

In the light of the breakup of the Soviet Union along ethnic lines, the nationalities in China, including the Zhuang, bear watching.

Unbroken Ties: The State, Interest Associations, and Corporatism in Post-Soviet Ukraine. By Paul Kubicek. Ann Arbor: University of Michigan Press, 2000. 275p. $49.50.

Robert S. Kravchuk, *Indiana University*

This year marks the tenth anniversary of Ukraine's historic Declaration of Sovereignty, and the country will celebrate its first full decade of independence in August 2001. Yet, Ukraine's record as an independent state has been disappointing at best. Even after ten years, living standards for much of the population continue to fall. Development of civil society has been slow, if nonexistent. Political parties are very much nascent institutions. Opinion surveys indicate high levels of frustration among citizens, coupled with extremely low levels of perceived political efficacy. The economy remains dominated by large, inefficient state enterprises and collective farms, dinosaurs left over from the former regime. In many respects, Ukraine appears unable to move forward;

the vision of its future has been shaped largely by its recent past.

This book addresses the mystery of why this country of 50 million, with a proud past, vibrant culture, and abundant human and natural resources (excepting cheap energy), should fail so miserably to develop a market-oriented pluralistic democracy. To answer this question, Paul Kubicek examines the structures, means of influence, and modes of organizational participation in post-Soviet Ukranian politics. He provides a highly detailed analysis of the relationship of organized economic interest groups to the state, an approach that has both academic merit and instinctive appeal. The effort focuses on the "institutional overhang" left behind in the wake of Soviet communism, a subject either downplayed or ignored in much of the literature, except in purely theoretical terms. The result is perhaps the most thorough-going examination yet of interest associations in a post-Soviet state, and its appearance is most welcome.

Most studies of postsocialist transitions privilege civil society, viewed by many as the lynchpin that brought down the USSR. The argument is that development of civil society is necessary for democratization and economic reform, but few have addressed the question of what fills the vacuum when civil society fails to appear, as in Ukraine. Kubicek challenges the notion that institutions of civil society will develop naturally as a consequence of political normalization in transition societies. Society may not grow strong enough to resist the power of the state. In Ukraine, however, the state also remains weak, so it can be described as neither monolithic nor authoritarian. Especially on key reform policy questions, certain organized interests have been able to frustrate the government's attempts at genuine political and economic reform. As Kubicek rightly points out, the result is a "rather paradoxical situation" in which the dominant Soviet-era elites have preserved or even consolidated their positions, but Ukraine nevertheless suffers from a prolonged crisis of power (p. 48).

The author characterizes Ukrainian politics as "weak corporatism," which derives from both a weak civil society and serious divisions among the ruling elite. The result is a process of interest intermediation that is "corporatist in content, if not in form" (p. 132). Institutionalized tripartism among state, business, and labor is weak, if it exists at all. Organized labor has been marginalized, whereas the largest business association, dominated by directors of large state-owned enterprises, has been invited into certain select governmental policy decisions. Agricultural interests continue to be dominated by directors of collective farms, and even though the usual corporatist structures are lacking, one group has been given exclusive rights to represent farmers and to receive and distribute state budget monies to farmers. In sum, economic reform has been hijacked by the powerful industrial and agrarian lobbies, whose positions have been sanctioned by the state. Clearly, the web of ties between the state and interest associations has affected the course of reform, and for the worse.

Kubicek concedes that certain peculiarities make it difficult to classify state-society relations as purely corporatist in Ukraine. He fairly states in his conclusion that corporatism "cannot be used as a term to capture the whole of the Ukranian reality" (p. 206). Political power is concentrated in a narrow circle at the top, the so-called Party of Power. But this is not a monolithic bloc, and its boundaries are not well defined. Rather, it is a somewhat amorphous group, many within it disagree about reforms, and its fortunes can change (and have) at the ballot box. He thus finds that Ukraine is not wholly an instance of "state corporatism" (dominated from

above) or of "societal corporatism" (power is shared). Furthermore, he finds that it is difficult to identify a single factor as the most crucial to explain corporatist tendencies in Ukraine. Kubicek is surely correct, however, that the primary goal of "Ukrainian-style (i.e., weak) corporatism" is to enable state control over interest associations that may threaten the existing social and political order. There is broad agreement among Ukrainian elites that proliferation of autonomous groups would produce political instability. Avoidance of social upheaval, perhaps leading to violence, is the "bedrock consensus" upon which the entire Ukrainian social order rests. No one wants it, and the populace appears willing to endure nearly anything to avoid it.

Kubicek boldly asserts that "the preexisting transitions literature, with its teleological assumptions and its focus on political parties and public opinion, needs substantial revision" (p. 198). Perhaps. But Kubicek's text also would benefit from some revision. The book is a dissertation and reads like one, which detracts somewhat from its substantive contribution. It is rich in detailed analysis but is lengthier than need be. There are relatively few citations for much of the detailed information presented. Finally, although the author mentions "path dependency" several times, he fails to define or describe it, and he does not rigorously argue for it. What the book does describe are certain inertial tendencies inherited from the 'former regime, which have been reinforced by a broad consensus to slow the drive toward a more pluralistic society.

The Art of Comparative Politics. By Ruth Lane. Needham Heights, MA: Allyn and Bacon, 1997. 147p. $32.46.
Political Science in Theory and Practice: The Politics Model. By Ruth Lane. Armonk, NY: M. E. Sharpe, 1997. 178p. $58.95 cloth, $27.95 paper.

Ronald H. Chilcote, *University of California, Riverside*

In both these short volumes, Ruth Lane assumes an optimistic stance, generally within the mainstream of political science, and attempts to synthesize past and present trends in an effort to show progress. She argues in *The Art of Comparative Politics* that, despite the disparate approaches, real advances have occurred within the field. Her interpretative essay focuses on the recent history of the field, with an assessment of the behavioral movement during the 1960s and subsequent emphases on development, state, grassroots and peasant politics, and the new institutionalism. In *Political Science in Theory and Practice* she affirms that a core consensus has appeared in the independent investigations of prominent political scientists. Thus, a coherent working model of political behavior guides political scientists to understand political realities. She argues that this concrete model coincides with scientific realism and the current understanding of a philosophy of science.

Lane's valiant effort to formulate a politics model attempts to resolve the prolonged struggle of political scientists to find coherence in the discipline. Lane argues that this framework is founded "not on human subservience to social, economic, or governmental institutions, but instead emphasizes the power of strategically situated individuals to understand, confront, and change circumstances" (p. 6). She defines the model in terms of multilevel interactive situations, individual decision makers, recognition of participant views and goals, interactions among individuals and the resources they employ, rights and duties in decisions about just distribution, and negotiations over the rules and institutions that distribute resources and rights. She insists that this model "strips

away surfaces and finds universal processes at the heart of events. While historical differences will mark these processes, the processes themselves are universal—they are what everyone calls "politics" (pp. 9–10).

The argument evolves through an elaboration of "concrete" rather than abstract theory as illustrated by mainstream examples of theorizing in various political research subfields. The summaries of this work help in defense of Lane's model, although the emphasis on similarities and conformities obscures attention to alternative possibilities. In this way, Lane demonstrates that we have moved beyond positivism and attention to universal laws and rigid emphasis on falsifiability and instead have adopted scientific realism and model-theoretic forms of explanation and the search for causal processes, so that "it now has become possible to qualify as a scientist without being a positivist" (p. 148).

Many of the examples that buttress the politics model also appear in the essay on comparative politics. Initially, Lane lays out definitions and her framework in a way that may be helpful yet also distractive to the student. The argument that comparative politics is a constantly evolving discipline suggests hope in finding direction among different approaches, and the stress on seeking theory to find order and explanation in the real world is an effort to rise above a level of description. Yet, the push toward a "new model" severely limits possibilities of reaching into the history of the discipline to uncover other approaches or to strike out in new directions. Presumably, the framework leads to a science of politics and comparative politics. Models, however, do serve generally to sort out ideas within a particular context, and they are subject to modification, especially when the conformism of mainstream and traditional political science is challenged. The push by Lane to define political behavior institutionally and the relegation of politics to an individual level not only reveal her preferences but also allow for understanding of trends that are presently popular.

Lane argues somewhat convincingly that comparative politics is the "essence" of political science and that a comparative politics that aspires to be a science is focused on understanding people, not judging them. She explains her emphasis on how political science can be a science by relating art to science: "If art is seen as the creative approach to reality, and science is seen as the discipline of inquiry into the real world, then the distinction disappears and art becomes an integral part of science" (p. 5).

Lane divides the history of comparative politics into four major periods: the behavioral revolution, the development movement, the return to the state, and individual choice. The latter period marked a tendency toward disillusion with the earlier trends. Each period is discussed in the light of mainstream contributions, successes, and weaknesses, but gaps are evident in the discussion.

For one, rather than identifying system and culture theory with particular periods in the historical evolution of comparative politics, Lane integrates them into her critique of behavioralism. She identifies David Easton as the spokesman of the behavioral revolution in political science and Gabriel Almond and Sidney Verba as the originators of behavioralism in comparative politics through their study of political and civic culture. Justifiably she criticizes Easton's theoretical abstraction, but she ignores his empirical work on schools and political socialization. Indeed, political socialization, once a major research area for political science and comparative politics, is overlooked altogether. She delineates Easton's "creed" of behavioralism without reference to his *APSR* presidential address, which acknowledged postbehaviorial counterinfluences in the discipline. In her elaboration of

interesting case studies that reflect the behavioral trends, the description reveals the results of Robert Dahl's important study of elite rule in New Haven. It overlooks, however, William Domhoff's followup, which is based on different questions to the same people interviewed by Dahl and reveals different understandings about rule and power in New Haven.

Gaps also appear in the attention to developmental theory. There is reference to Rostowian economic stage theory but no acknowledgment of A. F. K. Organski's futile attempt to apply such theory to politics. Lane reviews the criticism of structural-functionalism without linking it, for example, in the work of Gabriel Almond, to the underlying ideological belief that development appears only in systems of formal, indirect, and representative rule. What is missing in an otherwise instructive review of Samuel Huntington's notions of order in changing society is criticism of his reliance on authority and his conservative skepticism about revolutionary change for Third World society. In linking dependency theory with Marxism, Lane overlooks that few dependentistas actually relied on Marxism or failed to cite Marx. Furthermore, although Marx in the case of India emphasized the long path to socialism through development of the capitalist means of production, Lane ignores his writings on Ireland that come remarkably close to the dependency perspective.

The characterization of dependency theory as "vulgar Marxism" belies the fact that writers such as Paul Baran, André Gunder Frank, Theotonio dos Santos, and others all wrote about dependency and underdevelopment in defiance of distorted sectarian positions advocated by traditional and orthodox communist parties. These writers captivated intellectual interest about why capitalist development was not occurring in the Third World, and their views eventually became influential in mainstream North American social science. If, as Lane argues (p. 71), both Adam Smith and Karl Marx failed to outline a developmental path, leaving the task to comparative political scientists to find a different way, then it would seem that we might benefit from a close look at development in terms of capitalism and socialism as economic systems that dramatically affect politics.

Likewise, Lane's discussion of the return to the state emphasizes the work of Evans, Rueschemeyer, and Skocpol during the early 1980s without recognition of the seminal work of European political scientists, such as Ralph Miliband, Claus Offe, and Nicos Poulantzas, who elaborated a theory of the state a decade earlier. Also, Lane does not take us back to the origins of American political science, which was deeply influenced by the notion of state and formal notions of government and state in nineteenth-century German literature. Her focus on more recent theory of the bureaucratic authoritarian state overlooks the extensive literature and debates around corporatism that preceded it.

Finally, the interesting review of the new institutionalism turns to studies of the peasantry, grassroots, resistance, and rebellion. Lane elaborates on the history, principles, and influence of rational choice in the new institutionalism and sets forth an institutional model. A concluding chapter reviews various theories in an effort to justify her assertion that comparativists, working on their own over the years, have unconsciously put together the politics model she so carefully delineates throughout the book. These lapses should not deter comparativists from delving into these two interesting works which attempt to look at traditional and current thinking in new and challenging ways.

Privatization South American Style. By Luigi Manzetti. New York: Oxford University Press, 2000. 373p. $74.00.

M. Victoria Murillo, *Yale University*

Luigi Manzetti fills an important gap in the literature on market reforms in Latin America by providing a comparative analysis of privatization in Argentina, Brazil, and Peru. He engages the literature on economic reform in developing countries by focusing on the implementation of this single policy and complements a burgeoning scholarship on the economics of privatization in the region. The main contribution lies in underlining the relevance of political factors for explaining the success of privatization policies. The "South American" style, he suggests, reinforces the view of those who, like John Williamson and Stephen Haggard ("The Political Conditions of Economic Reform," in Williamson, ed. *The Political Economy of Reform,* 1994) and Guillermo O'Donnell ("Delegative Democracy," *Journal of Democracy* 5 [January 1994]: 53–69), associate the rapid implementation of market reforms with the concentration of executive authority at the expense of the checks and balances of liberal democracies.

The most extreme cases in this trend are Peru, with the self-staged coup of President Alberto Fujimori and the dismissal of nonsubservient judges, and Argentina, where President Carlos Menem packed the Supreme Court with political loyalists (p. 322). Nonetheless, Manzetti recognizes the importance of political bargaining for building coalitions in support of privatization. In particular, his comparison between the success in Brazil of President Fernando Henrique Cardoso's privatization efforts relative to those of his predecessor, President Fernando Collor, highlights the importance of bargaining in building a proprivatization coalition (chap. 4).

Manzetti proposes an analytic framework that incorporates many of the variables mentioned by the literature on economic reform in developing countries. The five main concepts are: the willingness to privatize, the political opportunity to do so, the government capabilities to implement the policies, the political responses generated by the decision to privatize, and the technical difficulties for accomplishing the process. In addition, the author presents a rich empirical description of privatization efforts by presidents Raúl Alfonsín and Carlos Menem in Argentina; presidents Fernando Collor, Itamar Franco, and Fernando Henrique Cardoso in Brazil; and presidents Fernando Belaunde Terry, Alan Garcia, and Alberto Fujimori in Peru.

Although Manzetti compares privatization experiences across time and across countries, he could have used the rich information in the empirical chapters for comparisons across or within sectors. That approach could have answered other comparative questions, in addition to the success of privatization efforts, such as those regarding the type of privatization. Why was electricity privatized without vertical integration as a strategy to promote competition in Argentina and Peru, whereas telecommunication companies were privatized as monopolies in both countries? Why did Brazil, in contrast, include competition from the onset in the privatization of telecommunications? Although implicit comparisons are made across privatization cases in different countries, an explicit analysis of sectors might have yielded answers to this type of question. In fact, variables that Manzetti examines— such as the effect of different bureaucracies, fiscal emergency, and interest groups—influenced the decision on how to privatize. His cross-national comparisons lay the ground for future studies to undertake this task.

The organization of the book makes it difficult to test the

analytical model, especially for readers who are not country specialists. Often, too much empirical description leaves little space for an extensive discussion of alternative explanations. The author begins with a thought-provoking and easy-to-follow list of plausible hypotheses to be tested, but his summary of findings would have been clearer if he had employed a similar analytic device in the concluding chapter. A graphic summary of the qualitative measures for each case study would have highlighted the explanatory power of the model vis-à-vis alternative hypotheses, even if no quantitative measures could be provided. In addition, the use of alternative research designs, such as Charles C. Ragin's Bayesian method (*The Comparative Method,* 1986), would have allowed Manzetti to state more clearly which of his five variables are either necessary or sufficient for the success of privatization.

This detailed comparative analysis is a point of departure for future studies on privatization in the region. Manzetti illuminates the politics of a policy that reshapes developmental states and modifies the ways in which the state is used for political goals, such as patronage or subsidies, in many developing countries (e.g., Robert Bates, *Beyond the Miracle of the Market,* 1991). Building upon a field characterized by single-country monographs and a literature that usually is too technical, Manzetti provides a comprehensive synthesis of various explanations of the success of privatizations in South America while emphasizing the political character of such processes.

Policy Representation in Western Democracies. By Warren E. Miller, Roy Pierce, Jacques Thomassen, Richard Herrera, Sören Holmberg, Peter Esaiasson, and Bernhard Wessels. Oxford: Oxford University Press, 2000. 180p. $65.00.

Valerie R. O'Regan, *North Dakota State University*

Those who study the concept of representation are undoubtedly familiar with the 1963 study by Warren Miller and Donald Stokes ("Constituency Influence in Congress," *American Political Science Review* 57 [March 1963]: 45–56), which had a profound effect on scholars' understanding of the relationship or "congruence" between representatives and constituents. Others (see Sidney Verba and Norman H. Nie, *Participation in America: Political Democracy and Social Equality,* 1972; Heinz Eulau and Paul D. Karps, "The Puzzle of Representation: Specifying Components of Responsiveness," in Heinz Eulau and John C. Wahlk, eds., *The Politics of Representation,* 1978) have made their own distinguished contributions by venturing to conceptualize and measure representation in an effort to further our understanding of the relationship between the representative and the represented. In the same mode, this collection of articles contributes to the study of the mass-elite relationship by providing a variety of approaches, methods, and measures to broaden the literature.

In the past, work frequently has been limited to single case studies of various countries, including France (Philip E. Converse and Roy Pierce, *Political Representation in France,* 1986), Sweden (Sören Holmberg, "Political Representation in Sweden," *Scandinavian Political Studies* 12 [March 1989]: 1–35), and the United States (Richard F. Fenno, *Home Style: House Members and Their Districts,* 1978), among others. Difficulties regularly associated with cross-national research, such as unavailable and noncomparable data, have hindered attempts at comparative analysis. The focus on specific cases,

however, limits our understanding of any cross-national similarities and differences.

The central contribution of *Policy Representation* is its thorough, comparative analyses of five industrialized democracies. They represent various types of systems (presidential and parliamentary, two-party and multiparty, majority and party list) found in democracies throughout the world. In addition, the contributors adopt different approaches that incorporate basic theories of representation, individual and aggregate data, diverse measures, as well as innovative and established methods. Similarities can be found among some of the chapters in the application of certain models as well as the significance of communication between elites and masses in the representation process, which was a critical point made by Miller and Stokes in 1963.

An introduction explains the motivation and circumstances that resulted in this collection, followed by six different perspectives that draw on diverse theories, measures, and methods. The second chapter explores the linkages between issue positions of voters and political parties by applying the responsible party model of representation. An original measure, Pierce's Q, is used to evaluate the mass-elite issue linkages in the five political systems. In the third chapter, two models of representation are applied in the study of issue congruence, and emphasis is on the usage of the left-right dimension in communications between the masses and political elites. Chapter 4 analyzes the political discourse and levels of understanding between masses and elites. In chapter 5, the Galtung system of distribution curve shapes is used to analyze collective policy congruence. The sixth chapter examines the occurrence and effects of geographical distinctions in voters' policy views. Chapter 7 examines the relationship between the different political systems and policy representation based on the characteristics inherent in the systems. The book concludes with an overview of the findings from each chapter.

Each chapter takes a distinct approach to the subject. In most cases, the authors present their arguments, methods, and analyses logically and with originality and clarity. A couple were challenged with unexpected results that they thoughtfully explain and point to as foundation for further cross-national research on mass-elite linkages. One slight criticism involves the explanation of the curve shape analysis of mass-elite congruence. The author is so immersed in explaining methods that some confusion is created. Overall, however, the book is well written and interesting. In addition, some of the chapters offer innovative methods, such as Pierce's Q, that will advance comparative research on policy representation.

In addition to the six different approaches that can be developed in future research, a major contribution of this work is the comparative format. All the chapters incorporate data sources that allow the authors to provide a truly comparative analysis of mass-elite linkages. The study also produced a noteworthy international data pool, and it is hoped this will be expanded as future research incorporates other countries into the policy representation analysis.

Ultimately, "policy representation is a multifaceted phenomenon" (p. 111). This book does a fine job of educating the reader on the complexity and difficulty in determining what policy representation entails. Furthermore, the authors make a significant contribution to the comparative study of the subject. Those who are intrigued and challenged by the concept of representation will find this book to be an interesting piece of the policy representation puzzle.

Money, Markets, and the State: Social Democratic Policies since 1918. By Ton Notermans. New York: Cambridge University Press, 2000. 312p. $59.95.

Carles Boix, *University of Chicago*

Notermans has written a bold and ambitious book in which he purports to explain the conditions under which social democratic policies, and therefore the social democratic project, have been successful in modern democracies. The book, which relies heavily but not exclusively on historical data, examines the ebb and flow of social democratic dominance in five countries—Germany, the Netherlands, Norway, Sweden, and Britain—since roughly the introduction of (male) universal suffrage after World War I.

What constitutes a social democratic program has been a regularly contested issue among political economists. To Notermans, social democracy requires, above all, the pursuit of accommodating monetary policies. Several reasons are advanced to support that choice. In contrast to a policy merely oriented toward price stability, an expansionary monetary strategy makes full employment possible, which maximizes the welfare of traditionally social democratic electorates. More important, and in line with the insights of postwar consensus literature, an expansionary monetary policy reconciles the divergent interests of both workers and employers. Probably for that reason, any reference to the expansion of the welfare state as a key contribution to social democracy is rather muted in the book. The zero-sum nature of purely redistributive policies was the stumbling block of social democracy in the 1920s. It was only when social democrats could deliver growth, through cheap money, that they could make redistribution palatable to all social sectors. In short, expansionary monetary policy is the essence of social democracy because it constitutes the precondition to overcome a potential trade-off between equality and efficiency.

The changing fortunes of the social democratic program throughout the twentieth century are examined, in vivid and fresh detail, in the three central chapters of the book. Notermans first explores why there was no social democratic breakthrough in the 1920s—this is probably one of the best parts of the book, giving original historical information on a period (pre-Keynesianism) that has been less well examined in previous work on the adoption and diffusion of demand management ideas. Notermans then delineates the successive steps that culminated in the golden age of postwar social democracy: labor and agricultural markets stopped being neoclassical, the gold standard collapsed, and, after World War II, Bretton Woods embedded interventionist domestic policies in a common international architecture. The triumph of social democracy started to unravel in the 1970s when inflationary tensions finally became unmanageable, but, as the book shows in a pristine way, the tempo varied substantially across nations. Keynesianism was rapidly abandoned in Germany and the Netherlands and came to a very abrupt end in Britain in 1979. By contrast, Scandinavian governments remained committed to accommodating policies for another decade, although there, too, the commitment to Keynesianism eventually became almost impossible to maintain.

The historical inquiry of the book is always bound by a theoretical tension to understand the causes underlying the growth and fall of the social democratic program in the last century. Three types of explanations are rejected: ideational factors, understood as the emergence of a set of technical beliefs that convinced policymakers; shifts in the electoral arena; and the internationalization of the economy. Instead, Notermans relies on the combination of what can be called an exogenous shock argument and an institutionalist explanation. The book points out (in a less explicit way than I would have liked) that fundamental shifts in policy regimes (especially from neoclassical economic policy to Keynesianism in the 1930s, but to some extent from the latter to monetarism in the 1980s) took place almost by default. The impression one gathers is that politicians (conservatives and socialists alike) always resist jumping from the old ship until they are literally forced to do so by events and catastrophes much superior to their forces. Once they are in a new territory, they rapidly cling to it until a new shock shatters the prevailing equilibrium.

Why both regimes (neoclassical and Keynesian) have not been completely stable is attributed by Notermans to domestic institutional conditions. Following the neocorporatist canon, he acknowledges that no expansionary policy can survive without moderate wage agreements (which in turn require certain types of unions and businesses). The explanation is compelling—it fits the neocorporatist agenda but gives it a much needed historical perspective—but it may be insufficient for two reasons. On the one hand, explaining why Keynesian policies fail does not address the question of why an orthodox monetary regime may break down, and this is not tackled in the book other than by saying that orthodoxy is always susceptible to be endangered by a deflationary "cumulative process" (p. 32). Of course, one can always maintain that expansionary policies are the natural regime of any democracy (due to popular demands for full employment), yet the short-run nature of the Phillips curve periodically leads to the breakdown of demand management. The problem is that this explanation is belied by the long-run success of non-Keynesian recipes in several contemporary democracies. On the other hand, the book omits discussing an important part of the corporatist literature: Wage pacts can only be sustained over time if some sort of political (and budgetary) exchange is enforced. In other words, Keynesianism goes hand in hand with either the construction of welfare states or the development of microeconomic policies to increase workers' skills and productivity.

Although the book is a very rich story of social democracy, there are three aspects that remain problematic. First, not all social democratic governments have followed Keynesian recipes to achieve full employment and equality goals. With the aid of the literature on market failures and, later, new growth theory, the construction of the welfare state has been justly defended for its positive effects on both growth and welfare. Similarly, supply-side policies, oriented to the creation of physical and human capital—to make sure that the marginal productivity of labor increases in such a way that equality is not detrimental to efficiency considerations—has been systematically used by the Left. Note the application of Rehn-Meidner plans in Sweden or the expansion of the stock of public capital in France and Spain in the 1980s under a regime of price stability.

Second, Notermans does not explain well how internationalization affects Keynesianism, particularly in the last decade or so. Although the process of internalization is not a deus ex machina, we should recognize that what other countries did and how capital markets changed affected the judgments social democratic regimes made about the economic and political feasibility of their expansionary policies. Their decision to dismantle in the mid- and late 1980s the set of capital controls and financial regulations that had helped them to sustain moderate (and relatively stable) levels of inflation was not simply a random event or the result of miscalculations (Notermans is not very precise about the causes of deregu-

lation). Social democratic policymakers probably calculated that, in the wake of extensive deregulation in other areas of the world, they could not risk being left out of the long-run benefits of growing capital mobility.

Third, electoral politics is strangely missing in the book, especially in view of the attention paid to the putative decline of social democracy at the polls in the 1980s and early 1990s. If, as the author claims, ideational considerations did not affect the choice of different policies, then material interests necessarily explain why governments changed their policies. These interests necessarily worked through the ballot box. Once certain institutions (or the lack of them) did not work to achieve noninflationary growth, Keynesianism was abandoned either because voters switched to conservative parties or because socialist politicians themselves changed in anticipation of the reaction of voters. But elections and electoral decisions as the mechanisms of change and, more generally, the question of which were the past and which will be the future political bases of social democratic parties are only given short notice in the book.

Gender Matters: Female Policymakers' Influence in Industrialized Nations. By Valerie R. O'Regan. Westport, CT: Praeger. 2000. 168p. $49.95.

Georgia Duerst-Lahti, *Beloit College*

Political globalization makes cross-national comparisons ever more important, especially those that reach widely and over time. Such policy analysis offers potentially practical applications for improved democratic representation. Valerie O'Regan provides such analytic reach in her study about the influence female policymakers have achieved in 22 industrialized nations. She considers the effects and effectiveness of female legislators and executives in representing women's interests. The central thesis, that gender differences in policy priorities will be reflected in policy outcomes as the number of female policymakers increases, is supported. The secondary thesis, that (wage) policy comprehensiveness will improve, is not.

The study is commendable on several fronts. O'Regan provides a thorough venting of contradictory definitions and approaches on most key concepts considered, including alternative ways to define women's issues and policy responsiveness as well as the varied approaches to analyzing policy influence. Change over time can be considered because the data cover an unusually long span. Unlike most such studies, she is able to construct and compare conditions and policies from 1960 to 1975 with those since the women's movement reignited, from 1976 to 1994. Her sample of 22 nations captures more of the industrialized world (e.g., Joni Lovenduski, *Women and European Politics*, 1986) over a longer period (e.g., Pippa Norris, *Politics and Sexual Equality*, 1987) than other endeavors. It also updates knowledge about a rapidly changing field. It includes both qualitative and quantitative analysis, which enriches the study and overcomes some weaknesses. The approach to this prodigious undertaking is intelligently thorough, although it is not without some flaws.

O'Regan groups the 22 nations into six categories according to region and policy similarities. The categories, such as North American nations, Mediterranean nations, and Scandinavian nations, make sense, but Japan awkwardly stands alone as its own category. The study provides a great service by pulling together equal wage policies as they evolved over the two periods for all the nations. These policies are usefully summarized in Figure 4.1. Each nation's policy is considered

for its orientation toward equal pay for equal work, equal value, or comparable worth as well as for jobs covered, policy targets, constitutional basis, and more.

Two sets of quantitative analysis pursue the presence of women's wage and social policies and the comprehensiveness of wage policy in pooled time-series analysis. Many facets of this analysis demonstrate adroit conceptualization. By adopting the notion of "nation years" (p. 41), O'Regan increases the number of observations: Each of the 22 nations is considered 35 times, thereby building more robust data. She controls for the right conditions throughout the analysis. For example, in the first analysis on social policy, abortion is treated separately, and in the second analysis on wage policies, she excludes the four nations whose wage policy is determined by collective bargaining. Factors such as percentages of Catholics, socialists, and females in the labor force and of childbearing age become variables. In several instances she drops particular nations because of unique circumstances. With her large sample, the analysis can proceed unhindered by these wise exclusions.

The central results confirm the importance of more female policymakers to the existence of women's employment and social policies, and some tantalizing surprises emerge in the findings. As expected, all nine regressions show that an increase in female policymakers is associated with policies beneficial to women. Also as expected, a higher proportion of women involved in unions and collective bargaining policy proves beneficial to policy. The finding that "female heads of government were more likely to have social policies and less likely to have employment and wage protection policies" (p. 98) than their male counterparts needs better explanation. Other surprises are that more policies on behalf of women are associated with a stronger Catholic influence, and conservative parties in power.

The analysis on the comprehensiveness of wage policy proved poor for women's representation. Only two variables were significant as predicted: nations with stronger economies and more women in their childbearing years. Contrary to predictions, nations with a higher percentage of female policymakers were less likely to produce progressive equal wage policies, with all other variables showing as significant in the opposite direction. O'Regan explains these negative results most of all as a methodological failing in the construction of the scales. This failure and her explanation of the contrary findings point to some of the study's shortcomings.

First, coding requires simplification of complex policy realities so that only a limited range of insight can be garnered. The first set of analysis coded merely whether a policy existed. It could not capture more about the nature of the policy or the quality of its implementation. The second analysis attempts to capture something more complex, and it does not succeed. The inconsistency in interpretation depending upon the success of the hypotheses nag at the validity of both analyses.

Second, and more fundamental to the study, although the data run through 1994, very little literature beyond 1990 is used. As a result, the explanations cannot benefit from the extensive development in gender theorizing since then. For example, never does the author suggest that the women who occupy policymaking seats, especially as heads of government, must "perform" the masculine gender despite being biological females. There is no reference to varying gender ideologies, but there is a real possibility that the particular women who have succeeded might well believe in gender universalism so support such policies. This advanced understanding of gender theory would explain apparent inconsistencies at least as well as problems with coding. Finally, the

book reads somewhat like a dissertation, although it also offers the benefits of being straightforward and thorough.

Scholars of comparative politics who care about democratic representation and policy should read this book, not only scholars of women or gender. Much can be learned from it.

Islam in Contemporary Egypt: Civil Society vs. the State. By Denis J. Sullivan and Sana Abed-Kotob. Boulder, CO: Lynne Rienner, 1999. 159p. $49.95.

Sami Zubaida, *Birkbeck College, London*

"Civil society" has become a key concept and a central quest in the search for paths to democracy and liberty in many parts of the world. This search has been particularly notable in Egypt, where an increasingly totalitarian state has sought in recent decades to project an image of democracy but at the same time attack and undermine all potential bases of social autonomy and political action. These are the central issues discussed in this book. The picture is complicated by the prominent part played by religious and religio-communal politics on the Egyptian stage. Are Islamic associations and forms of political action forces of civil society engaged in the quest for social autonomies and liberation from authoritarian strictures, or do they themselves add another tier of repression in the name of religious conformity and moral conduct?

The authors argue, quite correctly, that there are many different kinds of Islamic movements and tendencies in Egypt, with various orientations and programs. Many of them, especially the nonviolent political strands of the Muslim Brotherhood, are seen as forces of civil society, working for pluralism and democracy. The various religious charities and social services associations, some of them linked to the Brotherhood, others ostensibly nonpolitical, clearly form a vital component of the active nongovernmental organization (NGO) sector in the country. Islamists are also active in many spheres of public life, crucially in the professional syndicates of doctors, lawyers, and engineers, which they controlled at one point, until government intervention dislodged them. On the whole, the authors present a rosy picture of "moderate" Islamism, as a benign force for civil society and democracy against an authoritarian state. This is enhanced for them by the fact that the Muslim Brotherhood is the one oppositional political current with a broad social base and political constituency, in sharp contrast to "secular" parties, which are mostly feeble talking shops.

In common with the bulk of the literature, the authors identify civil society in voluntary associations, NGOs, charities, parties, and syndicates, and they note the prominent input of Islamism into these sectors. They point out the extensive volume and vitality of these associations in modern Egyptian history, despite close regulation by the authorities, who use wide-ranging, catch-all decrees that allow them to intervene in the direction and finances of these organizations as well as prohibit or even merge them at will. There are many scandalous examples, notably the feminist organization that was closed down and its assets passed to a religious, state-controlled women's charity. In the early 1990s the government issued equally arbitrary decrees to control professional syndicates and exclude the predominantly Islamic elected executives. It would seem that, far from being bases of social autonomy, these formations are constantly at the mercy of a capricious authority, concerned primarily with eliminating political opposition and tolerant only of associations and actions it controls, or that pose no challenge.

The diversity of the Islamic movement noted by the authors extends to the Muslim Brotherhood itself (which they tend to present as a unitary "moderate" movement). A crucial divergence at present is generational, between the conservative old guard and a younger more modern intellectual elite, the generation of the 1970s student movement. The quest for some measure of democracy and pluralism, attributed by the authors to the whole movement, may be more realistically found among the latter, who espouse political and social programs (mostly developed from earlier nationalist and leftist projects), as against their conservative elders, who take a predominantly moralistic and authoritarian stance. The authors cite many examples of Islamist intellectuals calling for democracy and pluralism as well as women's rights. The language of democracy, however, is spoken by most if not all sectors of Egyptian politics, including the president and the political directorate, but the actions and policies of most belie these proclamations.

Islamists of all tendencies have not shown particularly liberal attitudes in dealing with challenging opinions and have always been ready to censor and denounce any cultural product they deem at variance with the true religion. The secularist publicist Farag Fauda was assassinated by violent militants, who were subsequently defended in court by a prominent figure of the Brotherhood on the ground that apostasy is punishable by death. The persecution of "apostasy" (as defined by the accusers) has been extensively pursued in recent years, most notably in the case of the academic Abu Zaid, who writes textual critiques of canonical sources. He was brought to court by Islamist lawyers associated with the Brotherhood, who demanded that he be divorced from his wife, because an apostate cannot be married to a Muslim! Eventually their petition was granted by a higher court, but its execution was stymied (not reversed) by government maneuvering.

Other censorship campaigns have purged university libraries of a wide range of books judged harmful to religion and morality and have led to the banning of many publications, restrictions on performances and exhibitions, and attempts to censor or ban films. These acts are carried out by respectable "moderate" Islamists in high positions in ministries, the media, and legal institutions. This type of conservative Islamism, which seeks the moralization of public life rather than political reform, is the dominant trend in Egypt, and its networks and personnel span government and opposition, business and the professions. As one commentator has asked, is this society civil? Because this book glosses over these negative aspects of Egyptian society, it overlooks the deep complicity between the authoritarian state and many of those it rules.

Transitions from State Socialism: Economic and Political Change in Hungary and China. By Yanqi Tong. Lanham, MD: Rowman & Littlefield, 1997. 280p. $69.00 cloth, $26.95 paper.

Tianjian Shi, *Duke University*

Tong addresses one of the most important issues in comparative politics: What are the key factors that determine the courses and outcomes of transitions from state socialism? The book makes three contributions to this field of study. First, it reminds us that the relationship between state and society with regard to either power or objectives may not be zero-sum, as widely accepted by students of politics. The author argues that state and society may share a wide range of objectives, laying the groundwork for a cooperative rela-

tionship. This helps explain why the regime and a society may compromise in the transition process.

Second, Tong incorporates political culture in an innovative way. To her, reform of state socialism involves not only economic and political transitions but also an ideological transition. Reform usually starts with a break from orthodox communist ideology, but its replacement is not at all clear. Even though many people would prefer to see liberalism selected, deeply rooted cultural values can significantly influence the choice. More important, since the ideological transition is usually among the last to be accomplished, the absence of a value system that can provide norms for social behavior impedes the other two transitions. Democratic and market institutions require an appropriate cultural foundation if they are to function properly.

Third, since the transition involves a shift from state dominance to more societal autonomy, neither the state-centered nor the society-centered approach can fully explain the process. Tong suggests instead a "transitional" approach that focuses on both the state and society and on their power relations, which are determined at least in part by their mutual interaction. The approach is dynamic, for it pays attention to the objectives and resources that state and societal actors bring to their interactions. Because state policies can empower certain social groups by granting them access to resources that the state controls, the state's decisions on social, economic, and political matters will create new interests among the sectors of society affected by those decisions.

The comparison of reform experience in Hungary and China shows that the growing crisis of legitimacy stimulated regimes in both countries to launch economic reform. Its implementation called for a certain political flexibility to allow intellectual innovation and popular participation. In both societies, the reforms created complicated pressures for political change. By departing from traditional conceptions of economic structure, the reforms continuously challenged the previous notion of socialism and demanded its further redefinition. Moreover, economic reform increasingly erodes the ability of the state to control society in the old way. Finally, economic reform led to inflation, inequality, and corruption, which produced increasing and contradictory political pressure on policymakers to slow the pace of reform for the sake of stability and to speed it up to hasten completion of the process.

In both countries, socioeconomic grievances produced societal challenges to the political establishment. The communist parties in Hungary and China made considerable efforts toward accommodation. In comparing the forms of adaptation in both countries, Tong found an observable difference in the level of institutionalization. In Hungary the trade unions had a more routinized role in decisions concerning workers' interests than did their counterparts in China. Both the electoral and legislative reforms in Hungary were also more institutionalized. In China, there were few sustained institutional changes. To Tong, the traditional political culture in China, which emphasizes consultation over formal accountability, largely explains this critical difference.

When political and ideological liberalization permitted the emergence of a popular opposition movement that could express its grievances more openly in these two societies, the political leadership in both China and Hungary split over the speed and extent of reform, whereas the opposition movements divided into moderates and radicals. In Hungary, a coalition emerged between the reformers in the leadership and the moderates in the opposition, which produced an agreement on free elections and a noncommunist govern-

ment; in China, radicals seized control of the antigovernment protests in Tiananmen Square, and conservatives seized control of the party establishment. The result was the crackdown on demonstrators in 1989.

For Tong, the critical question is why the reformer-moderate coalition formed in Hungary but not in China. Through a structured, focused comparison, she identifies the following differences in these two societies: (1) the relative strength and sources of conservatives and reformers; (2) the degree of compatibility between the objectives of the establishment and those of the opposition; (3) differences in strategy and structure between the two opposition movements; and (4) the different contexts within which the confrontation occurred. The successful coalition in Hungary was due to a reliance by conservatives on support from the Soviet Union, which proved unreliable; the compatibility between the objectives of the reformers and the opposition; the relatively restrained strategies adopted by the opposition; and the ability of all the political forces to conduct pragmatic negotiations in an institutionalized setting. Such a coalition never formed in China because of the relative strength of the conservative forces in the leadership, the incompatibility between the aims of the reformers and the opposition, the radical moral and emotional orientation of the protest movement, and the intransigence of party hard-liners and the radical opposition alike.

In sum, the book is methodologically sophisticated, innovative, and information rich. It reveals the complicated dynamics of transition, especially the institutional logic that is generic to a shift from state socialism. It should be read both by China specialists interested in reform processes in that country and by general comparativists interested in the dynamics of transition.

After the Deluge: Regional Crises and Political Consolidation in Russia. By Daniel S. Treisman. Ann Arbor: University of Michigan Press, 1999. 262p. $57.50.

Thomas F. Remington, *Emory University*

Daniel Treisman offers an ingenious explanation for the fact that the Russian Federation held together after the collapse of the Soviet regime. Unlike the three other ethnic federations in the communist world—Yugoslavia, Czechoslovakia, and the USSR itself—the Russian Federation, which was the largest of the 15 nationally based constituent republics in the Soviet Union, avoided disintegration. Many observers in the early 1990s feared the same pressures that had led to powerful separatist movements among the Soviet republics would prove too strong for the fragile central government to resist. Yet, Russia managed to maintain itself as a federal state, albeit weak. The one constituent republic in which separatism ultimately led to armed confrontation was Chechnia, where a brutal war began in 1994, paused in 1996, and erupted again in 1999.

Chechnia was a tragedy, as Treisman observes, but it was also an anomaly. Russia's federal forces launched a military offensive there after the wave of regional challenges to the central authority had subsided, so the ebbing of that wave cannot be ascribed to the deterrent effect of the invasion. Therefore, a different explanation must be found for the center's ability to establish a rough but workable system of federal relations with its own restive regions. Treisman argues that the answer may be found in the center's adroit use of selective fiscal appeasement, by which it blunted the edge of the most rebellious challenges through a mixture of tax breaks, credits, and budget allocations. Rather than fuel

demands for greater autonomy, Treisman shows, these fiscal transfers translated into higher per-capita spending in the targeted regions. This in turn boosted public support both for the Yeltsin camp in Moscow and for regional leaders.

· There are three elements to Treisman's account. First, by selectively accommodating the most credible rebels among the regional leaders (even at the expense of allowing friendly regions to lose out in the fiscal federalism game), the center had more resources with which to punish less credible challengers. Second, the center's transfers enabled regions to increase per-capita spending in the social sector, an area largely under regional jurisdiction and chronically under-funded. Third, as public support grew in the targeted regions, local leaders were likelier to support Yeltsin and his fellow democrats in their collisions with the communists at the center.

A key test was the fateful showdown in September–October 1993, when Yeltsin dissolved the old parliament and called for new elections. Opposition-minded leaders in the Duma refused to go along, and both they and Yeltsin appealed for support from local leaders. In regions that had benefited from federal transfers and (presumably as a direct consequence) there was public approval of Yeltsin, governors read the poll results and supported him against his enemies. Treisman shows that the use of federal transfers to fuel regional social spending tied governors directly to the Krem-lin, implicating them with Moscow in the public's eyes. Therefore, they were more likely to try to maintain a cooperative relationship with the center.

Treisman's thesis is counterintuitive in many respects and is derived directly from a simple formal model of relations between a center and a set of federal units. It assumes a certain disposition to support challenges to the center in each region, and the center's resources for quelling challenges are finite. If the center can funnel rewards to a few regions where the public disposition to support challenges is greatest, and where threats to secede are therefore the most credible, then it becomes riskier for other regions whose rebelliousness is less credible to challenge the center.

The theory is tested empirically using an impressive body of data on the flows of resources between the federal center and the regions in the 1990s. Piecing together these records was a considerable achievement, considering the rapid change in Russian fiscal relations in the early 1990s and the spottiness of recordkeeping and reporting. Treisman also generalizes his argument to other cases, showing that Rus-sia's strategy for defeating disintegrative pressures through the selective use of fiscal levers was precisely what the three other communist ethnic federations failed to do. In Czecho-slovakia, Slovakia lost ground in its relations with the center in 1990–91; so did Slovenia and Croatia in Yugoslavia in the same period; so did the Baltic states in their relations with the USSR government. This is not to say that different policies would have prevented these states from breaking away, but it tends to support the proposition that Russia, faced also with serious separatist pressures, took a very different path and held together.

Treisman's account of fiscal federalism in Russia is consis-tent with other studies treating constitutional arrangements that divide power, such as separation of powers and federal-ism, as inducements to continuous bargaining among political actors. Leaders with competing electoral mandates use the institutional resources available to them to obtain the best possible terms before ultimately reaching agreements that enable them to benefit from cooperation. Conflict, including much ethnic nationalism, thus is not an indication of system

breakdown or irreconcilable differences but is an inevitable part of the give and take of the political process. The originality of Treisman's study lies in showing that Russia's leaders, whether through strategic calculation or dumb luck, happened upon a formula that enabled them to preserve Russia as a federal state at a critical moment. Treisman presciently concludes by suggesting that it should be the first priority of Yeltsin's successor to recapture administrative control over the federal government's own fiscal authority from lower-level governments. By his actions in summer 2000 to recentralize power at the expense of the regional gover-nors, President Putin seems to have acted on Treisman's advice.

Feminists and Party Politics. By Lisa Young. Vancouver: University of British Columbia Press, 2000. 227p. $75.00.

Jill M. Vickers, *Carleton University*

This comparison of the relationship between organized fem-inism and partisan politics in Canada and the United States addresses two questions. First, Young asks how much orga-nized feminism has influenced partisan and electoral politics in each country. Second, she asks how political parties in each country have responded to organized feminism. She answers these questions by examining the relationship between each country's largest feminist organization and its party system and by showing how each relationship changed between 1970 and 1997. The result is an important and readable book that demonstrates the value of feminist political science as an approach, especially in comparative politics. The book is head and shoulders above many other texts about feminist political activism, mainly because of Young's ability to bridge between feminist ideas about politics and the comparative politics literature about political opportunities.

Many women question why they should participate in electoral politics when parties and legislatures seem to be dominated by elite men who use their position to advance their own interests. Young poses a somewhat different ques-tion. Rejecting the radical feminist thesis that parties and legislators are inherently patriarchal, she argues that, if they are historically male dominated, they can be transformed under some circumstances. What circumstances make trans-formation possible? Young does not assume that it is simply a matter of women choosing to participate or that their participation will always be translated into feminist policy outcomes. Indeed, she demonstrates that even different forms of democracy present women with different political opportunities because of their structures, practices, and values. Although organized women may choose or reject participation in partisan politics because of their ideological position, collective experiences with their own political sys-tem also shape their stance. When women face a political opportunity structure that offers limited access, their views about parties and the electoral system will be less optimistic than when the structure is more open. Despite women's struggles to establish the principle that "democracy without women is not democracy," therefore, it is still assumed that "democracy" can exist without women's participation or presence, either in personnel or in gender-specific programs.

What insights does Young's approach provide for compar-ing the influence of organized feminism in Canada and the United States over three decades? Despite the widespread suspicion of government in the United States, Young con-cludes that the National Organization of Women (NOW) has

become more involved in electoral politics and developed close ties with the Democratic Party. In Canada, where there has been more trust of government historically, the National Action Committee (NAC) has moved from a cautious, multiparty engagement with electoral politics to less involvement and a more radical, antiparty stance. In these two organizations, more of the Canadian than U.S. feminists concluded that "power is not electoral" and advocate activism outside the electoral system.

Young's thesis is that both ideology and women's collective experiences in each country are needed to explain the different trajectories. The U.S. movement—seen through the prism of NOW at least—has been dominated by liberal feminists who are more trusting of parties and electoral politics. The Canadian movement—seen through the prism of NAC—has been more influenced by radical and socialist feminism, which moved it away from partisan involvement. (NAC also went from being largely funded by the federal government to minimal state funding, which permits greater radicalism.)

Young demonstrates the importance of moving beyond ideology to explore women's collective experiences with the political opportunity structure. She concludes that the U.S. congressional system, with its weak political parties and more independent legislators, provides stronger incentives for feminist participation than does Canada's closed parliamentary system, whose exclusionary and strongly disciplined parties provide less opportunity for infiltration. The passage in 1982 of the Charter of Rights and Freedoms, which included a sex equality clause and explicitly sanctioned affirmative action, opened up the attractive alternative of legal remedies. The movement's success in gaining these clauses as well as government funding for organizations seeking to influence the courts, made quite clear the value of having women who favor feminist goals in party and elected positions—and on the bench. (At one key point, there were three women judges with women-centered perspectives on the Supreme Court.)

Young's systematic comparison of structural and ideological differences shows the strengths of a feminist political science approach. Her research demonstrates, for example, that although the Canadian New Democratic Party (NDP) responded more positively to feminist policy demands than the Democratic Party, a nongoverning party's inability to deliver in a parliamentary system was a negative experience for Canadian feminists. Another was that Progressive, Conservative, and later Liberal governments co-oped prominent women but did not adopt NAC's feminist policy agenda. By contrast, the electoral gender gap in the United States made Democrats more open to adopting NOW's policy as well as to co-opting women. Young notes, however, that in both countries parties were more open to participation by nonfeminist women, who represented "women" symbolically, than they were to feminist goals. This validates Young's thesis that transformation of the male dominance of electoral politics is possible, but it will not be achieved simply by women choosing to participate in parties and electoral politics.

There are few weaknesses in this excellent book. One is Young's tendency to focus on majority culture (white, anglophone) feminists, with little attention to other women-centered political movements. I would have valued more discussion of the views and experiences of U.S. black and Hispanic women, who are marginalized in society compared to white feminists. We know from gender gap analysis that these large minorities behave differently in important ways from majority whites, who can afford the luxury of "stand-alone feminism" focused exclusively on gender issues. More attention to Franco-Quebec feminists would have strengthened the Canadian analysis, especially since NAC failed almost from the beginning to incorporate those groups, who were drawn by the nationalist debate to focus on the Quebec state. Moreover, after 1995, NAC was led by a coalition of "women of color," lesbians, and women with disabilities; as NAC increasingly represented marginalized women, it was increasingly marginalized; losing especially support from elected and partisan women and women in the media.

Both cases suggest that the relative power or lack thereof of women's organizations shapes their views and experiences with parties and electoral politics. Feminist political science, therefore, needs to move beyond "women" as its central category of analysis to consider how women who are different because of their minority "race," language, or nationality experience opportunities to participate differently. This means recognizing that women are not uniformly powerless, as some feminist ideology suggests. These are minor quibbles, however, about an excellent book that should be read by every political scientist interested in United States, Canadian, and comparative politics, as well as by every feminist activist grappling with questions about politics.

International Relations

The Spy Novels of John le Carré: Balancing Ethics and Politics. By Myron J. Aronoff. New York: St. Martin's, 1999. 316p. $49.95 cloth, $21.95 paper.

John Nelson, *University of Iowa*

Literary forms of inquiry into politics range far beyond the journal article and the scholarly monograph. Myron Aronoff's monograph serves us well by respecting the novel of international intrigue as an insightful form for analyzing the politics of diplomacy, bureaucracy, covert action, and international regimes. Aronoff targets the latter-day dean of spy novelists, David Cornwell, who writes under the name of John le Carré.

These days, spy novels have trouble gaining serious attention even from literary critics. Their popular form often prompts disdain, even though the likes of Graham Greene and le Carré are conceded high marks for their literary structures and styles. The latest obstacle is the end of the Cold War. This has led some reviewers to the strange supposition that espionage and other modes of intrigue have ended—or at least stopped being useful as devices for addressing dynamics of international relations. On the way to lambasting le Carré's *Our Game*, no less a figure than John Updike states that "the end of the Cold War should have put an end to Cold War thrillers" (*New Yorker*, March 20, 1995, pp. 102–3). Aronoff knows better.

My own interest is in le Carré's attempt to make sense of the international politics taking shape in the wake of the Cold War. His novels of the 1990s provide one telling analysis after another of modes emerging for states, nations, militaries, economies, communications, ecologies, and migrations throughout the planet. Aronoff's concern is more with le

Carré's "ambiguous moralism." This is Aronoff's name for the problematics of political action in our times by individuals who know too much for some pure idealism to seem plausible, but who care too much for sitting on the sidelines to feel responsible.

Le Carré has enjoyed one of the more sustained and successful careers of political analysis in novel form, and Aronoff traces this search for "skeptical balance" throughout his writing. As Aronoff argues, le Carré's most famous creation, George Smiley, epitomizes this posture, which makes him one of the more complicated characters to recur in the imaginative annals of postwar action. The first seven chapters keep coming back to Smiley, testing his attempts to strike a balance between the moral and political imperatives that confront western democracies—and especially modern individuals—with one dilemma in action after another. The eighth chapter compares fictional intrigues to "the Real World of Espionage," and the ninth chapter casts brief glances toward the novels of the 1990s. The book concludes with exceptionally helpful notes, a fine index, and a roster of dramatis personae for the le Carré spy novels that can stand any fan in good stead.

This is a work of liberal humanism. Aronoff's terms of art are a clear indication: balance, ambiguity, means and ends, individuals and institutions, dilemmas, skepticism. Certainly, this fits le Carré as the prime heir to Graham Greene. In their skein of liberal tradition, the practical details matter. Accordingly, Aronoff devotes particularly effective chapters to le Carré's portraits of bureaucratic politics, domestic as well as foreign, and to his cumulative account of espionage as both a culture and a craft. This binocular focus brings out the political depth of le Carré's settings. These stay informed almost up to the minute, and they manage an intelligence about international relations that makes his novels a good education even for professional students of statecraft and soulcraft.

Aronoff concentrates mostly on the soulcraft. He is fascinated by characters such as Smiley. He wants to know how they balance idealism and realism, how they combine sentiment and skepticism, how they manage loyalty and betrayal. The issue for Aronoff becomes whether such a morally ambivalent and politically ambiguous figure as Smiley can be appreciated as a hero—or even a human. The final chapter poses these questions directly, and it answers them emphatically in the affirmative. Aronoff's book plays this familiar game of humanism in terms at once insightful and persuasive. As they say on the cover of popular potboilers, it is a good and enjoyable read.

In a way, however, the game stays a little too familiar. It is no surprise to anyone at this point that the spy can be a human and a hero. But is the novelist and literary theorist Samuel Delany right to suggest in his Nevèrÿon tales that the hero in Western civilization must always be a spy? Is le Carré himself right to imply in his novels of the 1990s that a spy is simply a politician by another name and set of means? Might those same novels supplement individual actors with structuralist and poststructuralist sensibilities about regimes of transnational relations? It can be fun and instructive to read the best of liberal humanists for their leanings also toward postmodern politics, and this seems especially appropriate for a novelist who pays such sophisticated attention to the kinds of political institutions we have been constructing for the twenty-first century. Perhaps there is need or at least opportunity for a sequel from Aronoff. That would be a pleasure to anticipate.

Open-Economy Politics: The Political Economy of the World Coffee Trade. By Robert H. Bates. Princeton, NJ: Princeton University Press, 1997. 221p. $59.00 cloth, $18.95 paper.

John S. Odell, *University of Southern California*

For three decades political scientists have attempted to show that markets reflect the political institutions and politics within which they function. Also, many scholars have traced states' foreign economic policies to their domestic politics. *Open-Economy Politics* pushes both these projects forward with an extended case study of the world coffee market. Beginning in the late nineteenth century, Robert Bates takes us chronologically through key shifts in policies of the chief coffee trading countries—Brazil, Colombia, and the United States—especially the formation, operation, and collapse of the International Coffee Organization from 1962 through 1982.

The central argument appears on page 7: "What is required for the study of international political economy is a domestic theory of politics. Indeed, I will demonstrate that the foreign economic policies of the great powers that created the International Coffee Organization [ICO] represent the product of domestic political struggles, . . . a political process that is structured by institutions."

The book's most important theoretical contribution arises from what Bates means by institutional effects. He does not mean that centralized governments will tend toward one policy, and federal states with divided powers will tend toward another (p. 163). Rather, he emphasizes the incentives different institutions create for producers and politicians living in open economies, as well as the resulting behavior of these individuals toward one other. For example, although Brazil had become the dominant world coffee exporter by the 1880s, it did not use its market power to maintain the price until 1906 (chap. 2). The lag is a puzzle for the economic theory of cartels and for political hegemony theory. Earlier efforts by producers in São Paulo state to get their government to intervene in the market failed because of Brazil's federal structure, according to Bates. São Paulo needed support from other states. Brazil began to act like the unitary actor assumed by systemic theories only when coffee politicians hit upon a side payment (currency depreciation) that attracted sufficient support in other states.

Colombia entered the world market after 1906 with a deliberate strategy of taking a free ride on the price floor Brazil was enforcing by itself (chap. 3). During the depression Colombia spurned Brazilian appeals to accept some of the burden. These choices also resulted from a fascinating political story. The Colombian government wanted to cooperate with Brazil, but Colombian coffee growers defeated their own government's efforts in three different policy domains. They could do so for two reasons, Bates claims. First, politicians formed a producers' association that overcame their own collective action problems. Second, in Colombia during this period there was serious competition between two political parties, and the coffee growers played one off against the other.

Building on what is already known about the ICO's formation, Bates (chap. 4) adds the insight that, after World War II, both Brazil and Colombia had more centralized political institutions than before. Politicians in both countries were better able to exploit their coffee industries for the sake of national development. But after efforts to form a cartel failed again in the late 1950s, exporters turned to the United States to help enforce a price floor. To earlier accounts of U.S. support, chapter 5 adds that Congress delayed the commit-

ment's implementation for three years, another lag that is explained by domestic politics.

Chapter 6 offers an ingenious argument, supported by primary research, to explain how the ICO maintained a stable equilibrium for twenty years. Exporters negotiated long-term contracts that gave lucrative discounts to large U.S. coffee roasters, such as General Foods. These in turn lobbied Congress to support the organization that held up world prices (which smaller competitors paid in full) at the expense of U.S. consumers.

The book uses the method of deploying theories to interpret a narrative. Bates considers five alternative theories but finds each lacking in some respect. In addition to cartel theory and hegemony theory, he reports anomalies for dependency theory, the "new trade theory" of imperfect competition, and Rogowski-Frieden arguments that emphasize international markets as cause and domestic politics as response.

The presentation could have been more effective in a few places. When interpreting Brazil's key 1906 intervention in the coffee market, Bates neglects to describe who did it and how (p. 37). The actor was the São Paulo state government, according to Stephen Krasner ("Manipulating International Commodity Markets: Brazilian Coffee Policy 1906 to 1962," *Public Policy* [Fall 1973]: 498–9). A chapter emphasizing federalism might have clarified why federal legislation and the package deal were necessary. Furthermore, since São Paulo itself would benefit from currency depreciation (the side payment), it is not clear why other states would have "paid" for it with support for the new coffee policy. The point about party competition in Colombia during the 1930s is not supported with specific evidence showing the two parties' relative strengths, which makes it more difficult to evaluate the claim that coffee growers were pivotal. After finding that one particular game-theoretic interpretation falls short, the book generalizes too quickly to all of them ("game theoretic approaches too thus prove unsatisfactory" (p. 160)).

These quibbles notwithstanding, this study makes significant original contributions. The extended case study turns up theoretically relevant insights that would have been missed otherwise. The reach of the argument across so many decades is impressive. This is the first book to read on the political economy of coffee. The analytical approach of looking for ways in which institutions shape markets and political behavior in open economies should be fruitful in many other cases as well. Finally, Bates is part of a group working to show that rational choice theories and narrative history can enrich each other. (See the debate over their 1998 book *Analytical Narratives* in *APSR* 94 [September 2000]: 685–702). The group's efforts to transcend unproductive tribal warfare between academic schools deserve enthusiastic applause.

The Spiral of Capitalism and Socialism: Toward Global Democracy. By Terry Boswell and Christopher Chase-Dunn. Boulder, CO: Lynne Rienner, 2000. 281p. $55.00 cloth, $23.50 paper.

William R. Thompson, *Indiana University*

In introductory international relations courses, we were once accustomed to contrast three alternative approaches: realism, liberalism, and Marxism. The collapse of the Soviet Union and the proclaimed triumph of liberal politicoeconomic ideas has led to a deemphasis on the third paradigm or, in some cases, its substitution by constructivism. But, contrary to Fukuyama, history has not quite ended. Neo-Marxist interpretations of international relations persist, and new and interesting ones continue to emerge. The latest entry, Boswell and Chase-Dunn's new book, is a case in point. As long-time and leading contributors to world systems theory, they employ their theoretical interpretation of modern history (the last 500 years) to explain what went wrong with socialism and how the socialist strategy might still be salvaged in a future world-system (with a hyphen).

The book is divided into six chapters. The first gives an overview of the authors' version of world-system dynamics, with an emphasis on the continuities of a capitalist world economy, an interstate system, and a core-periphery hierarchy. Within these structures, five long-term trends have proceeded: commodification, proletarianization, state formation, increases in economic enterprise size, and capital intensification. Rounding out the focus are several cyclical phenomena, including globalization, long waves of economic expansion and contraction, and hegemony. In brief, it is the interaction of these systemic trends and cycles within the world-system structures that produce the dynamics of change.

The second chapter surveys revolutions within a world-system context. The emphasis is placed on "world divides"— the eras of upheaval that occur in between movements from one type of accumulation regime to another. The outcomes of these struggles, it is claimed, have altered periodically the nature of capitalism. In particular, living standards and political rights for labor have been enhanced. The spiral of capitalism and socialism then results from transformational opportunities in periods of organizational breakdown. No improvement or progress is guaranteed, but the probability of progress has at least been increased during these windows of upheaval and opportunity.

The last four chapters focus primarily on the past and future of socialist strategies. The basic arguments are that these strategies were adopted by semiperipheral actors seeking upward mobility in the world economy. The strategies worked to a point but ultimately imploded in the face of stagnation and changing modes of economic production. One of several problems with the strategies was that socialist economies were adopted in some national economies. To succeed in a capitalist world economy, socialism must be adopted on a systemic basis. Similarly, contemporary resistance to globalization tends to be nationalistic and is therefore equally doomed to fail.

To develop socialism on a global basis, and to resist more successfully the costs of globalization, the authors propose a "global democracy" strategy. The spear carriers would be social movements operating in regional and global arenas, as opposed to national ones. Labor unions, women and children rights movements, and green environmental groups, all contending as transnational actors, could transform the nature of world governance by working toward improved standards of living and expanded rights, as well as the creation of global institutions with agendas that go beyond conserving traditional capitalism. The outcome would be greater national and global democratization.

There is much to approve in this analysis, just as there is much with which to disagree. If one is more concerned about analytical dynamics than outlining future social movement strategies, chapters 1 and 2 could easily have been expanded into several more chapters. This would have permitted space for a more detailed examination of the basic world-system model. It combines elements of now conventional world-system dynamics with greater explicit attention to transformational principles and agents. Such an examination could easily have been a book in itself, and it is hoped that the authors will return to the elaboration of these theoretical issues in the future.

Another point of disagreement worth highlighting is the source of inspiration for some of the arguments. Boswell and Chase-Dunn believe that progressive labor and other social movements were critical in transforming national capitalist systems and strategies. They may well be right, but their argument would have been enhanced if they had developed this interpretation further. By showing how, under what conditions, and to what effect social movements altered national political economies, they might have better demonstrated how these processes might or might not work similarly at the global level. At the same time, more attention to nation-level transformations might also suggest that a perceived struggle between capitalism and socialism has become an increasingly obsolete way of framing the problem. The authors would have done better simply to emphasize the democratization dimension already present in their argument.

But continuing disagreements over how to frame world-system dynamics (with or without the hyphen) is precisely the overall point. Disputes about ways to analyze global dynamics will persist. So, too, will disagreement about the relative superiority of political-economic and social strategies. The triumphalism of the Cold War ending was something of a mirage. True, liberal democracies defeated aristocratic, fascist, and communist autocracies over the span of the twentieth century. But there are still many major policy problems to resolve, and people will continue to disagree about how best to manage global problems. However one feels about the relative virtues of socialist strategies per se, Boswell and Chase-Dunn definitely contribute to the theoretical "globalization" of our perspectives about what sort of critical processes and dynamics we are attempting to survive in the short run. In the longer run, the goal of a better world in which to live can hardly be sneered at. Just how we will attain that better world will be a leading question of the twenty-first century.

Maneuvers; the International Politics of Militarizing Women's Lives. By Cynthia Enloe. Berkeley: University of California Press, 2000. 418p. $45.00 cloth, $17.95 paper.

Mary Fainsod Katzenstein, *Cornell University*

When I was an undergraduate in the 1960s, as the Vietnam conflict was escalating, I took Stanley Hoffmann's mesmerizing course, "Causes of War." I thought back to this class as I read Cynthia Enloe's book, which deserves all the superlatives it has accrued. The experience of reading now and remembering back left me wondering: Without Enloe to consult (her first book on militarism and gender came out in the early 1980s), what were we missing in Hoffmann's class? The answer, I think, is this: We could understand well enough the contending theories about why nations go to war; but in the absence of Enloe, we were less able to ask how militaries could manage such massive mobilizations that required the often calamitous sacrifice of precious lives even for wars whose purposes seemed remote or unconvincing.

Militarism and its gendering, Enloe argues, prepare the ground for mobilization. If men and women are to go to war in whatever combat or noncombat capacity or are to encourage or suffer the tolls of war on themselves and their loved ones, then their identities must be militarized. A good mother will be one who wants to send her son not to school or to work but to war; a good son or husband is one who proves his manliness not so much on the sports field or behind a plow as in uniform; a good "militarized" prostitute is one who will have sex with whomever it is in the military's

interest for her to do so. Thus, militarized gender is like nationalism. Benedict Anderson writes that an imagined national identity sows the seeds of battle: The idea of the national community has made "it possible over the past two centuries for so many millions of people not so much to kill, as willingly to die for such limited imaginings" (*Imagined Communities*, [1983] 1996, p. 7). Enloe's work speaks of an imagined and "idealized" gendering, one suited for war. The brave male soldier, the wife who serves in wartime factories when the nation needs her, the prostitute who gives comfort to soldiers away from their loved ones, the nurse who tends the sick and dying without attending to what they are dying for. When gender identities are militarized—when they are "controlled by, dependent on or derive [their] value from the military as an institution or militaristic criteria" (p. 291)—it is more likely, Enloe argues, that violent conflict can occur.

For any core course in international relations, Enloe should be mandatory reading. Those already among Enloe's wide readership will know some of this text's central arguments, but *Maneuvers* offers a trove of new insights. A thesis even more powerfully developed here than in Enloe's earlier writings is the title of the book—how policymakers maneuver to make strategic choices. By emphasizing the purposefulness of policy choices, Enloe shows how the very different experiences of women located in varied ethnic, national, class, and occupational contexts are tailored to the needs of militarism, a project that is not always consistently successful, as she observes. But *Maneuvers* has more than a functionalist lesson; by emphasizing policy choices and variability across time and national context, Enloe shows that militaries are not governed by primeval identities. Gender identities must be created, including even those revealed in wartime rape, which Enloe argues are too often mistakenly presumed to be caused by "raw primal misogyny" (p. 134).

The policy choices made to foster and routinize prostitution ("Prostitution seems routine. Rape can be shocking" [p. 108]), which effectively disguise what often should be recognized as institutionalized rape, are some of the most vivid descriptions in the book. Yet, even as Enloe writes about such emotionally laden terrain, she never mocks or derides, never oversimplifies or closes the reader off to the complex motives and human pathos associated with even some of the most dire acts. From her description of the much publicized 1995 rape of a young Okinawan schoolgirl for which three American soldiers were indicted, one comes to understand the perspectives not only of the outraged Okinawan protesters but also of the mother of one of the soldiers and the American admiral whose career prospects crumbled when he said to the media, in criticizing the "stupidity" of the three soldiers: "For the price they paid to rent the car, they could have had a girl" (p. 117).

As in her previous books, Enloe insists on the importance of not just studying women in the military but of understanding the militarization of women's lives everywhere—in and out of uniform, in the United States, the Phillipines, Bosnia, Afghanistan. Indeed, her description of the many diverse international expressions of the militarization of gender is one of the most important contributions of this book. Her knowledge and observational powers are formidable and impressive for their specificity and accuracy. Whether she is discussing the star wars satellite-shaped pastas in a can of Heinz tomato and noodle soup, which many mothers might hope will persuade their sons to like their lunchtime soup, or the question of whether male marines should be allowed to carry umbrellas, or the dollar-per-day renting of a Thai woman outside the base gates (known as a "teafuck"

[p. 231]), Enloe never lets us forget the "normalization" of militarism.

Enloe makes her readers see differently. Next time you visit Washington, stop by Walden Books in Dulles Airport, terminal D. With the kind of curiosity Enloe instills, you will not fail to notice that under the categories "history," "world history," and "American history" (unless the shelves have been rearranged), easily 80% of the books are about militaries. Enloe's *Maneuvers* is featured on bookshelves in Sarajevo, Tokyo, Delhi, and Sydney. You may not find it in terminal D, but be sure to read it.

Polities: Authority, Identities, and Change. By Yale H. Ferguson and Richard W. Mansbach. Columbia: University of South Carolina Press, 1996. 476p. $49.95 cloth, $24.95 paper.

Claudio Cioffi-Revilla, *University of Colorado*

This complex, ambitious, and large book seeks to question, reformulate, and enhance the scope (and methods) of international relations theories, particularly those formulated within a realist framework. The authors question that framework, the Westphalian model of putatively unitary nation-states, quantitative methods of empirical investigation, and the levels-of-analysis paradigm. The breadth of their critique is extensive and, consequently, highly ambitious. Rather than approach international relations as a system of unitary nation-states inspired by realist principles, which is arguably a partial and simplified portrayal of contemporary research, the authors favor a complex system of "overlapping, layered, and linked polities" that have both horizontal and vertical dimensions. The latter dimension is particularly important, as it concerns patterns of authority, identity (including ethnicity), and institutions that compete for the loyalty of individuals.

The fifteen chapters are grouped into three parts: a theoretical introduction, historical applications, and conclusions. The first two chapters critique what may be called "empiricist Westphalian realism" and propose a framework of nested polities. The next twelve chapters present six historical case studies on major polity systems of the past—Mesopotamia, Greece, China, Mesoamerica, Islam, and ancient Italy—with the first chapter in each case being dedicated to the "horizontal dimension of politics" (spatial interactions in terms of diplomacy, warfare, trade, and the like) and the second chapter making the case for the "vertical" dimension (the "overlapping, layered, and linked polities") within the horizontal space. This structure provides the book with a high degree of organization, which seems vital, given the ambitious objectives. The summary chapter returns to the main themes introduced in the first two chapters.

Polities is highly original because it presents a challenging framework with several praiseworthy features. First, it stretches the temporal scope of most contemporary theory and research, which is based mostly on the recent historical past and, consequently, has a myopic view of the present and the future. As the authors demonstrate, and a few other political scientists would agree (myself included), international politics is a phenomenon of ancient origin and long evolution; it is not a recent pattern of behavior. Second, the book demonstrates the existence of international relations and world politics in non-European international systems, such as East Asia and Mesoamerica. This is essential for establishing the cross-cultural universality of patterns. Third, it addresses the complex phenomenon of nested polities and conflicting loyalties in world politics, a very real phenomenon

with observable consequences in the post–Cold War international system. Fourth, although the authors did not intend this, the framework also offers some new scientific challenges and opportunities for quantifying the past and testing new intriguing hypotheses. For example, empires are nearly universal polities that can be systematically measured and investigated, as Rein Taagepera (not cited) and others have demonstrated.

The originality and ambition of this book make it important not only for IR scholars but also for political and social scientists in general. Its style is eminently readable, if not always totally accurate (for example, the characterization of empirical approaches is somewhat simplistic, the much criticized levels-of-analysis framework is in fact used in the so-called vertical dimension of politics, and aspects of ancient history and long-term change could be stated differently). Regardless, this is an important book.

There are no other recent books like this written by American political scientists, although several scientifically oriented scholars (e.g., M. I. Midlarsky, G. Modelski, R. Taagepera, W. R. Thompson, D. Wilkinson, and certainly this reviewer, among others; none of them cited by the authors) have published on the origins and long-term evolution of international relations. A recent similar work by a British political scientist is K. R. Dark, *The Waves of Time: Long-Term Change and International Relations* (1998), although the framework here is quite different (Dark is also a theoretical archaeologist). Archaeologists have produced some important works related to the problematic of this book, among them G. M. Feinman and J. Marcus, *Archaic States* (1998), J. A. Tainter's classic, *The Collapse of Complex Societies* (1988), and a host of monographic regional works on the case studies included in this book.

Ferguson and Mansbach are political scientists known for their interest in historical change as well as for their skepticism regarding contemporary scientific approaches to IR theory and research. Ferguson is also known for his work on ancient Greece. Both authors have collaborated before on substantive and epistemological issues in the field, so this book may be seen as a recent product in their on-going joint project.

Polities is written primarily for IR scholars, but the issues raised should also be of significant interest to scholars of comparative politics and case study methods. In fact, many of the topics addressed, as highlighted in the first and last chapters, affect all contemporary multiethnic polities. The Balkans, the former Soviet Union, Latin America, the EU, and even the United States are good examples of polities with a significant "vertical" dimension (and levels of analysis). The book can certainly be used as a "Devil's advocate" within a graduate survey in IR. Its use in undergraduate instruction is more problematic, but feasible. (I have used it in an honors Introduction to IR; the "commoners' section" cannot handle it.) The background assumed by this book is considerable, particularly if one is to derive maximum profit from both the proposed framework and the case studies. At a minimum, it assumes familiarity with contemporary IR theory and research; basic scientific epistemology; case study methods; and a considerable range of prehistoric and historic materials for the six case study areas.

While *Polities* has numerous praiseworthy features, as should be apparent from the above remarks, some of its limitations tend to hinder the achievement of its full potential. Certain of these are substantive, others are stylistic or technical. On the substantive side, there is far too much reliance on secondary and tertiary sources (including textbooks) and not enough on the specialized primary sources (archaeology and epigraphy), which are essential in a project

of this nature, however challenging the use of primary sources may be. For example, the primary materials in the works of K.-C. Chang, D. Keightley, L. Liu, D. Nivison, A. Underhill, and other scholars of early China demonstrate that the emergence of political complexity in China dates to the Xia (Hsia) state, starting in about 2100 B.C. at the latest, not the much later Zhou (Chou) periods chosen by the authors. This is a difference of 1,000 years of political evolution, which is significant even in terms of China's long history. Similarly, primary sources demonstrate that political complexity—the rise of state-level polities—in West Asia had twin origins in Mesopotamia (southern Iraq) and the Susiana (western Iran), not just the former. Moreover, chiefdom-level polities originated thousands of years earlier, and it was Uruk that "created the first large regional Mesopotamian polity" (G. Algaze, W. Hallo), not Agade under King Sargon (p. 67). Again, this is a difference of ca. 2,000 years, which cannot and must not simply be ignored when the focus of investigation concerns the origins and evolution of polities. These are not issues of marginal chronological detail; they have to do with the main claims of the book, as presented in the first chapters and summarized again in the set of questions in the concluding chapter (p. 381). There are also some puzzling lacunae, such as the exclusion of precontact South America, where the Andean system of polities was as significant as those that developed elsewhere in the world, from Chavín to the Inca empire. These and other substantive problems are probably caused by the inclusion of too many general and tertiary sources to the exclusion of more specialized primary sources.

From a methodological perspective, the case selection criteria are not clear, because at least one major "primary" polity system is omitted (Andes), almost all polity systems skip over the truly formative stages (e.g., the transition from chiefdom to state-level polities), and insufficient attention is dedicated to the key issue of case selection. For example, the distinctions, similarities, and overlaps among areas of Mesopotamia, the Levant, Hatti, and the Susiana in West Asia are no clearer than those among the Olmec, Maya, Zapotec, or Aztec areas in Mesoamerica. Case units such as China, Greece, Mesoamerica, and Italy are in many respects incomparable without rigorous conceptualization and empirical operationalization, at least not on the same polity scale, even allowing for the "vertical" phenomena of nesting and layering. Far more work with primary sources needs to be undertaken. Such work will eventually provide us with systematic, comparable, and empirically reliable descriptions of these early polity systems.

On the stylistic side, the authors should be commended for the use of maps (IR needs far more in the area of historical cartography), although some errors escaped their attention (e.g., Umma was not an empire in the 24th century B.C., and ca. 1700 B.C. the shore of the Persian Gulf came close to Ur, not where it is today [p. 69]; Palenque is not located in the Gulf of Mexico [p. 226]). Yet, for a work of this nature the lack of chronological timelines is somewhat troublesome, particularly for students who lack solid historical background across the case areas. Brief tables with "key dates" (pp. 67, 170–1, et seg.) lack the much richer and accurate information that only side-by-side cross-polity timelines can convey.

On the whole, *Polities* is a highly provocative and stimulating book, one that should inspire the fields of international relations and comparative politics to investigate more ambitious horizons through collaborative efforts, including interdisciplinary approaches and methods, to understand better the present and future of world politics.

Immigration and European Integration: Towards Fortress Europe? By Andrew Geddes. Manchester: Manchester University Press, 2000. 196p. $69.95 cloth, $24.95 paper.

Jeannette Money, *University of California, Davis*

Andrew Geddes provides a European analysis of European migration policy. He asks two questions: To what degree has the European Union (EU) garnered control over migration policies of member states? What is the policy outcome? In answering these questions, the author makes two contributions to the literature.

First, Geddes documents the evolution of EU control over migration policy, from the origins of European integration through ratification of the Treaty of Amsterdam. (I refer to EU in the text, although this entity has had different names at different times.) He correctly points out that migration policy involves both intra- and extra-European population movement. Free movement for Europeans was central from the original Treaty of Paris (1951), which founded the European Coal and Steel Community, and its "constitutionalization" and "institutionalization" gradually evolved. Although the original texts were ambiguous, citizenship in a member state became the criterion for free movement. Ultimately, European citizenship was created in the Treaty on European Union (1992), which granted all citizens of member states the right to reside (and work) in any member state. Therefore, third country nationals (TCNs) do not qualify.

As for the EU's extra-European migration policy, the Single European Act (1986) created a single market and, more important, the Schengen Agreement (beginning in 1985) broke down internal frontiers, both of which led member states to cooperate on immigration and asylum policy. Geddes describes the gradual incorporation of these decision-making arenas into the EU's institutional structures, but intergovernmental cooperation was accompanied by "a resistance to integration" (p. 67), so these arenas remain subject to national control through rules that require unanimity for policy change. Moreover, according to Geddes, the shift to EU policymaking reduced democratic and judicial oversight in member states, which could increase internal security measures with respect to extra-European migrants. His account summarizes the trajectory of EU migration policy and provides a useful and detailed history of the changing institutional locus of migration policy decisions.

Second, Geddes provides a careful description of policy outcomes, especially those associated with TCNs. He discusses the "co-existence of restrictive and expansive tendencies in immigration policies" (p. 172) and the securitization of immigration policy. This is a useful corrective to such authors as Yasemin Nahoglu Soysal (*Limits of Citizenship*, 1994) and David Jacobson (*Rights Across Borders,* 1996), who emphasize the advent of a "postnational" citizenship. Geddes is not alone in his observations (see Gallya Lahav, "International vs. National Constraints in Family Reunification Migration Policy," *Global Governance* 3 [September–December 1997]: 349–73, for an earlier analysis of the restrictive elements of European migration policy), he reminds us that restrictions on TCNs are still considerable.

Having established the unequal treatment of TCNs in chapters 2 through 5, Geddes devotes chapters 6 and 7 to an evaluation of immigrants' efforts to extract more equal treatment from member states, using the EU as an institutional level. He points to political opportunity structures as a determinant of organizational efforts. Again, his conclusions differ from postnational analyses, which emphasize principles of human rights. Geddes argues that, because free market principles underpin free movement in Europe, immigrant

lobbies build on those principles to legitimate their demands. This is a persuasive argument that complements rather than contradicts Soysal's and Jacobson's focus on human rights, international law, and the courts as mechanisms for expanding migrant rights. Despite careful attention to the institutional context of migration policy, the author fails to provide a clear theory—and therefore no predictions—about either the degree of European integration on the immigration policy dimension or the inclusiveness of future policies.

Ultimately, the book makes primarily an empirical rather than a theoretical contribution. The author, in chapter 1, argues that he is moving beyond the theoretical dichotomy of intergovernmentalism and supranationalism by adopting a "multi-level approach," which documents that "powers and authority are now shared" by local, national, and supranational institutions (p. 37). His description of the different trajectories of free movement versus immigration and asylum policies is useful empirically but disappointing theoretically. The variation across policy arenas suggests that we need a theory to explain why most of the powers and authority for free movement are vested at the European level, whereas most authority over immigration and asylum remains at the national level. Geddes argues that the process of integration has not been uniform, and sovereignty is variously dispersed at local, national, and supranational levels for different policies. Without a theoretical framework, however, we cannot understand why and cannot predict future policy courses.

Finally, there are contradictions in the presentation that are glossed over rather than explored. Immigrant lobbies maintain that current policy, which provides national social benefits to immigrants based on residence but requires citizenship for supranational benefits, is "illogical." But few political scientists would argue that logic is the driving force behind most political decisions. Also, Geddes argues that EU institutions reflect a "democratic deficit" and require greater citizen participation; yet, the more generous efforts to integrate migrants are attributed in part to the EU's insulation from local political pressures.

These unexplored contradictions point to an underlying tension in the text. The author correctly chides (some of) the literature for its normative bias and argues that an empirical analysis is necessary. Moreover, he states that "policy needs to be based on a valid theory of cause and effect" (p. 24). Yet, because Geddes provides no theoretical framework, he cannot do more than caution against optimistic expectations for immigrant policy in Europe: "It is not possible to prejudge the outcome" (p. 169).

One might chide the author for some of his interpretations, but the basic empirical analysis is solid. If there is little by way of hypotheses and hypothesis testing, the book does not deceive. It lays out empirical questions and answers them with a careful review of the multilayered institutions governing free movement, immigration, and asylum policy in Europe. It will be widely cited by those interested in EU migration.

America and Political Islam: Clash of Cultures or Clash of Interests. By Fawaz A. Gerges. New York: Cambridge University Press, 1999. 282p. $59.95 cloth, $18.95 paper.

K. A. Beyoghlow, *Marine Corps Command and Staff College*

The central theme of this book is that U.S. strategy in the Middle East is fundamentally flawed but not irreparable. This may be the result of the inherent mismatch between strategy and policy and, more significantly, between America's principles and interests. In particular, the author argues that the U.S. approach toward Islam is "beset with ambiguities and tensions" (p. 3). Furthermore, he stresses that there is a

somewhat dangerous growing gap between the American people and their representatives in Congress, on the one hand, and presidential administrations, on the other, when it comes to dealing with "islamists" (those who espouse greater religious activism in politics). The former lean toward a confrontational attitude that is fed by cultural differences, stereotyping, and negative images of Muslims, whereas the latter strive to accommodate or tolerate a majority of moderate or pro-West Islamic forces and states.

The crux of the problem is that American presidents have gone out of their way to paint the majority of Muslims in a positive light, arguing that Islam is not the new "ism" in the post–Cold War world replacing communism, and therefore the United States should not have policies focused on religion, but they have not gone far enough. According to Gerges, they could have pressured America's allies in the Muslim world into more substantial political reforms and could have held the latter more accountable on domestic political reform. The United States opted not to do so for fear of upsetting the strategic partnership with these allied regimes. The result has been a policy of equivocations and inconsistencies. The other side of the coin is that American policymakers have been reluctant to apply the basic tenets of democratization to the Middle East, where only 25% of Muslims live, because the stakes are much higher there than in other regions (e.g., Asia-Pacific), where the majority of Muslims reside. Israel and oil tend to skew American policy toward seeking influence and power in the Middle East and North Africa instead of the focus on democracy pursued in the Pacific basin.

As an alternative, Gerges recommends that U.S. policymakers bet on reform-minded "islamists" worldwide, but particularly in the Middle East, in order to help bring about healthy, peaceful, and orderly change. After all, political reformists in this part of the world seek a better standard of living for their societies, an end to corruption and arbitrary rule, and pluralism—objectives inherent in America's principles and moral makeup. This book implies that not much will change, however, until the United States comprehends fully that the current political struggle in the Muslim world is really a struggle between civil society and the status quo, and between military authoritarianism or totalitarianism and democratic idealism.

According to Gerges, successive American administrations have gotten only part of the picture right. To its credit, the Clinton administration realizes that it is not on a collision course with Islam, but only with a handful of Muslims bent on using Islam as a vehicle for violence and nonconventional political participation. Yet, America needs to go a lot farther to ensure that its principles of compassion and liberal idealism are coordinated with its vital economic and security interests. This will not be easy for two reasons: (1) American presidential administrations will continue to face strong domestic opposition to any abrupt change toward reformist Islam, and (2) as Gerges correctly implies, not all islamists share America's vision of human rights, democracy, and economic globalization. U.S. policymakers, however, should not judge the current theocratic elite in Iran or Muslims elsewhere dichotomously, since categorizations such as radicalism and conservatism, status quo and revolution, and moderate and militant are at best misleading. A better measure of political attitudes is the saliency of sociopolitical and socioeconomic issues. Some issues unite Muslim factions, and others divide them. There is truly no such thing as a monolithic Islam.

America and Political Islam is rich in data, references, and policy recommendations and should be of great interest to

the serious student of Islam, regional politics, and American interests in the Middle East. The book is divided into ten chapters, including an outline that links culture and history to the making of U.S. foreign policy in the region; an effort to highlight the ongoing tension between American confrontationists and accommodationists toward Islam; an analysis of American images and perceptions that mistakenly connect terrorism with spiritual and political Islam; a discussion of the evolution of U.S. policies toward the Muslim world from the perspective of three American presidents and of the rationale behind those policies; and an analysis of the Clinton administration's efforts to play down the clash of civilization between Islam and the West. The rest of the chapters focus on U.S. response to Islamic resurgence and activism in Iran, Algeria, Egypt, and Turkey. In the conclusion, there is a serious attempt to recapture many of the policy recommendations strewn throughout the book on how to improve current U.S. policy toward the Muslim world.

I have two concerns with this book, one procedural and the other substantive. On the procedural level, although the chapters are well organized, they often lack a conceptual framework that ties them together in some systematic fashion. They come across as a mixture of academic research and investigative journalism. There is also an inherent problem with sourcing, which at times conveys a superficial treatment of the subject. For example, a sizeable portion of the information is attributed to interviews with unidentified individuals. In particular, a recurring interview is attributed to an unnamed U.S. Department of State official and is taken at face value as authoritatively credible, factual, and sound. Such sourcing could be potentially misleading and unconvincing. Likewise, the chapter on Iran is dependent narrowly on selected works of former policymakers, or current journalists. In short, some sections may lack rigorous analytical treatment and the originality of thought found elsewhere in the book.

On a substantive level, Gerges does not always capture the increasing complexity of making U.S. foreign policy in today's regional and global milieus: Foreign and domestic issues are becoming increasingly intertwined in the United States. U.S. foreign policy on any issue is part and parcel of a complex web of personal and institutional interactions that involve daily interagency debates and turf battles, aggressive congressional involvement in foreign policy decisions, an ambitious National Security Council staff, and a more assertive Department of Defense stimulated by the outbreak of multiple national, ethnic, and religious conflicts that require American military intervention. The Department of State is being preempted by these newcomers and by global and regional events, such that its traditional conflict resolution and preventive diplomacy often are forced to take a back seat to greater assertiveness and leadership by other agencies. For these reasons, American policy toward Islam will continue to muddle through rather than be expressed in a bold, explicit manner, as the author advocates.

Despite these concerns, *America and Political Islam* is a breath of fresh air that forces serious scholars and policymakers to rethink their positions on a very important topic with far-reaching political implications.

Exploring European Social Policy. By Robert R. Geyer. Cambridge, UK: Polity Press, 2000. 272p. $50.00 cloth, $14.99 paper.

William Walters, *Carleton University*

Social scientific interest in "social Europe" pales in comparison with the attention that has been directed toward the

economic and political dimensions of the European Union (EU). This is perhaps hardly surprising; for much of its relatively short history, the system that is today the EU has been almost exclusively economic in its focus. Only since the 1980s has the project of European integration acquired a significant social dimension. Given this imbalance, Robert Geyer provides a welcome and timely addition to the literature.

The major strength of this book resides in the significant empirical effort that underpins it. Early chapters deal with the historical emergence of a social policy for the European communities. The rest of the book "explores"—the author's favored term for his approach—this policy on an area by area basis, covering such topics as employment, gender, regional, and antiracism policy. Those new to the subject will be indebted to Geyer for a very accessible, systematic, and generally comprehensive overview. European social policy presents a bewildering array of social action programs, social protocols, social chapters, social charters, social dialogue, employment pacts, and so on. Geyer does an excellent job of untangling these and placing them in a coherent historical narrative. (Because the book is an introductory text, however, it would be helpful to have a glossary of terms.)

A major weakness of the book stems from the author's reluctance to engage with theoretical debates about social policy. Instead, Geyer presents his task as one of "mapping" or "exploring" EU social policy. His ambition is modest: "to provide academics and policy practitioners with an accessible foundation which they [can] use to explore their own particular questions" (p. 207). There is an attempt to relate what has become the standard classification of theories of European integration to the social field, but it is not very illuminating. Geyer also promises but fails to deliver a method informed by historical institutionalism (p. 6). Consequently, the book lacks a sound foundation to account for the developments it describes. Too often, the success or failure of a particular policy initiative is framed in terms of propositions about the general climate of the "integration" project at that time, as though integration were a singular process.

In no way does the ambition of exploring rather than theorizing about EU social policy invalidate this project. It does create problems, however, such as how one should define the scope and content of social policy. Geyer's solution is to accept the EU's "practical definition" (p. 5). Hence his empirical focus is basically the activities of the European Commission's directorate-general for employment, industrial relations, and social affairs. Yet, the social is not something fixed but an historically and politically structured field. Today, European welfare is heavily influenced through such mechanisms as the social regulation of consumer affairs and environmental issues, as much as through the more traditional social policy instruments (e.g., see Giandomenico Majone, "The European Community between Social Policy and Social Regulation," *Journal of Common Market Studies* 31 [June 1993]: 153–70). To ignore something like consumer policy, as Geyer does, is to miss something important about how the social field is being reconfigured and the new ways in which individuals and groups become active as consumer-subjects in their own government.

The book also would have benefited from some consideration of the relationship between the European social field and national policy dynamics. Geyer tempts fate somewhat by claiming to have "reviewed all major English language texts on EU social policy" (p. 7). This perhaps demonstrates the dangers of confining research too narrowly to a particular subfield. Geyer seems to have overlooked one of the foremost economic historians and assured voices on the subject

of European integration, Alan Milward. To grasp more fully the relative neglect of a social policy for most of the EU's history, one surely must consider Milward's *The European Rescue of the Nation-State* (1992). If European social policy has lagged behind developments in economic policy, it is perhaps in large part because the Common Market helped provide national economies with a framework for growth and prosperity. In so doing, it made postwar national social policies all the more financially viable and politically entrenched. In this way, perhaps the relative success of the Common Market has contributed to the failure—at least in its early years—of European social policy.

One final point: It would be more accurate, if slightly less elegant, to title the book "Exploring European Union Social Policy." Its concerns are almost exclusively the social policy programs and institutions of the EU. It is quite important that one not conflate Europe with the European Union. If the present EU has come to speak in the name of Europe, this should not be taken for granted or naturalized by using the terms interchangeably. Rather, it is a phenomenon that needs to be interrogated critically. The EU may be the most successful and hegemonic construction of Europe in recent times, but it is not the only one. Alongside the exploration of EU social policy that Geyer proposes—or any other policy field for that matter—we need to explore the other ways that international social policy has been attempted (e.g., see Carl Strikwerda, "Reinterpreting the History of European Integration: Business, Labor, and Social Citizenship in Twentieth-Century Europe;" in Jytte Klausen and Louise Tilly, eds, *European Integration in Social and Historical Perspective*, 1997). This might often be a tale of failure, but it nevertheless should expand our capacity to think about different possibilities for governing in the future.

The Challenge of Global Capitalism: The World Economy in the 21st Century. By Robert Gilpin, with the assistance of Jean Millis Gilpin. Princeton, NJ: Princeton University Press, 2000. 373p. $29.95.

Sylvia Ostry, *University of Toronto*

The word *globalization* first appeared in the second half of the 1980s and now has become the most ubiquitous in the language of international relations. It has spawned a new vocabulary: globaloney (Why all the hype when the global economy was more integrated in the age of Queen Victoria?): globaphobia (the new, mainly mistaken, backlash); globeratti (the members of the international nongovernmental organizations [INGOs] who travel around the world from conference to conference, except when they are on the Internet mobilizing for the next conference), and so on. For Robert Gilpin, among the world's most eminent scholars of international relations, globalization is insightfully defined as the deepening and widening integration of the world economy by trade, financial flows, investment, and technology. This, he notes, is the "second great age of capitalism." The basic theme of the book is whether, like the first great age, it will end with a bang (or even a whimper) or survive and thrive.

Gilpin provides a clearly written and comprehensive tour d'horizon of the drivers of integration—trade, finance, transnational corporations—and highlights for each domain the major policy issues. He spells out the main features of the growing backlash and is most acute in his critique of the simplistic views that globalization is either nirvana or Armageddon. But the most important thesis of the book—and its most important contribution to the discipline of international

political economy—is that the future of globalization ultimately rests on political foundations, not on the merits or demerits of the now dominant neoliberal paradigm. Like Schumpeter (and unlike Marx), Gilpin stresses that policy is the product of politics, not economics; unlike the international regime theorists who extend to the political domain the basic optimizing assumptions of economic theory, he stresses that the real world of political economy is too messy and prone to accident and error to support this reductionist model of policymaking.

At the core of the political economy of postwar global capitalism, Gilpin argues, was the Cold War. It is certainly true that the Cold War was vital to the launch of the Marshall Plan and the construction of the architecture of international economic cooperation. Churchill termed the Marshall Plan "the most unsordid act in history." That it was. But it was also, in Gilpin's classic realist approach, very much a response to fear of Stalin's communist missionary zeal. And there is little doubt that it was an immensely successful diplomatic maneuver to outwit the Soviet Union and score a triumphant preemptive move, the first salvo of the Cold War. It was also important that Stalin refused to participate and that both the Marshall Plan and the creation of the Bretton Woods institutions and the GATT were strongly supported by American business and labor, not for fear of communism, but because American industry was far more efficient than any potential competitors in Europe (let alone postwar Japan). Furthermore, although the Cold War constrained the spillover from "low" to "high" policy (e.g., from trade to security issues), the first major crack in the postwar architecture—the end of the Bretton Woods system of exchange rates—was catalyzed by the Nixon administration in the early 1970s, when the Cold War was still alive and well. Also, the shift to a multitrack trade policy and the rise of system friction with respect to Japan are related more to the increasingly complex evolution of the American trade policy agenda than to the threat of the evil empire.

As is clear from Gilpin's account, however, by far the most important effect of the end of the Cold War in the United States has been the decline in congressional deference to the executive branch in international economic policy. Gilpin cites Clinton's failure to secure fast track in 1997 and the struggle to win approval of IMF funding in 1998 as examples. Again, is it the end of the Cold War or the absence of a coherent view of America's role in a global economy that accounts for the increased domestication of foreign policy? What role is played by the media and, more broadly, enhanced communication in the erosion of the "permissive consensus" of the earlier postwar decades, which provided far greater scope for policy action in areas that did not resonate directly with the broader public? Obviously, no precise answer is possible, but it would have been useful to explore some of these factors. The American genius has always been creative ad hocery, and in a sense the United States became an "accidental hegemon" after being forced to enter World War II by the Japanese bombing of Pearl Harbor.

The uncertainty about America's willingness or ability to play a leadership role in sustaining and reinforcing the political foundations of the global system lies at the heart of Gilpin's concern with the future of this second age of capitalism. The main threats to the system, he argues, arise from regionalism, financial instability, and increased protectionism.

In reviewing the ongoing debate among economists as to whether regional trade agreements (RTAs) are building blocks for or stumbling blocks to preserving and enhancing the rules-based multilateral system, Gilpin comes down

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strongly—although for more than purely economic reasons—on the side of multilateralism. Many economists (myself included) would agree. But this analysis should have been broadened to include the proliferation of subregional agreements among southern countries (as tools to increase bargaining clout in the World Trade Organization). Indeed, the North-South divide, created in part by the Uruguay Round (and vividly manifested in Seattle by the walkout of virtually all non-OECD countries) is not mentioned. The threat may be a proliferation of RTAs without the United States, especially if the labor and environmental standards issues remain high priorities for the next administration. If a new round of negotiations cannot be launched within a reasonable time after the U.S. election, then the future of the WTO does not look promising.

For whatever set of reasons, we cannot dismiss the threat of fragmentation of the global system or the threat of another financial crisis—especially in light of the fact that the Clinton administration's ringing declaration to create a "new international financial architecture" has yielded little but modestly improved plumbing. As Gilpin points out, there is disagreement among economists and among governments about what should be done, and the United States is either unable or unwilling to lead the charge.

Gilpin wisely does not try to predict the outcome of these threats and challenges. He is certainly right in emphasizing that the political foundations of the global economy will determine its future and that the United States must play a leading role. But perhaps there are possibilities in other configurations of global leadership? Broader engagement was needed to launch the Uruguay Round. The creation of the WTO was due to a Canadian proposal latterly supported by the European Union to constrain U.S. aggressive unilateralism. The EU rescued the financial services negotiations after the outcome was rejected by the United States. These are only examples. It would worth exploring, especially by someone like Gilpin, whether a new pluralist system of global governance is possible. From the viewpoint of Realpolitik, perhaps the alternative would be bad enough for world leaders to give it a try.

Seeking New World Vistas: The Militarization of Space. By Roger Handberg. Westport, CT: Praeger, 2000. 304p. $62.50.

Larry Martinez, *California State University, Long Beach*

Space is a seductive place, where technology-induced vistas often blur the policy vision of earthbound military planners, scientific explorers, or commercial entrepreneurs. This is the message that beams down from Roger Handberg's book on space militarization. He leads the reader through the twists and turns of technology, law, and policy, through the bureaucratic labyrinth of the U.S. military and space industrial complexes. In the end, one is faced with the same imponderables that confronted President Clinton in deciding whether to deploy the National Missile Defense (NMD) system. Like an astute player on fourth down, he punted that space football to his successor, and the Handberg volume gives you the Monday morning quarterback advantage.

Handberg provides a brief history of space policy, segmented by technology digressions that help capture the flavor of the current policy environment. These background factors heavily influence the bureaucratic politics dominating space militarization debates. Military space policy, on the macro level, involves a debate about the intrinsic contradiction in

attempting to mold dual-use (military-civilian) technologies to fit military objectives.

Perhaps the most pervasive example is the wildly successful global positioning satellite (GPS) system, which originated with experiments in the 1960s. The U.S. Department of Defense (DOD) deployed some of its 24-satellite flotilla in 1990–91 in time for combat testing during the Gulf War. Even only partially deployed, GPS made a direct technology hit during Desert Storm. But the ancillary damage to military exclusivity was widespread; anecdotal stories abound of parents rushing to the local electronics stores to buy off-the-shelf GPS receivers so their soldiering children could guide their hummvees back to home base during blinding dust storms. The dual-use applicability—and profitability—of GPS has wrenched its control out of the hands of DOD designers and operators. In effect, due to its civilian and commercial appeal, GPS is becoming another Internet.

Just as the Internet represented a paradigm shift in networking architectures that continues to shape information-intensive societies and economies alike, Handberg raises the question of whether the outer space region performs an analogous task for global military configurations of power in general and for U.S. strategic doctrine in particular. The Internet obliterates the informational concept of distance, and Handberg wonders whether mastery of space will erase the concept of territory, enabling the U.S. military establishment to confront threats in a world that is "becoming a much more politically fragmented place with security threats becoming both more diffuse and very specific in certain regions" (p. 5). Space, in this regard, constitutes the ultimate high ground for the military establishment skillful enough to master it.

The basic seduction of space is the possibility of developing and using the perfect weapon, capable of "antiseptic" destruction of subnational or terrorist enemy threats without the messy political costs of noncombatant tragedies broadcast on CNN. Perhaps even more seductive is the prospect of constructing a global defensive shield against missile or aircraft attacks by rogue nations. Handberg astutely observes that the history of military space is littered with failed weapons, strategies, and paradigms. So what drives the current space military debate? He clearly locates decision making deep within the DOD corridors, as space architects battle it out with rogue commands that threaten to usurp long-range plans.

The latter chapters delve deeply into bureaucratic politics, detailing agency attempts to herd a workable ballistic missile defense-proposal into their deployment corral. The problem is, very few are willing to bet the farm on an untestable and perhaps unreliable defensive technology. From the earliest Nike-Zeus systems through the Strategic Defense Initiative (SDI) to the current crop of NMD proposals, the technology of hitting a bullet with a bullet in the fog of war is a dicey undertaking at best, and a disaster even with a 90%+ success rate. Nevertheless, the dream persists that it can be done, which fuels the bureaucratic battles among space architects, space commands, and stove piped systems.

The Gulf War was the first "space war." Widespread integration of space-based systems greatly enhanced the ability of Desert Storm forces to carry out missions and achieve objectives at minimum cost of lives and material. Whatever the final assessment of the Patriot's effectiveness as an antimissile weapon, the Desert Storm experience compelled a fundamental rethinking of U.S. space strategy, which Handberg identifies as taking place on two levels. The first is the theory-rich development of military doctrine and policy, and the second entails the applications-oriented process of

deciding which missions are most appropriate for space-based systems.

According to Handberg, on the first level there are four chief mission components for space-based systems: space force support, space force enhancement, space force applications, and space control. These missions are juxtaposed against four doctrines of action on the second level: space sanctuary (preserving space for free overflight and surveillance), survivability (a force enhancement tool), control (actual U.S. control of the outer space region), and high ground (space as the dominant theater of military operations). Handberg locates current U.S. military space policy within the sanctuary and survivability schools of thought, corresponding to prevalent "political and military needs." Whether the four missions move out to the control and high ground applications will depend largely on the technological and economic feasibility of future systems to make orbital access cheaper (control) and on the prevailing world security climate for the United States (high ground). Another and perhaps more crucial factor is whether these missions match programmatic and budgetary needs of the agencies that advocate them.

Handberg warns about the stealthiness of the space militarization issue and its proclivity to slip beneath the political radar screen. The debate is directed by bureaucratic infighters versed in the technologese that quickly alienates the uninitiated. Although the book attempts to impart a detail-rich view of the space militarization policy process, what emerges is a comprehensive but somewhat bewildering view of a policy morass, which just might be an apt description of current realities. Handberg advocates an open public debate about what the "new world vistas" mean for the ability of the United States and other countries to confront the diffused yet specific threats in the coming decades with military space infrastructures. Whether the vista matches the vision is anyone's guess.

International Relations and the Challenge of Postmodernism: Defending the Discipline. By D. S. L. Jarvis, Columbia: University of South Carolina Press, 2000. 288p. $34.95.

Jeffrey W. Taliaferro, *Tufts University*

Over the past twenty years, the so-called third debate, or the constructivist turn in international relations theory, has elicited a great deal of attention. Various critical theories and epistemologies—sociological approaches, postmodernism, constructivism, neo-Marxism, feminist approaches, and cultural theories—seem to dominate the leading international relations journals. Postmodernism (also called critical theory), perhaps the most radical wave of the third debate, uses literary theory to challenge the notion of an "objective" reality in world politics, reject the notion of legitimate social science, and seek to overturn the so-called dominant discourses in the field in favor of a new politics that will give voice to previously marginalized groups.

D. S. L. Jarvis's book is a wake-up call to international relations scholars who have become increasingly preoccupied with meta-theory and epistemology to the detriment of explaining "real world" phenomena, such as the causes of war and the conditions for peace. Jarvis offers a lucid and highly critical appraisal of the rise and fall of postmodernism in the study of world politics. For Richard K. Ashley and Robert Walker, the postmodernist challenge signals "a crisis of confidence, a loss of faith, a degeneration of reigning paradigms, an organic crisis in which, as Gramscians would say, 'the old is dying and the new cannot yet be born' " (Richard

K. Ashley and R. B. J. Walker, "Speaking the Language of Exile: Dissident Thought in International Studies," *International Studies Quarterly* 34 [September 1990]: 259–68). Jarvis persuasively argues that news of the discipline's demise and the ascendancy of postmodernism are greatly exaggerated.

Jarvis begins by tracing the history of international relations as a field of inquiry distinct from philosophy, international law, and history. "Rather than strong foundations and the building of a robust stock of theoretical knowledge, international theory looks to be cracking at the edges, its foundations crumbling amid the onslaught of perspectivism and epistemological debate" (p. 43). He then traces the evolution of postmodern theories and their importation to international relations in the late 1970s and early 1980s. Next, through a close examination of the writings of Ashley and Christine Sylvester, Jarvis traces the effect of "subversive and deconstructive" postmodernism on the field. In doing so, he is unsparing in his criticism of the extreme relativism (and in many cases nihilism), excessive jargon, polemical argumentation, repudiation of basic social science canons, lack of empirical evidence, and tortured prose that have come to characterize the so-called postmodern school. Finally, he concludes with an appeal to the continued utility of positivist research programs.

It is important to note the scope and limitations of the analysis. Jarvis does not launch a broad assault on the rise of constructivism and critical theories in international relations in general. For example, he does not take issue with the so-called conventional constructivism of John Gerard Ruggie, Alexander Wendt, Ted Hopf, and David Dessler. These scholars argue that discursive practice can fundamentally change states' foreign policies but seek a middle ground between the mainstream research traditions in international relations (realism, liberalism, and Marxism) and critical theory. This, however, is the portion of constructivism that has had the more lasting influence on the discipline. As Hopf notes, "to reach an intellectually satisfying point of closure, constructivism adopts positivist conventions" and, by doing so, can challenge realist, liberal, and Marxist theories of international politics (Ted Hopf, "The Promise of Constructivism in International Security," *International Security* 23 [Spring 1998]: 171–200).

Jarvis focuses on what he terms "subversive-deconstructive postmodernism," a body of scholarship that "displays a thematic concern with negation and resistance to modernist practices and discourses, primarily via a deconstructive-textual analysis of logocentric practices, modernist knowledge systems and language" (p. 66). Drawing upon the philosophy of Friedrich Nietzsche, Michel Foucault, and Jacques Derrida, as well as the deconstructive literary theory of Frederic Jameson, postmodernists repudiate and seek to undermine the entire intellectual tradition of the Enlightenment. Instead, all knowledge is located in the fact of textual analysis and situated in the subjectivity of each individual.

Jarvis presents a detailed critique of Ashley, who in the 1980s both brought constructivist accounts of the state, political power, and the practice of realpolitik into international relations and raised questions about construction of knowledge, meaning, and truth. "Never before have international theorists been so assaulted by excursions into meta-theory, especially when the depth of this excursion questions not only the ontological but also the epistemological and 'axiological foundations of their scientific endeavors'" (p. 90). Jarvis divides Ashley's work into two parts. In the "heroic" phase (the late 1970s to early 1980s), Ashley sought to highlight many of the epistemological and ontological premises upon which neorealist theory rests. In his seminal

essay, "The Poverty of Neorealism," Ashley criticized the theory for its undeveloped treatment of the state and showed how Hans Morgenthau's classical realism and Kenneth Waltz's neorealist balance-of-power theory rested upon normative assumptions (Richard K. Ashley, "The Poverty of Neorealism," *International Organization* 38 [Spring 1984]: 225–86). Jarvis notes that this stage of Ashley's research had a profound and positive effect on both the emergence of constructivism and the subsequent refinement of realism.

Jarvis is sharply critical of the second, or poststructuralist, phase of Ashley's work, which draws upon postmodernism to overthrow the dominant epistemology in international relations. By doing so, Ashley eliminates the "real world" problems of war, violence, poverty, and bigotry. Jarvis rightly asks: "Is Ashley really suggesting that some of the greatest threats facing humankind or some of the greatest moments of history rest on such innocuous and largely unknown nonrealities like positivism and realism" (p. 128)?

The main strength of this book lies in the author's efforts to assess the effect of postmodernism on the study of world politics against the standards set by Ashley, its foremost proponent. Paradoxically, this also is the book's main weakness. Postmodernism has not transformed the study of international relations, let alone the practice of statecraft, for two simple reasons. First, most international relations scholars are unwilling to wade through the postmodernists' abstruse prose to uncover the substance of their arguments. Instead of engaging Ashley, Walker, and others, most scholars (even their fellow constructivists) are quite content to ignore them. Second, by waging war on positivism, rationality, and realism, postmodernist scholars have marginalized themselves. As Jarvis himself acknowledges, "these are imagined and fictitious enemies, theoretical fabrications that represent arcane self-serving debates superfluous to the lives of most people and, arguably, to most issues of importance in international relations" (p. 128).

Immigration and the Nation-State: The United States, Germany, and Great Britain. By Christian Joppke. Oxford: Oxford University Press, 1999. 356p. $72.00 cloth, $22.95 paper.

Gary P. Freeman, *University of Texas at Austin*

This book will enhance Christian Joppke's growing reputation as one of the most thoughtful commentators on the politics of international migration and citizenship. *Immigration and the Nation-State* is an impressive cross-national comparison that builds on elite interviews and reanalysis of primary materials, but its chief value is in its bold synthesis and critique of a rapidly growing and highly disjointed secondary literature. Although it assesses a variety of theoretical concepts, the book is primarily a historically rooted, richly empirical work of analysis and interpretation. Joppke deals expertly with three liberal states with different nationhood traditions and immigration histories. The United Kingdom is distinctive in that it was at once a nation-state and an empire. The United States is the only case of the three in which governments deliberately sought to foster immigration for settlement. Germany was a divided nation whose commitment to reunification, embedded in the Basic Law, posed particularly troublesome issues for immigration and citizenship policy.

The key organizing concepts of the book are sovereignty and citizenship. The first argument is that liberal states still exhibit sovereign power over immigration policy, contrary to proponents of the globalization thesis (e.g., Saskia Sassen,

Losing Control? Sovereignty in an Age of Globalization, 1996), but they are self-limited by interest-group pluralism, autonomous legal systems, and elite moral obligations toward particular immigrant groups (p. vii). "The major dilemma of immigration control is to reconcile the popular constraints of states with their parallel, inherently unpopular, mandate to protect the human rights of migrants" (p. 5). Joppke's second principal argument is that national citizenship is critical for the incorporation of migrants (contrary to the postnational membership model advanced most prominently by Yasemin Soysal in *The Limits to Citizenship,* 1994), but nationally distinct citizenship regimes result in diverse multicultural arrangements. Citizenship practice in the three states is inclusive internally but exclusive externally. "Immigration has opened up a post-Marshallian view of citizenship, which stresses its externally exclusive dimension" (p. 6).

Joppke is unusual among immigration scholars because he does not accept a number of commonly held views and often tilts against the conventional wisdom. He disagrees, for example, that an overweening pattern of immigration restrictionism is sweeping across "Fortress Europe," even less across the United States. He discerns instead a marked trend toward inclusive and expansive policies in two of his cases. Only Britain fails to conform to this pattern. This does not mean, however, that European democracies are destined to open their doors to immigration in the manner of the United States. He disagrees with those who claim that the distinction between endogenous nations and nations of immigrants is moot (p. 9). European states, he observes, are not and have not been nations of immigrants. Among liberal democracies it is the settler societies—the United States, Australia, and New Zealand—whose immigration experience stands out as unusual.

Joppke writes especially well about Germany, whose immigration and citizenship policies are widely misreported by the press and scholars alike. He notes, for example, what is obvious but rarely appreciated: The *ius sanguinis* citizenship tradition is not a bizarre German idiosyncrasy but is, in fact, standard practice in continental Europe. The puzzle is not why Germany persists in claiming that it is not an immigration country, but why it should think of itself in that way (p. 62). He points out that the "as of right naturalization" procedures adopted in the early 1990s make German law more liberal in that respect than the American because there is no assimilation test for citizenship. On the enormously controversial issue of reforming the Basic Law's asylum guarantee, Joppke argues that the Federal Republic's grant of rights of appeal to the whole world was nonviable and out of step with all the other liberal democracies: "In the end, Germany has only adjusted its asylum law to the international standard. If this adjustment has appeared drastic and deviated from its usually incremental policy style, it is because an essential function of sovereignty, control of territorial access, has had to be recovered from a unique impairment" (p. 94). In the context of division, the "no immigration country" mantra seemed necessary. Germany was the homeland for all Germans. Reunification and the return of the *aussiedler* with the end of the Cold War permit a relaxation of the maxim and the normalization of immigration policy. The author shows that the "no immigration country" stance was in any case balanced by the provision of extensive rights for foreigners in the legal system. There was no real effort after 1974 to send the guest workers home, and they enjoyed access to the benefits of the welfare state equal to that of citizens.

No non-American has written with as much confidence and insight as Joppke brings to his discussion of the U.S. case. His tone is at times sardonic, and from his European standpoint

he obviously finds the chaotic American system entertaining. His review of race policies and affirmative action as well as the unseemly competition among immigrant groups to get in on the spoils provided by the American liberal conscience is especially telling.

Whereas Joppke sees aspects of immigration politics in America as faintly ridiculous and the German case as less appalling than commonly held, he expresses more or less unreserved scorn for British policy. He admits that Britain had a problem at least as serious as that created by Germany's imperative to be a homeland for the Germans. If the German problem was political boundaries too narrow to encompass the nation, the British problem was political boundaries that were wider than the nation (p. 100). The British Nationality Act of 1948 created no British citizenship but granted rights of entry and residence to hundreds of millions of British subjects around the globe, rights that no British official expected or wished to see exercised in large numbers. "The logic of British immigration policy is thus determined by the devolution of empire . . . [it] was to carve out the historical homeland nation from the vast empire, and to subject the rest to immigration control" (pp. 100–1).

British attempts to keep foreigners out have been more determined and successful than those of its democratic neighbors. According to Joppke, one reason British policy is so tough and mean-spirited is that it had never been deliberate government policy to encourage immigration and settlement from the empire and Commonwealth. Consequently, British elites felt few of the moral obligations expressed by their German counterparts toward the guest workers they had recruited. A second factor is the largely docile judicial system in Britain, which deprives migrants of constitutional protections from either administrative or legislative abuse. Due to the weaker legal and moral constraints, British policy moved in the opposite direction to that of Germany, "embracing rather than rejecting drastic solutions" (p. 114).

Joppke usefully criticizes and amends my argument that client politics is the typical form of immigration policymaking in liberal democracies (Gary P. Freeman, "Modes of Immigration Politics in Liberal Democratic States," *International Migration Review* 29 [Winter 1995]: 881–902). He agrees that client politics largely explains the development of U.S. policy throughout the postwar period and that of Germany until the recruitment stop of 1974, but he finds the model misleading for Germany thereafter and inappropriate altogether for Britain. Activist courts account for some aspects of U.S. expansionism, a factor that fails to fit into the client framework. In post-1974 Germany, the courts, welfare state rules, and the moral commitments of elites produced inclusive policies toward resident foreigners, while domestic security concerns motivated the government's resistance to new rounds of labor recruitment or the admission of large numbers of asylum seekers (p. 79). Because no important groups favored mass immigration to Britain, there was, by definition, no client politics.

Joppke's excursion into the politics of multicultural integration is highly illuminating. Readers may find his treatment of Germany most interesting if only because that case is so commonly misconstrued. Joppke concludes that the ethnonational citizenship model delineated by Rogers Brubaker (*Citizenship and Nationhood in France and Germany*, 1992) and the postnational model espoused by Soysal coexisted in Germany, the latter making the former possible. The ethnocultural mandate to reunify the German people was flexible enough to justify denying immigrants citizenship but at the same time embrace them as equal members of a postnational community. In neither case were immigrants expected

to assimilate; if carried far enough, assimilation would "destroy the ethnocultural texture of the nation" (p. 189).

Immigration and the Nation-State is essential reading for scholars in the field, and its clear prose and compelling storyline make it suitable as well for advanced undergraduates. The book perhaps concentrates too single-mindedly on domestic politics, giving only a passing nod to the European Union and presenting no data, as opposed to logic, in arguing against the influence of international courts and regimes on national policies. But Joppke presents a powerful argument for the predominance of national politics that others must confront in exploring the extranational dynamics of immigration policymaking.

Sovereignty: Organized Hypocrisy. By Stephen Krasner. Princeton, NJ: Princeton University Press, 1999. 207p. $69.95 cloth, $24.95 paper.

Bruce Cronin, *University of Wisconsin-Madison*

Few concepts in international relations have aroused as much debate and emotion among political leaders, activists, and scholars as that of sovereignty. Diplomats continually invoke it, transnational organizations attempt to circumvent it, and scholars debate its meaning and wonder whether globalization is making it obsolete. Yet, most accept the premise that sovereignty is not only the foundation of our international system but also one of the few consequential institutions we have in world politics.

Sovereignty: Organized Hypocrisy challenges both assumptions by offering a coherent and powerful analysis of how the theory and practice of sovereignty often diverge. The central thesis is that the characteristics usually associated with sovereignty (territory, autonomy, and recognition) do not provide an accurate description of the actual practices of the units within the international system. Specifically, Krasner argues that since its inception, sovereignty's primary attribute—domestic autonomy—is routinely compromised whenever political leaders believe it to be in their interest to do so.

Although this may sound like a classic neorealist argument, Krasner is not a typical realist, and his book moves well beyond a static structuralist account of state behavior. Although he uses a power-and-interest approach to examine how sovereignty is practiced in international relations, he takes the logic of this analysis farther than most realists have been willing to go. As do most realists, Krasner argues that it is impossible for any institutional arrangement at the international level to become deeply embedded—even one as fundamental as sovereignty.

Yet, whereas a realist may find strong systemic pressures that help maintain domestic autonomy for most states (e.g., the balance of power), Krasner makes no such assumptions. This is because his starting point is not "states" but the political elites who rule in the name of the state. Such individuals are primarily motivated by a desire to maintain their authority within the domestic sphere, so they can adopt a "logic of consequences" that is not consistent with either the norms or structural dynamics of the international system. In this sense Krasner's world is not a Hobbesian state of nature in which states act according to the necessities produced by international anarchy but, rather, a Machiavellian one, in which leaders act to maintain and expand their own power.

Krasner also departs from the assumption shared by realists and nonrealists that the core attributes of sovereignty—international recognition (legal sovereignty), indepen-

dence from outside actors (Westphalian sovereignty), and state supremacy (domestic sovereignty)—are necessarily connected. He argues that a state can have one of these attributes (e.g., international recognition) without enjoying others (e.g., state supremacy). Having made this point, he focuses primarily on Westphalian sovereignty and how leaders compromise this aspect through intervention and invitation.

Krasner argues that the basic principle of Westphalian sovereignty—domestic autonomy—is frequently compromised through coercive intervention by stronger states and by conventions and contracts that domestic political elites voluntarily sign. In each case, external actors influence domestic authority structures, thereby violating the fundamental norms of independence and autonomy. Thus, not only do powerful states routinely compromise the sovereignty of other states, but also domestic elites sometimes compromise their own sovereignty when it suits their interests.

The author supports this thesis through several case studies that cut across time and geographic region. Specifically, he examines the ways states' domestic autonomy is compromised through the conclusion of human rights conventions, minority rights treaties, international financial lending agreements, and conditions placed on the constitution of new states. In some cases states are forced to accept such intervention as the price of international recognition; in other cases they voluntarily agree to accept constraints on their domestic autonomy when such actions strengthen the ruling elite. Taken as a whole, the cases suggest that violations of sovereignty norms have been an enduring practice since the evolution of the nation-state system.

This book is an important addition to the literatures on sovereignty and international organization. Most studies present the institution of sovereignty as either empowering or constraining, and some examine how its meaning is constructed over time. Few are bold enough to suggest that it is irrelevant. Krasner also throws a curveball into the debate between institutionalists and structuralists. Although the two schools differ on the degree to which norms can become institutionalized in international affairs, both assume that, when and if they do, they can have a strong influence on behavior. Krasner offers the provocative argument that some norms do indeed become widely accepted and remain persistent over time, but even when they become institutionalized they are often ignored.

Krasner's study also contributes to the ongoing debate over whether sovereignty is changing or even becoming obsolete in the face of rapid globalization and the spread of transnational issues, such as human rights. Rather than address the influence of these factors on the future of sovereignty (as most studies have done), the author shows that states have never been "sovereign" in the way that many assume. Thus, current trends do not constitute a radical departure from practices that have occurred over the past few hundred years.

The book is powerfully argued and tightly organized, but there are several methodological and conceptual problems. First, the author presents his evidence primarily by chronicling clusters of cases in which international institutions or collectivities of states exercise influence on the domestic politics of other states. Yet, this leads one to ask how these examples compare to the large number of cases in which domestic autonomy is respected. How many violations does it take for one to conclude that they are routine and frequent? On a daily basis, diplomats and political leaders tend to respect one another's sovereignty most of the time. In fact, it could be argued that, given the high level of daily interaction among states, violations as the author describes them are

relatively rare. For example, Krasner discusses how international institutions imposed comprehensive minority rights requirements on new states in the periods around 1878, 1919, and 1992, in each case after a major war or systemic shock. These requirements constituted a significant intervention, but they involved relatively few states under extraordinary circumstances and only occurred three times in the past century.

Second, some scholars may challenge Krasner's interpretation of what constitutes a "violation" of sovereignty norms. The author defines Westphalian sovereignty in terms of domestic autonomy, but few would argue that this means states are unencumbered and free from all external constraints. If sovereignty is understood as a social construct, then the rights and duties contained within the institution are not fixed; rather, they are subject to interpretation and change by the collectivity of states. In fact, many argue that a state's sovereignty is derived from its participation in the international system, and therefore the members of the international community determine what counts as an international as opposed to a domestic issue.

This problem of defining violations is illustrated by the author's inclusion of contracts and conventions among the practices that compromise domestic autonomy. Any form of cooperation or collaboration reduces one's range of options in making policy. Political leaders voluntarily sign agreements in order to attain benefits they otherwise would have to forgo. It is difficult to understand how this violates their sovereignty. One can argue that the ability to enter into international treaties or contracts is actually an expression of sovereignty.

Finally, some may find the argument that sovereignty is not absolute and is often circumscribed by political expediency and the exercise of power is not particularly enlightening. There is virtually no political principle or concept that is either unambiguous or consistently applied in all contexts and under all circumstances at either the international or domestic level. The concepts of "freedom" and "democracy" are examples. Even in the best of circumstances, the application of these principles often has to be harmonized with other (often conflicting) principles. If this constitutes "organized hypocrisy," then all social interaction contains a degree of incongruity.

Gestures of Conciliation: Factors Contributing to Successful Olive Branches. By Christopher Mitchell. New York: St. Martin's, 2000. 333p. $65.00.

Paul D. Senese, *SUNY, Buffalo*

The end game of peace negotiations between international actors has received a great deal of scholarly attention over the years. Much of this effort focuses on the processes by which states, leaders, or diplomats achieve durable solutions to differences previously difficult to resolve. Christopher Mitchell offers a significant contribution to this literature by centering his attention on the observable starting points of peace, those with the potential to trigger a sequence of interactions that eventually produce an important agreement between previously conflicting sides.

Mitchell focuses his analytical energy not on general conflict resolution but on resolution of the most protracted and entrenched conflicts. The most significant contribution of his approach lies in its ability to advance our understanding not only of peace processes but also of peacemakers' adeptness in starting productive interactions between adversaries. Although he is interested in the full process of interactions

associated with peaceful international solutions, Mitchell singles out the importance of initial conciliatory gestures (or olive branches). This core concern allows him to differentiate, theoretically and empirically, between initial gestures that are quickly rejected and those that lead to a series of cooperative actions and reactions between foes. By closely assessing the key factors associated with both successful and unsuccessful conciliatory gestures, Mitchell draws conclusions that can guide academics, consultants, and diplomats in their selection of first-stage peace tactics.

The major focus is an assessment of what factors make it more likely that conciliatory initiatives will achieve any measure of success in bringing staunch international antagonists significantly closer to peace. Exactly what is meant by success? Mitchell, as he does with most aspects of beginning peace processes, devotes significant attention to answering this question (chap. 3). In the end, he suggests the possible utility of an ordinal scale of short- to medium-term success for conciliatory gestures (p. 57). In the abstract, this scale is fine, but operationalizing it would be (as Mitchell does point out) a difficult chore. This points to a general shortcoming evident throughout the book.

Although numerous hypotheses are presented (p. 124 and chap. 14), the book does not go nearly far enough in enabling them to be tested rigorously. Mitchell mentions this a number of times, but it still poses problems. For instance, getting two analysts (from the same or opposite sides of a dispute) to agree on the exact meaning of certain concepts would be almost impossible. Mitchell spends a great deal of time discussing the meaning of his concepts, but he does not offer operational definitions that would eliminate fuzzy interpretations of how the empirical world might support or falsify his hypotheses. Such operational measures are not always easy to provide, but their absence is notable. This is too bad, as this volume's painstaking attention to concept and nuance begs for testable empirical hypotheses.

In the final chapter, Mitchell offers thirty-nine hypotheses. He conceptualizes these as a list of factors that will have a positive effect on the success of conciliatory gestures. There is no mention, however, of their relative importance. Surely, the author must have good reason to believe that some are more integral to success than others. Mitchell is aware of this but suggests (p. 290) that sorting out relative importance is a matter for empirical investigation and testing. Unfortunately, the difficulty of matching operational indicators to his concepts limits our ability to do this. The influence of his book could have been even greater if he had developed a weighted list of these factors or, perhaps, a process model detailing the contingent or necessary status of some factors compared to others.

Much of the theorizing is connected to Mitchell's key empirical launching pad—the relations between Egypt and Israel from 1971 to 1979. The book offers a splendid account of the key interactions between these two states over the period, but much of the grist for Mitchell's hypothesis mill comes from his close consideration of this single case. There is attention to theory in the area, and occasional mention is made of other peace processes (e.g., the United States and North Vietnam, Argentina and Britain), but lessons learned from Israeli-Egyptian interactions (most notably those surrounding Sadat's offer to visit Israel in 1977) strongly influence the development of theoretical expectations. What if Mitchell had focused almost exclusively on another case? Would the hypotheses have been significantly different? This is, of course, the risk one takes basing inductive theory on a small number of cases.

In discussing structural changes that may be conductive to

conciliation (pp. 70–81), Mitchell gives a nod in the direction of quantitative social science by citing a few articles that deal with the "level" of conflict. These works are still worthwhile but are dated and have been surpassed to some degree by more recent efforts. For instance, the link between vital issues (including territorial integrity) and conflict escalation has been examined by such authors as Paul F. Diehl (ed., *A Road Map to War: Territorial Dimensions of International Conflict*, 1999), Paul K. Huth (*Standing Your Ground: Territorial Disputes and International Conflict*, 1996), and John A. Vasquez (*The War Puzzle*, 1993). Furthermore, chapter 10 contains an interesting discussion of signaling and its importance without mentioning prominent contributions by Bruce Bueno de Mesquita and David Lalman (*War and Reason: Domestic and International Imperatives* 1992), James D. Fearon ("Signaling versus the Balance of Power and Interests," *Journal of Conflict Resolution* 38 [June 1994]: 236–69 and "Domestic Political Audiences and the Escalation of International Disputes," *American Political Science Review* 88 [September 1994]: 577–92), and Kenneth A. Schultz ("Domestic Opposition and Signaling in International Crises," *American Political Science Review* 92 [December 1998]: 829–44). Nevertheless, Mitchell should be commended for reaching outside his own methodological orientation in referencing at least a small number of formal and quantitative international relations pieces.

Although *Gestures of Conciliation* has a number of shortcomings, on balance its contribution to the literature is overwhelmingly positive. Mitchell's attention to the importance of factors surrounding initial conciliatory moves in protracted conflicts is very worthwhile. His rich examination of a wide range of variables involved in the contemplation and implementation of such moves deserves to be read by practitioners and students of conflict resolution alike. This book should become a required text in graduate courses on the techniques of successful conflict resolution. Furthermore, its richness of ideas but lack of empirical testing provide fertile ground for future work, including many doctoral theses.

Taiwan's Informal Diplomacy and Propaganda. By Gary D. Rawnsley. New York: St. Martin's, 2000. 182p. $65.00.

John F. Copper, *Rhodes College*

This book assesses how Taiwan, the nation officially known as the Republic of China but called "Taiwan Province" in the People's Republic of China, uses propaganda to pursue foreign policy objectives, attain its national interests, and, even more than that, survive.

Taiwan is a unique case in that its diplomacy is hindered by the fact that Taipei does not have official relations with very many countries (around thirty in recent years), and Beijing pressures the global community to bar Taiwan from international organizations (especially those of a political nature). China wants to isolate Taiwan in order to force its leaders into talks that will lead to its absorption or reunification. All of this means that Taiwan must use propaganda more and diplomacy less.

Rawnsley begins by defining propaganda in a "softer" way than most other scholars: Communications that are intended for all to hear, or essentially publicity, in contrast to diplomatic communications, which may be private or public and are more formal. He maintains that all countries use propaganda, and these publicity efforts should not be seen so much as schemes to control the hearts and minds of people in other nations by using lies and misinformation; rather, this open

information dissemination works together with diplomacy in the normal and legitimate conduct of foreign policy.

Rawnsley begins the Taiwan case with a brief history of diplomacy there and the past use of propaganda. Although only a terse summary, it demonstrates that Taiwan has long been in the propaganda business, extensively so, and for good reason. After the Nationalists fled the mainland in 1949, Taipei had to struggle to win support from the United States. Congress and the American people were critical to this effort. The Cold War advantaged Taiwan in subsequent years, but this was never an easy task. When Beijing won a seat in the United Nations in 1971 and, in 1979, established diplomatic relations with the United States, Taiwan's only important ally, propaganda became an even more important part of its diplomatic campaign. In fact, says Rawnsley, Taiwan's efforts to defend its sovereignty depended upon its ability to convince others, especially Americans, it deserved support.

In chapter 2, the author examines in particular Taiwan's loss of diplomatic relations and notes how propaganda filled a void and became more a functional part of its foreign relations as a result. The terms flexible diplomacy and cultural diplomacy coined at this time reflected the new even more important role of information, Rawnsley argues.

Rawnsley proceeds to assess Taiwan's foreign assistance, its "tailored" use of information, and how propaganda efforts have been changed or updated to fit Taipei's altered view of the world. He also delineates the role of presenting knowledge about Taiwan and Taipei's work to build positive perceptions in other countries. Rawnsley suggests that in a number of cases these efforts had feedback and influenced domestic policies in Taiwan. Democratization was the most important of these, being seen in Taiwan as making the country more attractive abroad and thereby facilitating its diplomatic efforts. Democratization thus became a central theme, which helped Taipei counter Beijing's campaigns to delegitimate Taiwan. A specific example was Taiwan's decision to end martial law in 1987. Much was made of this because it would impress other countries, even though polls indicated most citizens did not object to it and did not want it rescinded. Rawnsley also notes that Taiwan took advantage of the improved global image that resulted from its democratization at a critical time, in 1989, when China's stock plummeted after the Tiananmen Massacre. Taiwan's "information people" later promoted the plenary elections in 1991 and 1992 and the watershed direct presidential election in 1996 (the first in 5,000 years of Chinese history).

The third chapter is devoted to an assessment of the government agencies responsible for making and disseminating information. Rawnsley focuses mainly on the role of the Government Information Office. He discusses at considerable length its links to and relationship with the foreign ministry and other organs of government. He also cites cases of success and failure, drawing on the experiences of foreign offices, especially those in Europe, South Africa, and New Zealand.

Chapter 4 analyzes Taiwan's use of news agencies, newspapers, and radio to present its case abroad. Television is not broadcast abroad very much, and the Internet has only recently become a means of delivering information (although Taiwan has been very quick and adept at using it). The author describes how Taiwan more than other nations emphasizes its relationship with the media. He also notes that Taiwan uses the media to communicate with China. There is, of course, a propaganda war going on between Taipei and Beijing; but clarifying policies and calling for negotiations also comprise efforts to initiate diplomatic talks, he says.

In the concluding chapter, Rawnsley reiterates his conten-

tion that propaganda and diplomacy are closely linked. Taiwan's extensive use of information, he asserts, is evidence for this. Rawnsley also delineates ties between information and business. Again, Taiwan is a good case. But he also notes that Taiwan has been blessed by having money to pay for information dissemination, which is costly. Thus, although Taiwan is a model for other nations to follow, many would have to spend considerable sums to do what Taiwan has accomplished. Finally, Rawnsley connects propaganda to lobbying efforts, especially in the United States.

Rawnsley uses the literature adroitly to prove his points, citing a broad range of works on both diplomacy and propaganda. He applies them well to Taiwan. He also looks broadly and carefully into how Taiwan's efforts have worked, but he might have made more of the successes in the United States. Taiwan is seen in Washington as having very good diplomats, and the reason for that is largely propaganda efforts (or lobbying) with Congress. Taiwan's pseudo-embassy in Washington is said to be one of the three most effective there (along with Japan's and Israel's) and has a reputation for excellence compared to China's embassy and staff.

Taiwan's Informal Diplomacy and Propaganda is unique in part because no other scholar has written a book-length study on this topic. But Rawnsley also does an expert job of assessing the bureaucratic system involved, the relationship of propaganda and foreign policymaking, links with the media, and results in the field. He has produced a work that is recommended to scholars interested in Taiwan's foreign relations and its use of information in pursuit of its national interests, as well as to those looking for a good case study of a government that ties together propaganda and diplomacy.

The Moral Purpose of the State: Culture, Social Identity, and Institutional Rationality in International Relations. By Christian Reus-Smit. Princeton, NJ: Princeton University Press, 1999. 199p. $35.00.

Jennifer Sterling-Folker, *University of Connecticut*

The central puzzle motivating this book is why different systems of sovereign states develop different types of systemic institutions. Why did Greek city-states favor arbitration, whereas Italian city-states adopted what the author calls "oratorical diplomacy," the absolutist state preferred "old diplomacy" instead, and the modern nation-state relies on international law and multilateralism?

From the start it is clear that Reus-Smit is not interested in explaining just any interstate interaction but cooperative interaction specifically. He has clear sympathies with a neoliberal perspective on the subject (p. 11), although he is also quick to point out explanatory weaknesses. Reus-Smit makes a strong case that the motivating puzzle has not been answered in a satisfactory manner by any alternative theoretical perspective. Although neorealists can be faulted for failing to recognize that there is a puzzle, neoliberals can be criticized for their dependence on rational choice to address it. And although a constructivist himself, Reus-Smit is willing to take to task such constructivists as John Gerard Ruggie and Alexander Wendt for their exclusive reliance on the theoretically underdetermining attribute of sovereignty as a means to explain systemic institutional evolution.

Alternatively, Reus-Smit argues that such institutions can be traced to "the complex of values that define state identity" (p. 30). That is, their source lies in the meta-moral purposes that justify the existence of the state in any historical period, which states then transplant to the systemic realm as the

institutions that will guide interstate cooperation. "Informed by prevailing beliefs about the moral purpose of the state, the systemic norm of pure procedural justice shapes institutional choice, licensing some institutional solutions over others" (pp. 33–4). The result is different cooperative institutional arrangements under different sovereign state systems. Because the Greek city-states saw public discourse as the means to arrive at what was good and just, this was also the cognitive, institutional template for their dealings with one in the form of arbitration. Similarly, because "the metavalues that define legitimate statehood and rightful state action in modern international society are based on an individualist social ontology," we find the development of systemic institutions such as international law and multilateralism.

There is much to like in Reus-Smit's approach. The historical sweep of this study is both absorbing and amply demonstrates the tremendous scope and depth of his approach. The author breathes new life into the interpretation of myriad historical institutions, and along the way he grapples with a number of puzzles specific to particular state systems. There is also something intuitively correct in his argument that systemic institutions derive from the same meta-morals that give the state its purpose in any given period. Thus, it comes as no surprise that modern international law approximates American norms and institutional preferences so closely, although as Reus-Smit points out the foundation for these systemic institutions was laid by historical social practices begun in and shared by the nation-states of Europe as well.

As to whether Reus-Smit's constructivist perspective manages to address his central puzzle in an entirely satisfactory manner, the answer is mixed. One problem is that it is not always clear what the logical connection is between the value complexes Reus-Smit asserts for the system of sovereign states and the cooperative institutions supposedly produced by them. The link between Italian internal patron-client relationships and the external "oratorical diplomacy" style seems logical enough, but not the link between Greek internal public discourse and external arbitration (are decisions by third-party judges really the same thing as public discourse about which all decide?). The asserted link between the absolutist state and "old diplomacy" is not just undertheorized but contradictory to the main thesis (the value complexes simply licensed war-waging and almost no cooperation). And the connection between the modern state's meta-moral purposes and international law also has logical difficulties (if the nation-state's purpose is to protect its own citizens, why would it want mutually binding interstate contracts?).

One reason some of these links may be problematic is because Reus-Smit's constructivist alternative is only concerned with the production of cooperative institutions. As do most approaches to the phenomenon of interstate cooperation, Reus-Smit's alternative separates the subject of cooperation from the subject of war as if they were on different causal tracks. His description of the modern period, for example, focuses on the treaties following major wars but makes only a passing reference to the wars themselves, as if they were mere aberrations in the on-going and apparently primary process of cooperative interstate institution-building. But treating the historical record in this way raises an obvious question. If the state's internal meta-moral purposes produce cooperative institutions at the interstate level, then why does interstate war even occur?

Of course, it seems unfair to expect Reus-Smit to address such a question when his own interests lie with cooperative rather than violent interaction. But in order to illustrate his argument, Reus-Smit must evoke an historical record that makes elements of his own approach, as well as the standard analytical separation of the two phenomena, increasingly questionable. Just how important can these values be to interstate interaction in general if the meta-moral purposes Reus-Smit has identified cannot also account for violent interaction and hence can only cover half (and sometimes the least interesting) of the interstate historical record?

There is also the possibility that an opportunity is missed. What Reus-Smit provides is the constructivist foundation for a holistic approach that, if meta-moral purposes are appropriately identified and carefully linked to resulting interstate social practices, could explain so much more than cooperation. The same puzzle that informs Reus-Smit's study of cooperation occurs for war-making as well. Not only do different systems of sovereign states produce different cooperative institutions, but also they wage war differently. Is it not possible, then, that the moral underpinnings of the state could explain institutional variance in both interstate war-waging and cooperation? The chapter on the absolutist state provides ample supporting evidence for such a proposition, but because Reus-Smit is only looking for cooperative systemic institutions, he misses the chance (at least in this book) to account for war and peace under the same constructivist umbrella.

In order to develop such a holistic approach, however, the intimacy between internal moral authority and the power to disseminate it as a basis for interstate interaction would have to be examined more critically. Reus-Smit does not deny that there is a relationship between power and the moral underpinning of interstate cooperation (e.g., pp. 152–4), but he does consistently treat the two as if they were explanatory alternatives to one another. Yet, as many indigenous peoples and human rights activists could tell us, modern international laws (and the multilateral forums where they are propagated) are more often tools for status quo oppression by the powerful than vehicles for the universal protection of individual rights.

Thus, we find a correspondence between the interstate cooperative institutions established by the modern nation-state and the purpose of the modern nation-state to protect what rights its citizenry enjoys against the claims of individuals from other states. This is a correspondence that is fundamentally about power, that is only made possible with power, and that seeks to reinforce the power of haves against the have-nots. Reus-Smit's excellent if not foundational book points us in the right direction for unlocking the puzzle of cross-systemic institutional difference. Yet, oddly and simultaneously, it underscores the cognitive distances we must still traverse in search of the appropriate keys.

Causes of War: Power and the Roots of Conflict. By Stephen Van Evera. Ithaca, NY: Cornell University Press, 1999. 270p. $35.00.

Allan C. Stam III, *Dartmouth College*

Stephen Van Evera explicitly sets out to accomplish two tasks. The first is to present a set of five hypotheses on the causes of war grounded in "misperceptive fine-grained structural realism" (p. 11). He lists (1) false optimism about the outcome of a future war, (2) perceived first-mover advantages, (3) opening and closing windows of opportunity and vulnerability, (4) cumulativity of resources, and (5) beliefs about the offense-defense balance. He then develops 23 related hypotheses. The second task is to test some of the major hypotheses (the second, third, and fifth) against a small

set of cases. He succeeds at the first task but is not so successful at the latter. He also briefly speculates on the effects of the "nuclear revolution."

The major contribution of *Causes of War* lies in Van Evera's demonstration of the similarity of what heretofore have been presented as competing research paradigms: realism and constructivism. Realists traditionally have focused on the material bases of the balance of power and its presumed effects on international relations; constructivists have emphasized the role of ideas and their socially constructed nature. Van Evera expands the explanatory and predictive power of realism by incorporating leaders' beliefs and misperceptions about "fine-grained power" relations between states (p. 7). In doing so, he demonstrates, however unwittingly, the underlying similarities of constructivism and realism. This is a particularly valuable contribution to international relations theory.

As Brooks and Wohlforth point out, "establishing a strong, independent role for ideas will be particularly difficult when material constraints are especially significant and/or when there is relatively little lag between material and policy changes" (Stephen Brooks and William C. Wohlforth, "Power, Globalization and the End of the Cold War: Reevaluating a Benchmark Case for Ideas," *International Security* 25 [Winter 2000–2001]). Van Evera manages this well, within the inferential constraints of his method. By noting that "realism thus is most powerful—if we repair it by shifting its focus ... from power itself to national perceptions of power" (p. 9), he blurs the distinction between realists and constructivists, whose key contribution has been to note the explanatory power of socially constructed national beliefs, or ideas about power and relations thereof (e.g., Alexander Wendt, *Social Theory of International Politics*, 1999).

There is much to like about the book, but there are problems. From an empirical perspective, several weaknesses stand out. Van Evera uses a method perhaps best termed historical constructivism, an empirical analog to the theorized social construction of ideas at the heart of the paradigm. The hypothesis tests consist of clever historical constructions based on carefully selected facts and recollections through which the author demonstrates that beliefs (also referred to as ideas or misperceptions) lie at the core of his microfoundational realist agenda. Van Evera commits four inferential sins that further obscure the eroding distinction between his brand of realism and social constructivism to the extent that, empirically, any remaining differences might be considered superfluous.

First, the observations from which the hypotheses are inferred are the same ones used to test them. For example, when testing his "jumping the gun" hypothesis (p. 63), Van Evera presents the historical evidence in support of the test in footnote 107. Rather than reveal an historical insight, however, this note simply refers the reader to the evidence presented in note 55 (p. 49) and notes 70 and 71 (p. 52), found in the section where the hypothesis was developed. Van Evera is well aware of this and other inferential problems but seems blissfully unconcerned. "The orthodox methodology creed ... requires that cases not be selected on the dependent variable. ... It warns against testing theories with cases from which the theory was inferred. ... It warns against selecting atypical cases loaded with the causal phenomenon. ... I have never found these rules useful, and my case studies break them all. Readers can judge if my recalcitrance did any harm" (p. 12). What do we lose by adopting this historical constructivist method? Foremost is the ability to distinguish between cause and effect and the ability to falsify judgments thereof. From a methodological perspective, this

is not Wendt's positive social constructivism but historical constructivism in the best tradition of Foucault at his most cynical (see Michel Foucault, *Archaeology of Knowledge*, 1982).

Second, both the tests and hypotheses rely completely on a review of the secondary literature on the origins of World War I and others. We become captive to the judgment of the historians Van Evera carefully selects to construct his argument. On World War I, for example, a more balanced review of the historiography reveals that the Russians' intentions and degree of military optimism were not as clear as Van Evera claims they were. As Goemans points out, "on the one hand, the secondary literature often asserts that like France and Germany, Russia expected the war to be short and victorious. On the other hand, closer study reveals that well-informed decision-makers were often much less sanguine about Russia's relative strength and the duration of a war" (Hein Goemans, *War and Punishment: The Causes of War Termination and the First World War*, 2000, p. 127, in particular note 9).

Third, Van Evera does not randomly sample the facts he cites from the population of states that might go to war. Rather, it appears the evidence has been selected both on the value of the dependent variable and also to bolster the author's argument, not necessarily to get to the truth of the matter (e.g., note 118, p. 66). Since we cannot identify the general population of crises at risk of escalation to major war, it becomes difficult to have confidence in the few policy recommendations based on the findings.

Fourth, the evidence typically consists of individuals' observations, but the stories being tested are state-level arguments. Why is this of concern? Early on, Van Evera makes clear what his brand of realism is all about: citing Robert Keohane, he summarizes: "1. States are the most important actors in world politics, 2. States are unitary rational actors, and 3. States seek power ... and they calculate their interests in terms of power" (note 11, p. 7). If states are the unitary rational actors Van Evera assumes them to be, then we should search for evidence at the state level or in observations about state characteristics. Instead, most of the evidence is individuals' recollections of how the tide of myopic decisions made by others drove events forward. We could just as easily construct a story using Van Evera's evidence that is more consistent with the psychological explanations of dispute escalation found in Richard Lebow's *Between Peace and War* (1984).

The historical constructivism in *Causes of War* will prove seductive to many because of the polished prose that leads the reader though what appear to be powerful tests of myriad hypotheses. Although Van Evera's construction of fine-grained realism succeeds on this level, it fails on a more rigorous epistemological level of positive social science. In the end, readers will likely take from the book what they bring to it. Realists may find the emphasis on ideas somewhat puzzling, and positivists will be deeply disappointed by the outright flaunting of scientific rules of inference and in the end will find little of systematic substance to take with them.

Globalizing Concern for Women's Human Rights: The Failure of the American Model. By Diana G. Zoelle. New York: St. Martin's, 2000. 169p. $49.95.

Fiona Robinson, *Carleton University*

It is difficult to argue with Diana Zoelle's claim that liberal democracy, as conceived and developed in the United States, is a problematic model in globalizing concern for women's

human rights. Moreover, when she suggests that U.S. ratification of the International Convention on the Elimination of All Forms of Discrimination against Women (CEDAW), although not a panacea for the attainment of full equality, would constitute an important step toward alleviating women's oppression, she is probably correct. Finally, although her claim that the potential currently exists to accord human rights to all people in a world community that is less torn apart by bipolar enmity, less subverted by ideological tensions, and less compromised by the economic priorities of multinational corporations is probably not correct, one cannot help wishing that it were.

It is difficult to disagree with most of Zoelle's arguments, but one is left with the feeling that a great deal more needs to be said. Indeed, the subject of her book—the failure of liberalism, especially in the United States, to deliver women's human rights—is of great importance and has tremendous potential. Yet, too many pages are devoted to long appendices and annotated articles of CEDAW; much of what remains is undertheorized in the extreme. Not until the last chapter is the reader introduced, in a most cursory way, to the central conceptual debates that bear heavily on the topic of this book, such as the relationship between liberal democracy and capitalism; the postmodern ideas of identity/difference, self/other; and the feminist debates about the relevance of "rights talk" to the moral and social lives of women. Moreover, the book puts forward a potentially radical and even original thesis, but the end result is neither. Zoelle is ostensibly critical of liberal democracy, yet she seems to find the whole idea of universal human rights beyond question. A more thorough and profound interrogation of the notion of rights as a moral concept, as well as of the political relationship between rights and power, could perhaps have made this interesting and important book a magnificent one.

Globalizing Concern for Women's Human Rights is organized into five chapters, and includes the full text of CEDAW as an appendix. Zoelle begins by arguing that U.S. civil rights legislation has failed in its task of correcting past discrimination against women and racial minorities. She claims that civil rights are different from and secondary to human rights; the latter are universal and incontrovertible, whereas the former are augmentary, supplemental, and subject to the vagaries of politics. The United States has limited itself to an emphasis on civil rights but has failed to take seriously the human rights of its own citizens, while simultaneously choosing to police the behavior of states that have ratified international treaties.

Zoelle raises some important points in this chapter; indeed, perhaps her most insightful claim here is that liberal democracy, as it exists in the United States, is structured around exclusion and oppression. "Further, because the practice of exclusion is not simply civil and legal, but is also sociopolitical, economic, and cultural, more is required to alleviate these problems than civil remedies" (p. 15). This is certainly a crucial argument, but Zoelle's distinction between civil and human rights is perhaps too strongly made. When she states that "civil rights discourse ... disregards social and economic preconditions for unequal treatment and civil laws are certainly no substitute for inherent human rights" (p. 16), she seems unaware that this argument can and has been leveled at human rights discourse as well, by both feminist and nonfeminist critics. (See chapters by Tony Evans, Spike Peterson and Laura Parisi, Caroline Thomas, and Anthony McGrew and John Galtung in Tony Evans, ed., *Human Rights Fifty Years On: A Reappraisal*, 1998).

Chapter 2 examines the U.S. record on women's human rights and argues that CEDAW is the best means available to articulate a commitment to the rights of women in particular. Zoelle rehearses now familiar (yet still important) feminist arguments regarding the public/private dichotomy in liberalism and state complicity in systematic human rights violations against women. Chapter 3 discusses the ongoing debate regarding difference among women and the problem of constructing a coherent voice in the expression of "women's" concerns. The arguments are clearly stated, and the key issues are highlighted, but Zoelle's position lacks rigorous theoretical support. When she claims that "it is not difference that must be eradicated but the attendant asymmetry in power arrangements" (p. 64), the reader is left unsure of the conceptual starting points that lead her to this conclusion. Indeed, when Zoelle makes the bold, and somewhat paradoxical, claim that "diversity is a fundamental truth" (p. 64), she seems to be appealing at once to poststructuralist approaches as well as to a kind of moral and epistemological absolutism.

Chapter 4 takes a slight turn to examine U.S. foreign policy vis-à-vis human rights. Zoelle describes as "perplexing" the record of the United States on arms sales, economic assistance to oppressive regimes, and support of U.S.-based multinational corporations in highly repressive nations. Again, it is certainly to her credit that she makes these arguments, yet it is disappointing that she has not drawn on the bounteous literature that offers some credible explanations for these "perplexing" activities. (See Tony Evans, *U.S. Hegemony and the Project of Universal Human Rights*, 1996, and Noam Chomsky and Edward S. Herman, *The Political Economy of Human Rights*, 1979).

The final chapter is in many ways the best. Zoelle begins to engage with theoretical and conceptual arguments and introduces some important debates within feminism, liberalism, and human rights theory. The relationship between democracy and capitalism is finally mentioned, but the tone is more assertive than analytical: "Liberal democracy in conjunction with a market economy fails to address the situation of those within the state who have been, historically and globally, denied full access to the institutions and instruments that regulate their lives" (p. 106).

The spread of capitalism and liberal democracy worldwide seems to contradict Zoelle's earlier claim that "the potential exists to accord human rights to all people in a world that is ... less compromised by the economic priorities of multinational corporations" (p. 4). Curiously, her brief discussion of global capitalism and the exclusion for which it is responsible is followed almost immediately by a reference to the poststructuralist arguments of David Campbell, who regards exclusion, by contrast, in discursive terms: "identity is achieved through the inscription of boundaries that serve to demarcate an inside from an outside, a self from an other, a domestic from a foreign" (quoted in Zoelle, p. 107). Some theoretical consideration of materialist versus discursive analysis is surely needed here. This is also true of the hugely important question raised but not explored on page 111: "Can a rights discourse serve as a vehicle for expression of women's claims?" Indeed, that this question is even worth posing demonstrates the potential fragility of the premise on which Zoelle's entire project is based. Even if the author would answer the question in the affirmative, as I believe she would, by engaging with the feminist criticisms of rights-based approaches she would have strengthened the arguments of her ambitious and important book.

Human Rights as a Common Concern

CHARLES R. BEITZ *Bowdoin College*

The doctrine of human rights has come to play a distinctive role in international life. This is primarily the role of a moral touchstone—a standard of assessment and criticism for domestic institutions, a standard of aspiration for their reform, and increasingly a standard of evaluation for the policies and practices of international economic and political institutions. International practice has followed the controlling documents of international law in taking a broad view of the scope of human rights. Many political theorists argue, however, that this view is excessively broad and that genuine human rights, if they are to be regarded as a truly common concern of world society, must be construed more narrowly. I argue against that perspective and in favor of the view implicit in contemporary international practice, using the right to democratic institutions as an example.

More than fifty years have passed since the U.N. General Assembly adopted the Universal Declaration of Human Rights, and in that time the doctrine of human rights has come to play a distinctive and in some respects an unexpected role in international life. This is primarily the role of a moral touchstone—a standard of assessment and criticism for domestic institutions, a standard of aspiration for their reform, and increasingly a standard of evaluation for the policies and practices of international economic and political organizations. This role is carried out in a variety of ways. Perhaps the most visible is the increasing willingness to regard concern about human rights violations as an acceptable justification for various kinds of international intervention, ranging from diplomatic and economic sanctions to military action, in the domestic affairs of states.

But coercive intervention in any form is exceptional, and the political functions of human rights usually are considerably less dramatic. For example, a government's human rights record may determine eligibility for development assistance programs, or human rights conditions may be attached to internationally sponsored financial adjustment measures. The likely effect on satisfaction of human rights may function as a standard of evaluation for the policies of international financial and trade institutions. In the United States, legislation requires periodic reporting by the government regarding human rights practices in other countries and makes eligibility for certain forms of preferential treatment in U.S. foreign policy dependent on satisfaction of minimum human rights standards. In various parts of the world, most notably in Europe, regional codes have been adopted, and there is a developing capacity for adjudication and something like enforcement (even the European Court of Human Rights' capacity to hold governments accountable lacks

the machinery of coercion typically associated with adjudication within the state).[1]

The public role of human rights also has been important—more so perhaps than generally recognized in the United States—beyond the sphere of intergovernmental relations. Human rights have served as bases for standard setting, monitoring, reporting, and advocacy by nongovernmental organizations at both the domestic and the international levels of world politics (Best 1995; Korey 1998). To whatever extent contemporary international political life can be said to have a "sense of justice," its language is the language of human rights.

I do not mean to overstate the case. Notwithstanding the hopes of its authors, the Universal Declaration does not function today as an "international bill of rights." The international capacity to enforce the requirements of human rights law on states is at best embryonic. Outside Europe, most individual victims of human rights abuses have no effective appeal beyond their domestic courts, if there. And even in countries within the global "human rights culture" there is great variation in the degree to which internationally recognized human rights are embedded in domestic legal systems.[2] The juridical role of human rights is both limited and uneven. But none of this shows that the national foreign policy measures, international institutions, and nongovernmental organizations dedicated to the advancement of human rights are politically inconsequential. In fact, the global human rights regime is almost certainly more influential today than at any time since World War II.[3]

This fact recalls a longstanding worry about the doctrine of international human rights, expressed variously in terms of its alleged partiality or parochialism.

Charles R. Beitz is Professor of Government, Bowdoin College, Brunswick, ME 04011.

[1] Notwithstanding, governments seem to acknowledge the court's authority, as the decision by the British government regarding the treatment of homosexuals in the military illustrates (*Financial Times* 1999). On the variety of roles played by human rights in intergovernmental relations, see, e.g., Forsythe 2000, pt. II; Vincent 1986, esp. chaps. 4–6; and the case studies in Risse, Ropp, and Sikkink 1999.

[2] The idea of a human rights culture derives from the Argentinean jurist Eduardo Rabossi (Rorty 1993, 115).

[3] Precisely why this should be true is an interesting question. There is a provocative discussion that focuses on the growth of the European human rights regime in Moravcsik 2000.

In practice, the worry arises as an objection to external measures that are intended to induce a government to comply with the doctrine's requirements. Such measures by another government or an external organization are sometimes said to constitute the imposition of foreign values upon a culture whose history and conventional moral beliefs do not support them—in the extreme case, a kind of postcolonial imperialism.

There is a reflection of this worry at the theoretical level in a tension between two conceptions of human rights that can be found in philosophical thought. According to one conception, human rights represent the common element in a range of views about social justice or political legitimacy found among the world's cultures. A variant of this position, which is more permissive as to what might be counted as a human right but is motivated by a similar idea, regards human rights as political standards that would be reasonable to accept regardless of one's (culturally influenced) views about social justice or political legitimacy. This notion might be expressed by saying that human rights strive to be nonpartisan, nonparochial, or neutral among conflicting political cultures and ideologies.[4] I shall call this the nonpartisan or restricted conception of human rights.

The other conception regards human rights as distinctive of a particular view or family of views about social justice or political legitimacy. Although a list of human rights might not be a complete description of the requirements of social justice for a society, on this conception it would be more than the common element found among, or acceptable to, otherwise divergent views of social justice. That is, human rights identify conditions that society's institutions should meet if we are to consider them legitimate. But because there is no general reason to believe that these conditions are included in all the views about social justice or political legitimacy that exist in the world—or even among those that have achieved widespread acceptance in individual societies—there is no claim that human rights are nonpartisan. On this view, in contrast to the first, the advocate of human rights takes a stand on controverted questions of political theory. I call this the liberal or full conception of human rights.[5]

Many people will think that the restricted conception is the more plausible because it seems to embody a tolerance of culturally embedded moral differences

that is missing from the liberal view. But there is something paradoxical about this thought. Once we begin to describe evaluative standards for social and political institutions, it is hard to explain why we should stop short of a full description of these requirements as we see them. Of course, any such standards should be appropriate for the empirical circumstances in which they are supposed to apply, and it is important to add that this will leave some room for variation. But the intent would still be to state conditions for the legitimacy of institutions. If this is the intent, then why should we stop short of a full, liberal conception of human rights? And what would be the principle of distinction between the full and the restricted conceptions?

My purpose here is to explore the thinking that might lead someone to advocate a nonpartisan or restricted view of human rights. More precisely, I shall take up one aspect of this subject: Does the nonpartisanship, nonparochialism, or neutrality of a set of rights, in itself, provide a reason to treat these rights, as opposed to a more extensive set like that found in international doctrine, as having a special status in international affairs? In putting the question this way, I mean to distinguish considerations of ideological and cultural pluralism from various other kinds of reasons for giving some political aims priority over others—for example, reasons of urgency, efficiency, and institutional competence. These other reasons are obviously important and may often prove decisive in establishing priorities for political action, but they are also more easily understood, so for now I lay them aside. I will conclude—tentatively, because I cannot give the view an affirmative defense here—that considerations of ideological and cultural pluralism need not, in themselves, limit the scope of a plausible doctrine of international human rights, although they may have important bearing on reasoning about the connection between human rights and political action.

INTERNATIONAL HUMAN RIGHTS AS PARTISAN STANDARDS

To place the theoretical question in its political context, I begin with some summary remarks about the history and content of the doctrine of human rights as we find it in international law and practice.

Although the contemporary international doctrine of human rights has many antecedents, both philosophical and political, it is principally a legacy of World War II. It arose, on the one hand, from the statement of allied war aims in the Atlantic Charter (1941) and, on the other, from persistent pressure brought by individuals and groups outside government for a declaration of political principles for the postwar world. The Preamble to the United Nations Charter (adopted in 1946) affirms "faith in fundamental human rights," and Article 1 commits the organization to encourage respect for "human rights and for fundamental freedoms for all." (By contrast, there was no mention of human rights or any analogous idea in the Covenant of the League of Nations [Lauren 1998, chaps. 5–6].) The

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[4] In an article whose title ("Human Rights as a Neutral Concern") inspired the title of this paper, Scanlon (1979, 83) describes human rights as "a ground for action that is neutral with respect to the main political and economic divisions in the world" and as standards that "are not controversial in the way that other political and economic issues are." He does not suggest, as do some of the writers considered below, that human rights aspire to be neutral among all conceptions of justice or legitimacy. Rawls (1999a, 65, n. 4 and accompanying text) cites the Scanlon article as a source for the conception of human rights in *The Law of Peoples*.

[5] Donnelly (1999, 81) characterizes human rights as "a distinctive, historically unusual set of social values and practices." Others who have espoused a liberal view of human rights include Waldron (1993, 10–24), Nino (1991, passim), and Rorty (1993). Needless to say, agreement about the scope of human rights can coexist with disagreement in other dimensions.

charter does not give content to the idea of human rights and fundamental freedoms, however. For that one must refer to the Universal Declaration (1948) and two international covenants, one on civil and political rights and the other on economic, social, and cultural rights (both 1966). It bears remembering that the declaration is just that, a declaration of the General Assembly without the force of law, whereas the covenants are treaties to which national governments have acceded. Together these documents, which often are referred to collectively (and, as I suggested earlier, misleadingly) as the International Bill of Rights, constitute an authoritative catalog of internationally recognized human rights.

There are various ways to classify the rights enumerated in these documents. For our purposes it is useful to think of internationally recognized human rights as falling roughly into five categories, although it is less important to agree about categories than to appreciate the scope and detail of the enumerated rights.

1. Rights of the person refer to life, liberty, and security of the person; privacy and freedom of movement; ownership of property; freedom of thought, conscience, and religion, including freedom of religious teaching and practice "in public and private"; and prohibition of slavery, torture, and cruel or degrading punishment.

2. Rights associated with the rule of law include equal recognition before the law and equal protection of the law; effective legal remedy for violation of legal rights; impartial hearing and trial; presumption of innocence; and prohibition of arbitrary arrest.

3. Political rights encompass freedom of expression, assembly, and association; the right to take part in government; and periodic and genuine elections by universal and equal suffrage.

4. Economic and social rights refer to an adequate standard of living; free choice of employment; protection against unemployment; "just and favorable remuneration"; the right to join trade unions; "reasonable limitation of working hours"; free elementary education; social security; and the "highest attainable standard of physical and mental health."

5. Rights of communities include self-determination and protection of minority cultures.

I note, but shall not discuss, that other international agreements have elaborated and enlarged the scope of human rights in the areas of genocide, slavery and forced labor, racial discrimination, apartheid, discrimination against women, and the rights of children.[6]

There has been a long-standing dispute in official international discourse about human rights doctrine on two major points: whether the international community should recognize any priorities, either moral or pragmatic, among categories of rights (particularly between civil and political as against economic and social rights) and whether human rights doctrine should take note of

cultural differences in a way that would make the content of a person's human rights depend upon features of that person's culture. The last major international conference on human rights, conducted in Vienna in 1993, considered these issues at length. The final act of the conference declined to set priorities among categories, holding that "all human rights are universal, indivisible and interdependent and interrelated." Although it recognized that "the significance of national and regional particularities . . . must be borne in mind," it declared that "it is the duty of States, regardless of their political, economic and cultural systems, to promote and protect all human rights and fundamental freedoms" (United Nations 1993, sec. I.5).

Human rights are sometimes thought to set a minimal standard, but it is not obvious what this can mean. The rights of the declaration and the two covenants, taken in their entirety, include requirements that bear on nearly every significant dimension of a society's basic institutional structure, ranging from protections against the misuse of state power to requirements concerning the political process, welfare policy, and the organization of the economy. In scope and detail, international human rights are not more minimal than, say, the requirements of Rawls's principles of social justice. And those principles are not minimal in any very interesting sense.

Still, one can acknowledge the scope and detail of internationally recognized human rights without giving up the idea that they are or should aspire to be neutral or nonparochial standards. So it may be useful to recall, briefly and without critical comment, some recent instances in which it has been said that human rights are not neutral because they conflict with practices endorsed by one or another of the world's major conventional moralities. All of these are familiar in the human rights literature.

One example is the dispute about "Asian values." In the last decade some East Asian political leaders (e.g., Lee Kwan Yew of Singapore and Mahathir Mohamad of Malaysia) argued that some of the political and civil rights found in the international doctrine—mainly freedom of expression and political participation—are incompatible with traditional Asian political beliefs, which value social harmony over public dispute and the collective pursuit of shared interests over the individual pursuit of private interest. The civil and political rights of the declaration were distinctively "Western" values. For this reason, it was said, international pressure for domestic political reform (exerted, e.g., by means of the attachment of political conditions to international financial arrangements) was inappropriate (Kausikan 1993).

Or consider the question of the subordination of women in traditional Islamic doctrine, elements of which are carried over into some authoritative modern interpretations. There is, for example, no presumption of equal treatment or equal protection of law, no protection against forced marriage, and either required or permitted forms of gender discrimination (e.g.,

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[6] These agreements, as well as the Universal Declaration and the two covenants, are conveniently collected in Brownlie 1992.

mandatory veiling and sexual seclusion and segregation). To the extent that these elements are embodied in the public law and legally sanctioned practices of Islamic states, such as Iran and Pakistan (or for that matter Saudi Arabia), there is a clear conflict with the requirements of international human rights doctrine, and pressure to conform to these requirements will be regarded as partisan.[7]

Finally, there is the much discussed matter of female genital mutilation (FGM), still practiced ritualistically in Sahelian African on as many as two million girls, at or before puberty, each year. FGM, which can take several forms, is sustained by cultural acceptance rather than the force of law, so it does not obviously represent a case of a human rights violation by the state. Yet, where it occurs, FGM is not an aberration; it is entrenched in local cultures and permitted or required by local moral codes. And it is subject to intervention, if not by the state, then by nongovernmental agencies that claim to be acting to defend the human rights of the women affected. There is controversy about the seriousness of the harms brought about by FGM in comparison with various practices found in Western cultures, but whatever one's view about that, it would be hard to argue that interference to curtail FGM constitutes the application of a culturally neutral standard.[8]

In each of these cases it has been said that the local moralities that permit or require practices inconsistent with international human rights are sufficiently complex to allow for an internal critique of the offending practices.[9] This is true and important, but it does not diminish the impression that human rights operate in all three settings in a nonneutral way. Indeed, the existence of disagreements internal to a culture, combined with the fact that the weight of human rights seems usually to favor the modernizing, cosmopolitan side of the disagreement, only strengthens the view of international human rights as a partisan rather than a neutral concern. Jack Donnelly (1999, 84) has written that internationally recognized human rights "set out as a hegemonic political model something very much like the liberal democratic welfare state of western Europe." No doubt this overstates the case, at least insofar as it suggests there is an unambiguous "liberal" position about the full range of the subject matter of international human rights (there is, e.g., no single liberal view about self-determination or the rights of minority cultures). But Donnelly is correct that the declaration and covenants cannot really be regarded as setting forth a culturally or politically ecumenical or syncretistic doctrine.

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[7] See the analysis of contemporary sources of Islamic human rights law, including the 1981 Universal Islamic Declaration of Human Rights, in Mayer 1995, 95–6 and 117–8. On the extent of officially sanctioned human rights violations in the countries mentioned, see U.S. Department of State 1999.

[8] Welch 1995, 87–97. For the criticism that concern about FGM is ethnocentric, see the discussion by Tamir (1996) and the response by Kamm (1996).

[9] See Sen 1999, 231–46, on Asian values, and An-Na'im 1990, chap. 7, esp. pp. 175–7, on gender in Islamic law. In general, compare Perry 1998, 76–8.

NEUTRALITY AND PATERNALISM

The evident partisanship of international human rights doctrine has led some philosophers to suggest that we should distinguish between the full set of values recognized as human rights in international law and a restricted subset variously referred to as "basic rights" (Shue 1996)[10] or "human rights proper" (Rawls 1999a, 80, n. 23). For expository purposes I shall call the restricted subset—whatever its contents turn out to be—"genuine" human rights. The fact that the rights in the subset could be regarded as nonpartisan, or ideologically or culturally neutral, might be seen as qualifying them to play a special role in foreign policy for which international human rights generally are not suited.

Among those who believe there are grounds for restricting genuine human rights to some sort of nonparochial or neutral core, it is not always clear what these grounds are or why we should care about them. In this section and the next, I discuss these questions in connection with each of two distinct interpretations of neutrality or nonparochialism.

Consider first an approach suggested by some remarks of Michael Walzer (although he does not make the connection with human rights explicit), who distinguishes between "thin" and "thick" moralities. Walzer (1994, 9–10) speculates that a comparison of the moral codes found in various societies might produce "a set of standards to which all societies can be held— negative injunctions, most likely, rules against murder, deceit, torture, oppression, and tyranny." These standards would constitute "the moral minimum," not a complete moral code but, rather, "reiterated features of particular thick or maximal moralities." Someone influenced by such a distinction might regard human rights as part of the "minimum" or "thin" morality, nonparochial in that they are part of a core of requirements shared by all conventional or "thick" moralities—the common elements in a global moral pluralism. Thus, for example, R. J. Vincent (1986, 48–9) writes of a "core of basic rights that is common to all cultures despite their apparently divergent theories," which he describes as a "lowest common denominator."[11]

As Walzer's speculation suggests, this conception of nonparochialism, if treated as a constraint on what we should count as genuine human rights, would yield a relatively short list. Among others, rights requiring democratic political forms, religious toleration, legal equality for women, and free choice of a marriage partner would certainly be excluded. Other rights might be excluded if they were understood to generate certain kinds of duties; if, for example, the right to a high standard of physical and mental health were

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[10] Compare Miller 1995, 74–5. There is an interestingly different view in Buchanan 1999, 52–6 and 59–60.

[11] Similarly, Martin (1993, 75) believes human rights are principles that "would be regarded as reasonable by persons at different times or in different cultures. And such principles, again cross-culturally, would be thought to have connection . . . with a fairly wide range of differing conventional moralities."

thought to imply that society has an obligation to ensure the accessibility of health care for all, then the existence of disagreement about distributive responsibilities outside of families or local communities would presumably exclude this right as well.[12]

The narrowness of the resulting conception might encourage us to think that this interpretation of neutrality relies excessively on the metaphor of a "core" of rights common to the world's main conventional moralities. Perhaps this is too restrictive; after all, the idea of a right is itself culturally specific. So one might shift to a more elaborate conception that sees human rights as falling within an "overlapping consensus" of political moralities.[13] On such a view, nonparochial human rights would not necessarily be part of a common core in the sense of being recognized by all conventional moralities; instead, they would be rights that could be accepted by a reasonable person consistently with acceptance of any of the main conceptions of political and economic justice in the world. The idea here is that human rights should be the objects of a possible agreement among the world's political cultures; they are norms for the conduct of governments and international organizations that anyone who belongs to one of these cultures can accept without renouncing other important political principles.[14] Such a view would be narrow in comparison with the present international doctrine, but presumably it would be broader than the "common core": A value could count as a genuine human right even if it were not explicitly present in every culture, just in case members of each culture could reasonably accept it as consistent with their culture's moral conventions.

There are other forms of this basic idea, but rather than proliferate interpretations I shall turn instead to the question why we should care about a doctrine of human rights limited to either a common core or an overlapping consensus. In answering this question, we should remember that one function of human rights in international politics is to justify external interference in a society aimed at changing some aspect of the society's internal life. Such interference might aim, for example, to stop genocide or forceful political repression, to protect the innocent against civil violence when local authorities are unwilling or unable to do so, to restore a democratic government removed by force of arms, or to deliver humanitarian assistance to those imperiled by natural disaster or political collapse.

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[12] In Walzer's (1994, 28–31) view, distributive justice generally is part of thick but not thin morality; see his suggestive and interesting remarks on "the cure of souls and the cure of bodies in the medieval and modern West."

[13] The idea of an overlapping consensus is due to Rawls, but he does not use it in the analysis of human rights. See Nussbaum 1997, 286, and 1999, 37–9 and passim, for the application of this idea to human rights.

[14] I think this is consistent with Scanlon 1979, but it does not seem to be consistent with his view in *What We Owe to Each Other* (1998, 348). The position taken there allows judgments about the (un)reasonableness of culturally influenced beliefs about value to enter into bottom-line judgments about right and wrong; there is no guarantee that these judgments would satisfy the condition in the text. The latter seems to me to be closer to the truth.

I believe the reason many people aspire to a nonparochial or culturally neutral doctrine of human rights is connected to this interference-justifying role. Those who object to interference to protect human rights may claim that the interference is unjustifiably paternalistic. It would be paternalistic in that it limits liberty on the grounds that those whose liberty is limited (the "subjects") will be better off as a result of the interference, and it would be unjustified either because the subjects are capable of making choices for themselves or because the intervenor judges "better off" by standards the subjects have no reason to accept. A doctrine of human rights that satisfies a neutrality constraint might seem to offer the best prospect of meeting the antipaternalism objection because, if the human rights at stake are neutral in an appropriate way, then it can be replied that the aims of interference are ones that its subjects themselves would accept if they were in a position to bring their own moral beliefs to bear on the matter at hand.

The antipaternalism objection, as interpreted above, faces the following problem. When we are concerned about a violation of human rights in another society, we are usually not confronted with a situation in which people are unanimous in endorsing standards of conduct that justify the behavior of concern to us.[15] The picture of a "we" who believe in human rights and a "they" who do not is badly misleading. Among the "they" are oppressors and victims, and usually there is little reason to believe that the victims all share the values that the oppressors think justify their conduct. What this shows is that the perception of interference to defend human rights as a form of paternalism can be a misapprehension. Paternalism is an intervention in a person's self-regarding choices on the grounds that the intervention is good for that person. The individual whose liberty is interfered with is the same person as the one whose good the interference is intended to advance. In typical cases of interference based on human rights, however, some people's liberties are infringed in order to protect the human rights of others. The justification appropriately appeals not to paternalistic considerations but to the desirability of preventing a harm or securing a benefit for someone threatened by another agent's wrongful actions or omissions. (Although not always: Interference to persuade a young girl not to undergo an FGM procedure is genuinely paternalistic, but noncoercive interference—such as providing information and so respecting the girl's capacity for choice—affords a different defense.) That this should not be immediately obvious is evidence of the continuing grip of the analogy of person and state, which tempts us to treat the state as if it had the moral attributes of an individual rather than as an aggregate of separate persons with wills and interests of their own.

In most cases, then, what I have called the antipaternalism objection, if it pertains at all, must be interpreted elliptically. It must hold that, for purposes of

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[15] The point has often been noted. See, e.g., Nussbaum 1999, 10–2; Scanlon 1979, 88.

justifying external interference in a society, we should base our judgment of what constitutes harm or benefit to a member of that society on standards of value that belong to the conventional morality of the society, even if we have reason to believe that those on whose behalf the interference occurs would reject these values in their own cases. We might call this the principle of cultural deference.

In itself this is not necessarily a form of moral relativism, since it does not deny that sound cross-cultural moral judgments are possible. Nevertheless, taken as a general principle of practical reasoning, it is a strange, even a bizarre, view, for it allows the content of the doctrine of genuine human rights to be determined by the array of political moralities or conceptions of justice to be found in the world. Suppose a society with a racist political culture approves of the forced sterilization of a despised minority race as a means of population control. If we accept the principle of deference, we are forced to delete the right against genocide from the catalog of genuine human rights, because it is neither part of nor consistent with the racist conception. But surely we would resist doing so.

Someone might think that cases like that of a genocidal society are only theoretical possibilities, that no society would for long support such a horrible morality. Perhaps, over time, one expects a "normal distribution" of conventional moralities, each with a distinctive structure and content, but all converging on a substantial common core. This seems to me demonstrably too optimistic, but even if one regards the genocide case simply as a thought experiment, reflection about it suggests that the ground of our belief that, for example, genocide is a great wrong has to do not with the fact that other people agree it is so, but with the nature and consequences of genocide itself (compare Scanlon 1998, 337–8).[16]

Whether a standard should be accepted as a ground of action, and a fortiori as a ground of international action, does not turn on whether the standard is a part of, or implied by, existing conventional moralities. Actual agreement is too strong a condition to impose on any critical standard, and I believe it misrepresents the motivating idea of human rights. To say that human rights are "universal" is not to claim that they are necessarily either accepted by or acceptable to everyone, given their other political and ethical beliefs. Human rights are supposed to be universal in the sense that they apply to or may be claimed by everyone. To hold, also, that a substantive doctrine of human rights should be consistent with the moral beliefs and values found among the world's conventional moralities is to say something both more and different, and potentially subversive, of the doctrine's critical aims.

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[16] Brown (1999, 119) claims to the contrary that "there are no general moral standards that apply" to "Bosnian Serbs who kill Bosnian Muslims" or "Muslim extremists who think that the death penalty is an appropriate response to apostasy" because in each case the agents do not believe the conduct in question is wrong. This cannot be right. The Bosnian Serbs who killed innocent civilians were wrong to do so, whether they accept this or not.

DECENCY AND MINIMAL LEGITIMACY

I shall turn now to a different reason for limiting genuine human rights to a nonparochial core (and therefore a different idea of the way the core can be nonparochial). The basic idea is that we can distinguish between minimal and full legitimacy, with human rights serving as necessary conditions of minimal legitimacy. A minimally legitimate regime is one that merits respect as a cooperating member of international society, even if it falls short of being (what we would recognize as) fully legitimate or reasonably just.

Something like this distinction lies behind the conception of human rights found in Rawls's *The Law of Peoples* (1999a). It can be seen as an attempt to describe a view that is significantly nonparochial without being neutral in either of the senses I distinguished in the last section. As Rawls conceives of human rights, they are normative standards that would be satisfied by any "decent" regime, whether a liberal democracy or a (nonliberal, nondemocratic) "decent hierarchical society." For Rawls, "decency" is a term of art that serves to demarcate the boundaries of acceptable pluralism in international relations. Decent societies are those that liberal societies have reason to recognize as "equal participating members in good standing" of international society (the "Society of Peoples") (p. 59). Being so recognized, decent societies are entitled to a presumption against interference in their internal affairs; it would be wrong for foreign governments to intervene militarily, to attach political conditions to bilateral relationships and transactions, or to criticize. Rawls distinguishes decency from liberal justice: All liberal societies are decent, but not all decent societies are liberal. Human rights are common to all decent societies, whether they satisfy the requirements of liberal justice or not. So conceived, human rights "cannot be rejected as peculiarly liberal or special to the Western tradition. They are not parochial" (p. 65).

What should count as genuine human rights? Rawls believes that all decent societies would respect the rights of the person, the rights associated with the rule of law, freedom of religious belief and thought, freedom of expression (although perhaps not as extensive as justice requires in liberal societies), and certain economic (mainly subsistence) rights. Decent societies might, however, diverge beyond this area of overlap; specifically, they are not required to provide for equal freedom of public religious practice (but there must be sufficient liberty to allow the practice of minority religions "in peace and without fear" [Rawls 1999a, 74]), equal access to public office, or a right to democratic political participation. Therefore, the corresponding rights of the declaration—equal freedom of public religious practice as opposed to freedom of conscience and private religious practice, the right to vote in free and fair elections—do not count as "human rights proper"; they "seem more aptly described as stating liberal aspirations" or "appear to presuppose specific kinds of institutions" (p. 80, n. 23).[17]

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[17] Freedom of religion can be considered a human right "proper," in

Rawls's view has been criticized for being too tolerant of illiberal regimes (e.g., Buchanan 2000; Téson 1994). This may turn out to be correct, but there is a danger of overstatement: The scope of international toleration in Rawls's theory depends on the idea of "decency," which as he understands it is more restrictive than it may seem. A decent regime renounces aggressive war as an instrument of policy; follows a "common good conception of justice," in which everyone's interests are taken into account (although perhaps not on an equal basis); and respects certain basic rights, including subsistence rights, for all (so that, among other things, official discrimination against women is not permitted) (Rawls 1999a, 64–7). It is true that decency is compatible with a state religion and with undemocratic, but not nonparticipatory, political institutions: decency is, and is intended to be, a weaker requirement than liberal justice.[18] Even so, the constraints of decency are hardly undemanding and, taken seriously, probably would exclude many of the nondemocratic regimes in the world today and possibly some ostensibly democratic ones as well. In the end this may not be enough to meet the criticism, but it helps avoid a distorted picture of the theory.

It is important to see that, unlike the common core or overlapping consensus views, Rawls's view does not require the content of the human rights doctrine to be restricted by the array of political-moral conceptions in the world. The content is determined from the beginning by the normative idea of decency; human rights are said to be nonparochial in relation to all decent societies, not all societies simpliciter. This is why Rawls's view is not open to the objection that it deprives the human rights doctrine of its capacity to serve as a basis of social criticism. But there is a price to be paid. As Rawls (1999a, 80–1) observes, human rights must be considered as "binding on all peoples and societies, including outlaw states" that violate these rights. But because human rights are conceived so that they are necessarily common only to decent societies, it cannot be argued that interference to protect human rights in other societies would always be consistent with the conventional moralities of those societies.

Of course, much depends on the facts of the case, particularly on the relationship between the nature of a government and the content of its society's conventional morality. The case of a rogue tyranny oppressing a population that shares a decent political morality is different from the earlier example of a genocidal government in a racist society. But the possibility of variation does not affect the basic point that a doctrine such as Rawls's might justify interference in nondecent societies that could not easily be defended against complaints that it imposes alien values. Something more needs to be said to respond to such a complaint.

The response might have to do with the normative idea of decency itself, which serves to characterize the minimum requirements of legitimacy. Where does the force of this idea come from? The answer is not clear to me. The underlying thought is that a society should not have to satisfy liberal principles of justice in order to be regarded by other societies as legitimate; a society may be deficient by liberal standards yet still embody elements that distinguish it from a band of thieves who have achieved a modus vivendi. These elements include the rule of law, an acceptance that all persons have legal personality and the capacity to participate in public life, and a "common good idea of justice" that is shared, at least, by judges and other public officials. Such a society might be said to embody a form of reciprocity even if, from a liberal perspective, it is not the preferred form.[19] Unlike liberal societies, such a society might embody and promote a single, comprehensive view of the good life; but it would do so under conditions (including respect for "human rights proper") that render the society tolerable as a cooperating partner for liberal societies in the international order.

The question, however, is not whether a society that satisfies these criteria of decency is to be preferred to one that does not; so much is clear. At issue is whether, and if so why, decent but not just societies should be regarded as legitimate and, therefore, as qualified for treatment as "members in good standing" of the international order. Why—for the (limited) purposes of international political life—should decency be regarded as on a par with liberal justice?[20]

At one point Rawls (1999a, 67) writes that the definition of decency is simply stipulated for the purposes of the theory, and the reader must judge "whether a decent people ... is to be tolerated and accepted." But it is a serious question whether we have enough to go on intuitively to make such a judgment. Do we have a clear enough common-sense idea of decency, as a standard for institutions distinct from that of social justice, to judge other than arbitrarily? At another point he suggests that the content of the idea of decency is related to the function this idea plays in the conduct of liberal foreign policy. Liberal states should tolerate decent nonliberal states (which respect "human rights proper") because they are so structured and governed as to be peaceful, cooperating members of international society and therefore do not threaten international stability, whereas interference is permissible in "outlaw" states (which do not respect these

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Rawls's sense, only if its scope is interpreted more narrowly than what some believe to be the intent of Article 18 of the Universal Declaration. The human right to freedom of religion, as Rawls understands it, forbids the persecution of minority religions, but it allows for a state religion that enjoys various political privileges, such as public offices open only to its members (1999a, 65, n. 2), and the state religion may, "on some questions, be the ultimate authority within society and may control government policy on certain important matters" (p. 74).

[18] In the political sphere, for example, a decent regime need not be democratic, but it must provide regular opportunities for all citizens to communicate their views and preferences to those authorized to make political decisions. Rawls (1999a, 64) calls such an arrangement a "decent consultation hierarchy." The details are complex, and I pass over them here.

[19] I am grateful to Amy Gutmann for help in clarifying this thought.
[20] The restriction to international political life is important. Rawls need not (and does not) claim that decency and justice are "on a par" for any other purpose.

rights) because their internal features cause them to threaten international order (p. 81). As a practical matter this may be true, but it cannot give a plausible account of the basis of human rights, because it would locate the justification in the wrong place, not in the significance of human rights for the rights holders but in the beneficial consequences for international order of reducing the number of regimes that do not respect them.[21]

Rawls's most perspicuous argument for tolerating decent but illiberal regimes appeals to the consequences of toleration for these societies themselves. Decent societies, by definition, are open to internal, nonviolent change, and Rawls (1999a, 61–2) believes that the evolution of their institutions in a liberal direction is more likely if they are treated "with due respect" as equal members of international society. This is an empirical hypothesis about political development, and I suspect that some version of it is true in a significant range of cases (although I am not sure what would count as evidence for it). Yet, although the political development hypothesis bears clearly on the question of how we should act toward a society, it does not bear so obviously on the question of the ethical significance of a society's political decency or, derivatively, of the proper scope of a doctrine of human rights. Perhaps the connection, in Rawls's view, is that human rights should be understood as a class of moral consideration whose *only* role in political discourse is to justify coercive intervention in a society's affairs. If that is correct, then the fact that a value is not sufficient to justify coercive intervention counts against identifying the value as a human right.

But whether Rawls holds this view or not, there are two reasons not to accept it. First, it is not true that the only role of human rights in international discourse is to justify coercive intervention. As I observed at the beginning, human rights are also, for example, invoked to justify noncoercive interference by outsiders (governments, international agencies, nongovernmental organizations) and to justify programs of reform by compatriots. We should conceptualize human rights in a way that is adequate to this larger role. Second, as before, the argument against interference does not easily extend to an argument for limiting the scope of human rights. Granting the political development hypothesis grants nothing about the moral standing of the values expressed as human rights; the hypothesis is about the best means of realizing these values, not about their standing as values. Indeed, the best argument against reform intervention in a decent society, assuming that the hypothesis is correct, is that intervention is more likely to retard than encourage the society's movement from decency to (liberal) justice. But such an argument depends on rather than repudiates the claim that the liberal conception is an appropriate standard for the society in question.

What is the upshot for human rights? I believe it is this. If it were possible to regard a decent society as

minimally legitimate, in the sense of being, for purposes of its international relations, morally on a par with a liberally just society, then it would be possible to understand "human rights proper" as necessary conditions of minimal legitimacy. There would be a clear sense in which these human rights, as against the full catalog of internationally recognized human rights, could be defended as nonparochial. If the ethical significance of decency derives from that of liberal justice, however—for example, if its normative force depends on the hypothesis that decent societies, left to their own devices, are likely to develop into liberal ones—then the hypothesis might yield a reason not to interfere in decent societies, but there would be no deep distinction between "human rights proper" and other human rights that are part of liberal justice but not of decency. Indeed, it is hard to see any distinction of principle at all.

PURPOSES AND LIMITS OF INTERNATIONAL HUMAN RIGHTS

Notwithstanding these doubts about Rawls's interpretation of human rights, reflection about his view suggests two related precepts for any plausible conception. I shall try to formulate these precepts in a general way and then explain why they seem plausible.[22]

First, a satisfactory philosophical conception of human rights should be suited to the public role that we need human rights to play in international affairs. The doctrine of human rights is a political construction intended for certain political purposes and is to be understood against the background of a range of general assumptions about the character of the contemporary international environment.[23]

Second, the conception should interpret human rights as "common" in a special sense, not as the area of agreement among all existing political doctrines or comprehensive views, but as principles for international affairs that could be accepted by reasonable persons who hold conflicting reasonable conceptions of the good life.

Here are some points of clarification. First, to say that international human rights compose a doctrine adopted for certain political purposes is to reject some traditional views about the character of human rights, such as those that interpret human rights as a contemporary restatement of the (or a) theory of natural law or natural rights, or as a statement of a single comprehensive view about political justice or the political good that is supposed to apply to all human societies at all times and places.[24] Human rights are standards intended to play a regulative role for a range of actors in

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[21] I do not mean to say that Rawls himself gives such an account of human rights.

[22] Thomas Pogge's comments on an earlier draft helped me formulate these precepts.

[23] Jones (1996, 183–204) emphasizes the political character of Rawls's interpretation of human rights. Note, however, that Rawls (1999a, 81, n. 25) has reservations about this interpretation.

[24] For example, Finnis (1980, 198) believes human rights are "a contemporary idiom" for natural rights (see pp. 210–30 for his view of the content and limits of the doctrine of international human rights).

the political circumstances of the contemporary world. Yet, to describe human rights doctrine as a "political construction" is not to say that human rights are unrelated to these other kinds of views: In adhering to the doctrine or in criticizing it, one might be moved by beliefs about natural law or natural rights or by a comprehensive conception of the good. But it would be an error to identify these more fundamental moral beliefs with a political doctrine of human rights.

Second, according to these precepts, the particulars of the public political role expected of human rights are essential to a comprehension and defense of the doctrine. I shall say more about this role below. For now the essential point is that human rights are meant for certain political purposes, and we cannot think intelligently about their content and reach without taking account of these purposes. I do not mean to say that one should accept uncritically the conception of the political role of human rights prevailing in international affairs any more than one should accept the details of prevailing views of their content. But criticism must begin with some conception of the practice being examined, and the contours of this practice are to be found in the doctrine of human rights as we have it in contemporary international life.

Third, the second precept states that human rights should be acceptable to reasonable persons, not peoples. This is possibly in contrast to Rawls, who writes of peoples as corporate wholes with more or less widely shared conventional moralities. I have discussed my doubts about this elsewhere (Beitz 2000) and here simply call attention to the possible contrast and note its importance in thinking about the content of the human rights doctrine. In my view, human rights are ultimately justified by considerations about the reasonable interests of individuals, not those of whole societies conceived as corporate entities.

A view of this kind is at odds with some traditional conceptions in distinguishing between human rights as a political doctrine and various underlying views about social justice. Why should we accept the revisionist view? Part of the answer is that, as a historical matter, international human rights doctrine is not accurately interpreted as an effort to fill the same conceptual space as was filled by natural law or natural rights in the Western political tradition. Those ideas aimed to supply something different—a comprehensive conception of the good or just society, perhaps, or an account of the constraints a government should observe in the use of its monopoly of political power. By contrast, the international doctrine is a negotiated agreement (or set of agreements) that describes "a common standard of achievement for all peoples and all nations" (Universal Declaration, Preamble), and it is meant to provide guidance in the conduct of international political life by actors such as international organizations and their member states, nongovernmental organizations, and individuals.

But the argument need not rest on a historical observation. Contemporary international society needs a doctrine of the kind imagined by the framers of the Universal Declaration. One reason, which Rawls no-

tices, arises from the developing international capacity and disposition to intervene coercively in the affairs of states to protect the interests of their own people. Standards are needed to guide the use of this coercive power. As I have been urging, however, human rights doctrine serves other purposes as well. The most general, albeit awkward, statement of these purposes might be this. The global political structure contains an array of institutions and practices, including the foreign policies of states, with the capacity to influence the conditions of life for individuals in their domestic societies. In some cases this influence comes about through intentional action, such as military intervention or the attachment of political conditions to development aid. In other cases it occurs through the normal operation of an institution, such as structural assistance provided by international financial bodies. Moreover, as I have been emphasizing, transnational action that affects human rights is not limited to the operations of governments and international organizations; it may also be carried out by nongovernmental organizations, acting in international fora or within the internal political processes of individual societies. The doctrine of human rights is a statement of standards to guide the structures and conduct of global political life insofar as these bear on the conditions of life for individuals in their societies.

To be more specific, a doctrine of human rights suited for contemporary international practice should be capable of playing at least three kinds of roles. First, it constrains the domestic constitutions of states and the fundamental rules of international organizations and regimes. (Whether this constraint should operate by means of the embodiment of these norms in constitutions, organizational charters, and so forth, I take to be another question, one not settled by theoretical considerations.) Second, it describes goals for social development applicable to all contemporary societies, to the extent that they are or can be influenced by such external forces as the foreign policies of other states and the practices of international institutions. (The degree and kinds of influence appropriate in particular cases is again another question, involving both normative and pragmatic considerations.) Third (and derivatively), the doctrine furnishes grounds of political criticism to which it would be appropriate to appeal in the setting of global politics by a range of international and transnational actors—not only governments but also officials of international institutions and nongovernmental organizations acting in their capacity as citizens of global society.

On this view the doctrine of human rights is significantly teleological. It is a statement of aspiration applicable to all contemporary societies, but all of its requirements may not be capable of being satisfied simultaneously or in the short run. Human rights may not bear on political choice as straightforwardly as they would if conceived in more traditional terms as side constraints or prohibitions. The actions required to satisfy a human right will depend on the case. This is not only because achieving a given end may require different strategies in different settings, but also be-

cause priorities will have to be set and compromises reached when, in the short term, the effort to secure one right threatens to block efforts to secure another. Joel Feinberg (1973, 95) observed long ago that some rights of the declarations seem to be more accurately conceived as rights "in an unusual new 'manifesto sense'" than on the model of legal claim-rights.[25] The view I sketch here is compatible with this observation.

A CASE STUDY: POLITICAL RIGHTS

According to the formula I suggest, the doctrine of human rights is "common" in the sense that, considered in light of the political purposes it is expected to serve, reasonable persons could accept it despite differences in their reasonable conceptions of the good. Because this formulation depends from the outset on judgments about which conceptions to count as reasonable, its effect is to frame the question of the justification of human rights as a substantive problem of political theory, comparable to problems such as the justification of principles of social justice for domestic society. What distinguishes the problem about human rights from the others is the special character of the international political environment in which these standards must operate. Concerns regarding cultural parochialism or political bias would arise, if at all, within the substantive argument for each of its elements.

To illustrate, let me consider whether the doctrine of human rights should recognize a right to democratic institutions. The Covenant on Civil and Political Rights is unequivocal. It holds that there are human rights to political institutions that afford every citizen an opportunity to participate in public affairs either directly or through "freely chosen representatives"; to compete for public office and to vote in "genuine periodic elections"; and to assemble peaceably without restrictions "other than those . . . which are necessary in a democratic society in the interests of national security or public safety" (Arts. 21, 25). As a purely descriptive matter, there is no question that these requirements are nonneutral in the sense that they are not endorsed by all the major political moralities in the world. What is the ethical significance of this fact? Does it mean that we should not regard democratic rights as genuine human rights, or that we should not accept the defense or promotion of democratic rights as a justification for interference in a nondemocratic society's domestic life?

These questions are worth special attention because the element of international human rights doctrine most often said to be objectionably parochial is that concerned with democratic rights. At the same time, there is a discernible trend in international law toward recognition of a universal right to democratic institutions (Franck 1995, chap. 4). So these questions mark a

specific point of tension between the restricted conception of human rights prevalent in philosophical thought and the development of international law and practice.

Consider a hypothetical case. Imagine an authoritarian regime in a society in which historically the predominant political beliefs are not democratic. Citing the Covenant on Civil and Political Rights, a modernizing insurgency fighting for democratic reforms calls upon the international community for military and financial help. Given the society's cultural history, international interference, if successful, would produce a result that would be regarded as a change for the worse by a significant portion, perhaps even a majority, of the society. The question is whether this fact argues against interference to help the reformers and, if so, for what reason.

One possible reply returns to the issue of paternalism. Interference in this kind of case, perhaps in contrast to most interferences to defend human rights, would be genuinely paternalistic: It involves coercive interference in some people's liberty on the grounds that the results would be in their own interests. But it would not be justified paternalism. Normally, the justification of a paternalistic choice has at least three elements: (1) a claim that the subject is unable to choose rationally for himself owing to a failure of reason or will; (2) evidence that the choice is guided by knowledge of the subject's own interests, to the extent they can be known, or by a reasonable conception of the interests it would be rational for the subject to have; and (3) a reasonable expectation that the subject will come to agree that the agent's choices on his behalf are the best that could be made under the circumstances.[26] In my example, because a significant portion or even a majority of the population does not share democratic political values, for this portion of the population the second element (and possibly the third) of the justification would fail. The interference does not appear to take seriously the moral beliefs of those whom it coerces.

This reply seems to me to yield the most plausible account of the ethical significance of the fact that many in our hypothetical society hold moral beliefs inconsistent with democracy. Yet, it is open to certain doubts. First, it may be questioned whether what I describe as people's "moral beliefs" accurately identify their political interests. This is primarily an empirical issue, and I have not worked out the case in enough detail to resolve it one way or the other. One would want to know, for example, about the nature of the evidence that many people reject democratic values, whether the society has any past experience with democratic forms, and whether there have been occasions for public political deliberation about forms of government.

On one set of assumptions, the very fact that political institutions lack the features characteristic of democracy—such as free expression, political competition, voting—would suggest that preferences about political forms are not either fully informed or freely arrived at.

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[25] Feinberg's use of "manifesto sense" is not, as some writers have thought, derisory; he endorses and expresses sympathy for this usage. It is also worth noting that one can accept the idea that some human rights are "manifesto rights" without also accepting Feinberg's view that it is not possible to assign corresponding duties to them. That, I believe, is a mistake.

[26] I rely here on the discussion in *A Theory of Justice* (Rawls 1999b, 218–20).

In that case the justification of paternalistic interference must fall back on a judgment about what it would be rational for people to want if they were in possession of full information and able to reason freely, and here we have no choice but to engage the substantive question of the value of democracy. Telescoping a long argument, suppose there is reason to believe that democratic institutions are instrumental to the enjoyment of certain (nonpolitical) human rights, including the rights of the person and subsistence rights. Then, assuming these other rights are not themselves culturally controversial, there is an argument that it would be rational to want democratic rights as means of ensuring the satisfaction of urgent human interests, whatever the present political values in a culture.[27] (There is also the counterargument that some other configurations of political institutions, like Rawls's "decent consultation hierarchy," would be equally effective in securing human rights. Which is correct depends on a historical and political judgment, not an ethical one.)

Second, the reply considers only the perspective of the nondemocratic portion of the population. What about the democratic insurgents who asked for outside help? Again, one needs more information, but presumably the insurgency has local causes and responds to local grievances and aspirations. From the perspective of this group, interference is not a matter of paternalism at all but of avoiding or reducing harm or protecting against injustice. It is hard to see how this issue can be addressed other than by examining the urgency of the interests at stake in relation to the costs of interference and its probability of success. Once again, it seems that the justifiability of interference to support the democratic reformers should be faced as a freestanding issue in political ethics in which the values that interference may achieve are compared with the costs and risks of making the attempt. There is no categorical conclusion possible about the sufficiency of democratic reform as a justification of intervention in a case like this.

These reflections suggest an alternative explanation of the ethical significance of local disagreement over political values. It may be that this bears on the feasibility of constructive interference, or on its prospects of success in the long run, rather than on the nature or scope of human rights themselves. If a significant portion of the population lacks democratic sympathies, then it is not likely that democratic institutions will be sustained even if a democratic insurgency attains its immediate objectives. In that case it could be true both that there is a human right to democratic institutions and that interference in support of a prodemocratic insurgency would be wrong.

Why, then, should we say there is a right at all? If the acceptability of interference to promote democratic institutions effectively depends on the extent of democratic commitment within a culture, have we not conceded that there is no universal (and hence no human) right to democracy? The answer is that we have not. The question trades on the idea that there can be no right without a remedy, or no right without some feasible strategy for its realization. But the fact that intervention is unlikely to succeed in establishing democratic institutions in a divided political culture does not imply that nothing ever will; institutional change is a complex historical process, usually accompanied by changes in political belief as well. Moreover, a human right to democracy may have practical force otherwise than by licensing coercive intervention. For example, it might call for efforts at persuasion and education or support for the development of elements of a democratic social infrastructure (associations, labor unions, and so on). Of course, to accept this as a reply to the objection, one must accept a conception of a human right as something different from a legal right or certain moral rights; for example, although it may generate duties for various agents, a human right cannot always be a ground for insisting on immediate compliance. But if human rights are regarded as political constructions in the way I have described, this is unremarkable.

CONCLUSION

The discourse on international human rights suffers from a strange juxtaposition. In major arenas of international politics concerns about human rights are more prominently expressed than ever before, and there is some reason to believe that these concerns increasingly motivate action. Yet, within contemporary political thought human rights are often regarded with suspicion. These suspicions are diverse. Some people think there is no such thing as *universal* human rights (i.e., rights that may be claimed by anyone). Some think there is no such thing as universal *human* rights (possessed by human beings independently of their relationships with others and their institutional memberships). Some think that "internationally recognized human rights" are not *rights* (at least not in any sense that would be familiar to someone influenced by Hohfeld). Some think the international doctrine of human rights is a good idea corrupted by overextension: Although there may be such a thing as a universal human right, some (perhaps many) of the rights specifically enumerated in the international instruments fail to qualify. And some believe the doctrine of human rights is a cloak for liberal political values, an instance of partisanship rather than a neutral basis for global agreement.

I have only addressed the last of these suspicions directly, although I have adverted to some of the others. I have observed that the doctrine of human rights, regarded for the moment as part of the positive law of international society, cannot plausibly be considered culturally or politically nonpartisan. And I have argued that this fact, in itself, does not count against the doctrine. What is distinctive about human rights as a category of normative standard is not their suppos-

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[27] The argument is made in Shue 1996, 74–8, and Sen 1999, 178–86. I made a similar argument about the value of democracy, concentrating on the circumstances of developing societies, in Beitz 1981, 177–208.

edly symmetrical relationship to the conceptions of political justice or legitimacy to be found in the world's cultures but, rather, the role they play in international relations. Human rights state conditions for political and social institutions, the systematic violation of which may justify efforts to bring about reform by agents external to the society in which the violation occurs. This interference-justifying role may limit the content of the doctrine, but there is no reason to suppose the limitations will yield a neutral or nonpartisan view. Indeed, it is hard to see how things could be otherwise. In the words of the Vienna Declaration, human rights specify conditions that institutions should satisfy in order to respect "the dignity and worth inherent in the human person" (Preamble); but the concept of a person with inherent dignity and worth is a substantive moral idea and will almost certainly be more congenial to some than to other conceptions of justice or political good.

Is this kind of partisanship problematic for the doctrine? I believe not, provided that each of its elements can be defended by an appropriately general argument, as I suggest is possible for the right to democratic institutions. Such a defense would hold that human rights are "common" in a morally significant way without being, so to speak, empirically nonparochial. This, of course, is not to say that cultural and political differences do not come into deliberation about how to act. These differences may enter in a variety of ways—for example, as factors determining the feasibility and cost of a contemplated interference or the risks of collateral harm. They may also enter at a more basic level, as factors influencing a judgment about the rightness of using coercive means of interference, particularly when the purpose of the interference is genuinely paternalistic.

This conception of human rights faces a variety of objections. Here I note three of the most prominent and simply gesture at the kind of reply that might be offered to each. First, it may seem excessively pragmatic to regard human rights as a "political conception." Whatever else they are, human rights are surely moral standards, standards whose authority rests on recognizably moral considerations. To suggest otherwise, the objection holds, fails to take seriously both the character and the history of the idea of human rights. I believe, however, that the objection starts from a faulty premise. To say that the doctrine of human rights is a political conception is not to deny that its authority rests on moral considerations; human rights are political, not in the source of their authority, but in their role in public ethical life. As I have described them, human rights are standards to which it is reasonable to hold political institutions accountable in the processes of contemporary world politics. They operate as prima facie justifications of transnational (although not only transnational) political action aimed at bringing about change in the structure and operation of domestic (and international) institutions. Any account of the authority of human rights must take note of the political contexts in which they operate,

but this hardly means that the account would exclude moral considerations; in fact, it would depend upon them.

Second, in some ways a contrasting objection is that a partisan conception of human rights is insufficiently realistic. According to this objection, unless a doctrine of human rights is culturally neutral it cannot possibly play the role that we need a doctrine of human rights to play in international affairs. The reason is that if a violation of human rights is not regarded as a shared basis for political action, then the capacity to enlist international support when it is most needed will deteriorate, and the doctrine of human rights will become little more than a sectarian hope. The latter proposition seems true enough, but the point about neutrality does not obviously follow from it. It is an empirical question whether a political doctrine must be neutral in order to enlist enough international support to be influential, a claim that not only has not been proved but also is most likely false. The growth of the global human rights regime itself may be evidence to the contrary.

Third, there is a residual worry that an expansive doctrine of human rights can too easily be used as an instrument of neocolonial domination, as a way to rationalize the use of coercion by a hegemonic power to advance its own interests. Of course, there is one sense in which this is a legitimate worry if not almost a necessary truth. If an expansive doctrine of human rights embraces liberal political values, and if the hegemonic power identifies its interests with the advance of these values, then coercion that is soundly justified by human rights considerations also will advance the interests of the hegemonic power. What troubles people, however, seems to be not this kind of case but one in which human rights considerations are abused or distorted in order to make self-interested political action seem to be justified by other-regarding considerations. The fear is that an expansive doctrine will be more open to this sort of abuse than a minimalist one.

This is not an abstract fear. The history of intervention (e.g., by the United States in Central America) includes many instances of what plausibly can be seen as analogous abuses of the values of self-government and individual liberty as rationales for self-interested interference. Let us therefore concede the hypothesis that an expansive doctrine is more open to abuse by a hegemonic power than a more narrowly drawn conception. What follows? Since we are conceiving of human rights as a public, political doctrine, it cannot be replied that the possibility of abuse is irrelevant to the content of the doctrine. If this possibility were significant, and if unilateral intervention were the only mechanism realistically available to promote human rights, then a narrowing of the doctrine's content might be appropriate. But there is an alternative: It is to establish multilateral institutions to protect human rights doctrine from unilateral abuse. This is one source of the argument for a world human rights court, and it may

also argue for something like the complex voting system found in the Security Council, which otherwise might be seen as objectionably constrained by its supermajority requirements and the great-power veto. If such mechanisms can be made to work, the potential for neocolonial abuse of human rights doctrine will not by itself argue for a limitation of the doctrine's substantive scope.

All of this only gestures at how one might defend a more robust theory of human rights than that presumed by those who regard neutrality as a virtue, and one more in keeping with the doctrine of human rights found in contemporary international practice. Plainly, more needs to be said to develop such a defense and to explore the objections that could be brought against it. And plainly, that would be beyond the scope of a single article.

So I will conclude with an observation on a different although related point. The question of whether an expansive conception of international human rights can be defended is different from the question of whether we ought to accord human rights a fundamental place in international political theory. As a central element in international practice, human rights are well established, and political theorists should strive for a critical understanding that takes seriously their practical role. But there are good reasons to resist thinking of human rights as the fundamental terms of international political theory. For example, rights are rarely self-evident and usually stand in need of justification, and the justification seldom terminates in another assertion of right. And because the satisfaction of a right typically imposes costs on others, we need a mechanism for assigning responsibility for bearing those costs. In itself, however, the concept of a human right is not much help in designing such a mechanism because it is concerned with the interests of the beneficiary of the right rather than with the relationship in which the right is satisfied. For both reasons it seems likely that a satisfactory theory of human rights is better conceived as an aspect of a more general theory of global justice.

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Interpreting Berlin's Liberalism
JONATHAN RILEY *Tulane University*

I argue that Isaiah Berlin's pluralistic liberalism is best interpreted as a sophisticated form of liberal rationalism, as Berlin himself suggests. His value pluralism, even if it is viewed (as his critics typically view it, with considerable justification) as claiming that any choice between conflicting incommensurable values cannot be a rational choice, does not subvert his liberalism. Rather, this agonistic pluralism emanates from his liberal rationalism, which pictures reason as too weak to resolve conflicts of incommensurables. Yet, reason remains strong enough to discover that certain basic liberal values, including those associated with some minimum core of equal rights, are far more important than any competing values created by mankind. Berlin apparently sees his pluralistic liberal rationalism as a genuine rationalism that, in stark contrast to mainstream utopian rationalisms which wildly exaggerate the power of reason, makes suitable room for the valid insights provided by the romantics.

Sir Isaiah Berlin's (1969, 1991, 1999) work has recently been interpreted as a muddled and half-hearted version of liberalism (e.g., Crowder 1994; Gray 1995, 1998; Ignatieff 1999; Kateb 1999). The main problem, according to the critics, is his "agonistic" brand of value pluralism, which holds that plural basic values (and constellations of values, or cultures) are not only incompatible but also incommensurable in the sense that they cannot be rationally compared or ranked in cases of conflict. Such pluralism undermines the possibility of liberal rationalism, given that liberal and nonliberal values are incommensurable in the sense described. As Gray puts it, "Berlin's agonistic liberalism—his liberalism of conflict among inherently rivalrous goods—grounds itself on the radical choices we must make among incommensurables, *not upon rational choice*" (1995, 8, emphasis added). "Radical choice" is "ungoverned by reason," "without criteria, grounds, or principles," and is at "the heart of Berlin's liberalism" (pp. 23, 61). Morgenbesser and Lieberson (1991, 3–7) confirm that Berlin gives many examples of this "radical kind of choice" arising "as part of the normal human situation." As they also point out, he credits Machiavelli for implicitly suggesting that " 'ends equally ultimate, equally sacred, may contradict each other, that entire systems of value may come into collision *without possibility of rational arbitration*' " (p. 6, emphasis added; quoting Berlin 1981, 74).

If the critics are right, Berlin's commitment to liberalism must ultimately be lacking in rational conviction. Perhaps Berlin admires liberal and nonliberal cultures alike as if they were artworks, for example, objects whose peculiar beauties not only cannot be compared but also cannot be reduced to, or judged in

terms of, common standards of truth and morality. Kateb suggests that Berlin is in the grip of such an aestheticism, so much so that he typically ignores or rationalizes the potentially grave moral costs (such as violations of fundamental rights) that flow from his aesthetic outlook: "Berlin appears to me to be, despite his liberalism, unaware that human dignity involves . . . the status of each individual person, a status that only the recognition of individual rights can fully protect" (Kateb 1999, 1037). By implication, Berlin fails to recognize that his aestheticism (if that is what it is) countenances the destruction of his liberalism. He might point to the admiration he feels for liberal social institutions and practices to explain why he picks them when nonliberal cultures are available. But no appeal to practical reason can justify his aesthetic choice: Nonliberal cultures cannot rationally be ranked as inferior to liberal ones within the aesthetic realm. There is nothing in reason to prevent the aesthetic pick of a nonliberal culture.

I shall argue against the critics that Berlin's work is best interpreted as an unusual but coherent version of liberal rationalism. In making the argument, I shall assume that the critics are correct to attribute to him the doctrine of agonistic or tragic pluralism, with its idea of incommensurability as incomparability. This is not to deny that a more benign brand of pluralism might be attributed to him. Unlike agonistic pluralism, benign pluralism views incommensurability as allowing rational comparisons of conflicting values (see, e.g., Barry 1990, 3–8; Chang 1998; Crowder 1998a, 1998b; Galston 1999, 771; Hurka 1996; Larmore 1996, 157; Stocker 1990; Williams 1979). Berlin does appear to waiver between agonistic and benign pluralisms. At times he suggests that incommensurable values can be rationally compared, with the caveat that such comparisons do not involve either reduction of the values to a common denominator, such as welfare, or the mechanical application of a single scheme of lexical priorities valid for all times and places (Berlin 1983; Berlin and Williams 1994, 307; Jahanbegloo 1992, 142–3). Nevertheless, he often endorses agonistic pluralism as well, as the critics claim.[1]

Jonathan Riley is Professor in the Department of Philosophy and the Murphy Institute of Political Economy, Tulane University, New Orleans, LA 70118 (jonriley@tulane.edu). During the academic year 2000–01, he is Visiting Rockefeller Fellow, University Center for Human Values, Princeton University, Princeton, NJ 08544–1006 (jriley@princeton.edu).

[1] Lukes (1994, 706) distinguishes between incommensurability and

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Moreover, it is agonistic pluralism, not benign, that captures the antirationalism Berlin associates with the romantic reaction to the Enlightenment. As depicted by him, the most ardent romantics insist that individuals and nations create their own ideals in an endless process that transcends rational assessment. The creative will, or spirit, or imagination, or passion is not something that can be caged or ordered by reason. Ultimately a mystery, it is the stuff of religion and myth (Berlin 1999, 99–109, 121–2). Thus, since rational standards are lacking, the ideals created by one individual or nation cannot be judged to be more or less reasonable than those chosen by another. Rather, men ought to defend their ideals at all costs, as authentic manifestations of their peculiar creative drives. True, when different ideals clash as they inevitably will, the human consequences may be tragic since the conflict cannot be rationally resolved. But the pure romantic accepts this and assigns "the highest importance" to "integrity, sincerity, readiness to sacrifice one's life to some inner light, . . . fighting for your beliefs to the last breath . . . , martyrdom as such, . . . [and] dedication to [your] ideal . . . no matter what it was" (pp. 8–14, 139–41). Berlin makes clear that he regards the romantic tide of antirationalism as a valid corrective to mainstream Enlightenment rationalism, which he repeatedly attacks as incoherent and (despite appearances to the contrary) dangerous to liberty. The romantic movement "attacked and gravely damaged . . . the old proposition that virtue is knowledge," he states, and thereby dispelled the mainstream illusion that the art of life is "a jigsaw puzzle" to be fit together by reason (pp. 118–9).

I shall read Berlin as an agonistic pluralist not only because such a reading engages his critics on their own terms and better reflects the antirationalistic spirit of the romantics but also because it makes more difficult my project of reconstructing his thought as a version of liberal rationalism.[2] Indeed, it may seem inconceivable that he can be interpreted as any kind of rationalist, given his strictures against mainstream rationalism and his support for romanticism. Nevertheless, he explicitly encourages such an interpretation: "Fundamentally, I am a liberal rationalist. If you believe in liberal principles and rational analysis, as I do, then you must take account of what the objections are, and where the cracks in your structures are, where your side went wrong" (Jahanbegloo 1992, 70–1). Taking Berlin at his word, I shall argue that his work is best seen as a liberal

rationalism with "cracks" or imperfections built into the power of practical reason to resolve ethical and political conflicts (including conflicts of equal rights).

Berlin's agonistic liberalism has foundations in rational choice. Such a liberalism involves subsets of possible outcomes about which it is rational to believe that reason is powerless to determine our ethical and political choices. Within these subsets or zones of rational indeterminacy, agents are morally free to choose as they wish because conflicts of incommensurable values cannot be rationally resolved: There is no rational justification for choosing any one option over the others, all things considered. But the boundaries of the zones are defined such that the freedom to choose does not extend to illiberal choices. A minimum core of human rights must not be violated, even though other liberal rights (distinct from the core rights) may conflict with one another in ways that implicate plural and incomparable values.

It deserves emphasis that when Berlin refers to himself as "a liberal rationalist," and when I attribute to him a species of rationalistic liberalism, no endorsement is implied of the Western rationalistic tradition. In his view, mainstream rationalism, exemplified by Socrates as represented in Plato's dialogues; by enlightenment philosophers such as Voltaire, Helvetius, and Condorcet; by (to a large extent) Kant, Schiller, and Goethe; by utilitarians such as Bentham, James Mill, and J. S. Mill; by Marx and Marxists; and by Hegel and his Anglo-American followers, including Green, Bosanquet, and perhaps even Dewey, is committed to fake social utopias of various descriptions, the pursuit of which inevitably leads to state oppression (e.g., Berlin 1969, xxxvii–lxiii, 167–72; 1991, 1–48; 1999, 21–34). The fundamental mainstream error, he thinks, is the proposition that "virtue is knowledge," more specifically, that an all-powerful Reason can discover how to resolve any and all ethical and political conflicts, whether by maximizing some single ultimate value, such as happiness, or by adjusting plural ultimate values into an ideal pattern of perfect harmony. This is the great error exposed by the romantics.

But Berlin's clear rejection of mainstream rationalism as a false and dangerous ideology does not imply that he is hostile to all conceivable forms of rationalism, including those which combine their commitment to rational choice with a belief that reason is incapable in principle of resolving all ethical and political conflicts. Rather, his point seems to be that mainstream rationalism, by exaggerating the power of reason to classify and arrange the moral universe, distorts and obscures a genuine rationalism, which recognizes the true limits of reason and makes room for agonizing moral choices that lack rational justification. Indeed, I suggest that Berlin's depiction of himself as a liberal rationalist is far from careless, and it may be intended to encourage us to investigate more closely the distance between aggressive mainstream rationalism (which he sees as false and illiberal) and a more humble and imperfect rationalism (which he sees as genuine and liberal).

Admittedly, Berlin could not consistently call himself

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incomparability but argues that both ideas are among the different components of Berlin's pluralism. He may be right that Berlin's pluralism is an unsystematic collection of ideas that "seem to be separable and not to entail each other" (p. 708).

[2] For similar reasons, I shall not read Berlin as endorsing what may be termed partial commensurability, that is, the view that competing values can invariably be reduced to a common scale but only imperfectly so. A unit of justice might correspond to some range of welfare units, for example, whereas a unit of mercy corresponds to a different but overlapping range of welfare units. Some comparisons of justice and mercy in cases of conflict might then be rationally determinate in terms of welfare. Whatever its attractions, partial commensurability represents a concession to monism in which Berlin shows little if any interest.

a liberal rationalist if he fully endorsed romanticism. Unrestrained antirationalism implies that nothing is worth salvaging from mainstream rationalism. Virtue is not knowledge but creative self-expression and a willingness to die for one's ideals no matter how foolish they appear. The good life is not a jigsaw puzzle that can eventually be grasped and understood by rational people. It is an endless striving to express one's own true and perfect nature, endless because that creative nature is literally inexpressible and beyond the power of reason to articulate. As such, the good life cannot ever be pinned down by rational methods. Rather, human life is necessarily tragic, replete with romantic "nostalgia" if not "paranoia" (Berlin 1999, 104–9).

But Berlin makes clear that he does not endorse "unbridled" romanticism. He points out that some common values must exist for people to understand and communicate with one another at all. "To the extent to which there are common values, it is impossible to say that everything must be created by me; that if I find something given, I must smash it; that if I find something structured, I must destroy it in order to give free play to my unbridled imagination. To this extent romanticism, if it is driven to its logical conclusion, does end in some kind of lunacy" (p. 145). In short, romanticism and its focus on creative self-expression must be restrained by some common ethical and political norms that are accepted as given. A common moral horizon properly limits the range of plural and incommensurable ideals that may be chosen spontaneously by individuals or groups. Berlin thinks this common horizon is something that can be discovered by rational methods. Its unusual structure is the focus of a genuine liberal rationalism.

For Berlin, then, the "cracks" in mainstream rationalism on which romanticism throws light are fatal to the mainstream image but not fatal to rationalism altogether: "This great structure was not overthrown, but it was cracked, as it were, by the romantics" (Jahanbegloo 1992, 159). Mainstream rationalism must be given up in favor of a more complex liberal rationalism that makes room to a limited extent for a tragic pluralist conception of moral life. All hope of a rational social utopia must be abandoned since conflicts of values cannot always (or perhaps even often) be rationally resolved. At the same time, however, the more complex doctrine must include liberal limits on the pluralist conception that are rationally justified. In other words, the scope of pluralism must be limited by a common moral horizon such that the elements of the horizon are reasonably held to take precedence when they conflict with any other ideals and values created by men. Thus, a belief in reason—rational choice as opposed to radical choice in Gray's sense—is at the foundation of Berlin's liberalism.

RECONSTRUCTING BERLIN'S DOCTRINE

Making Room for Romanticism

Berlin (1999, 146) argues that the romantics left a lasting legacy: "The notion that there are many values,

and that they are incompatible; the whole notion of plurality, of inexhaustibility, of the imperfection of all human answers and arrangements; the notion that no single answer which claims to be perfect and true, whether in art or in life, can in principle be perfect and true—all this we owe to the romantics." The romantics, it seems, made explicit what had only been implicit in Machiavelli's writings (Jahanbegloo 1992, 44–5, 53–61). An agonistic value pluralism, which holds that the plural ideals created by men can conflict in ways that cannot be rationally resolved, is true to our moral situation, Berlin insists. Thus, any idea of a rational social utopia, in which conflicting values have been reasonably adjusted or balanced to produce complete ethical and political harmony, must be dismissed as false.

These romantic insights into pluralism and the imperfection of reason are apparently confirmed by genuinely rational methods of ethical and political inquiry. Berlin (in Jahanbegloo 1992, 39) certainly does not deny that such rational methods exist: "Rational methods, roads to the truth, ... are, as Socrates taught, of cardinal importance to the fate of individuals and societies: about that the central traditions of Western philosophy are right." But he argues that the genuine methods are not those employed by mainstream rationalistic ideologues themselves, who have an unwarranted a priori intuition that omnipotent Reason can settle any and all conflicts of values. "I never said that I didn't believe in 'reason.' But I simply don't understand what some philosophers mean by reason, which is for them a kind of magical eye, which sees nonempirical universal truths" (p. 113). In his view, mainstream rationalism is truly a species of irrationalism "enunciated by over-rational and over-scientific analysts" (Berlin 1999, 146).

Berlin emphasizes that mainstream rationalism involves a false a priori intuition that a rational social utopia is possible. "Since some values may conflict intrinsically, the very notion that a pattern must in principle be discoverable in which they are all rendered harmonious is founded on a false a priori view of what the world is like" (Berlin 1969, li). Even great liberal philosophers have been under this mainstream hyperrationalistic illusion, he alleges, including Mill (p. li) and Kant (Jahanbegloo 1992, 109). Against the mainstream, Berlin argues that genuine rational methods tied to empirical observation confirm the validity of tragic pluralism and thus the impossibility of finding a best option in which all truly valuable things find their rational place:

If we are not armed with an a priori guarantee of the proposition that a total harmony of true values is somewhere to be found—perhaps in some ideal realm the characteristics of which we can, in our finite state, not so much as conceive—we must fall back on the ordinary resources of empirical observation and ordinary human knowledge. And these certainly give us no warrant for supposing (or even understanding what would be meant by saying) that all good things, or all bad things for that matter, are reconcilable with each other. The world that we encounter in ordinary experience is one in which we are

faced with choices between ends equally ultimate, and claims equally absolute, the realization of some of which must inevitably involve the sacrifice of others (Berlin 1969, p. 168).

Such a pluralism must be distinguished from relativism and subjectivism about values (Galston 1999, 770, 773–4; Gray 1995, 46–7; Newey 1998, 494). It claims that certain propositions are true about the nature of moral value. According to one proposition, some limited field of plural and incommensurable values is part of a common moral horizon that can be identified and understood by any reasonable human being, whatever his social context. Indeed, it is because these different values and ways of life all belong to the common horizon that "we can communicate with people whose forms of life may be different from ours—living in different conditions at different times" (Jahanbegloo 1992, 108). A second proposition holds that these incomparable values and forms of life cannot be rationally harmonized to form a best option when they come into conflict.

Like the romantics, Berlin seems convinced that some values naturally belong with others in that they are observed to evolve together in particular clusters or cultures, from which they cannot be extracted without loss of vitality (Gray 1995, 43–75, 129–31). As spontaneous creations of particular groups, cultures are living webs of interconnected values. The elements of one web cannot be artificially mixed with those of others to form some artificial utopia. Virtues peculiar to the pagan culture of Periclean Athens cannot be ripped from it and rationally combined with virtues peculiar to Christian culture, for example, as Mill ([1859] 1977, 266) mistakenly supposed. Rather, as Machiavelli suggested, the values of pagan self-assertion and Christian self-denial are incompatible and cannot be rationally harmonized within the same culture.[3]

To make room for romanticism, then, Berlin seems to be saying that a genuine rationalism must recognize, first, that there are plural irreducible values and, second, that there is no perfect moral or cultural arrangement in which all conflicts of values are rationally resolved. By implication, a genuine rationalism must reject both value monism (i.e., descriptive homogeneity of the feasible options of value as, for instance, just so many different quantities of welfare) and—something quite different—the possibility of a best or utopian outcome that beats or at least ties every other feasible option in an all-things-considered ethical ranking of the given options. In short, a genuine rationalism must recognize the validity of agonistic pluralism, which does reject both monism and perfectionism. Rejection of monism does not preclude the possibility of a complete and transitive all-things-considered ordering. As Sen (1987, 63, n. 6) warns, "the issue of ethical ordering must not be confused with that of descriptive homogeneity . . . an ordering does not re-

quire descriptive homogeneity." But rejection of any possibility of a best outcome does entail rejecting any chance of a complete and consistent ordering.

Agonistic Pluralism and Rational Indeterminacy

Agonistic pluralism affirms that conflicts among incommensurable values cannot be rationally terminated. The conflicting values, being equally ultimate and giving rise to equally absolute directives, cannot be reasonably compared and balanced to yield a best outcome that beats or at least ties every other possible outcome in the domain implicating those values. Rather, any ethical ranking that gives due consideration to the respective directives of the equally ultimate values must exhibit rational indeterminacy, such that no option can be rationally chosen as best from the given set of feasible options.[4]

To illustrate how tragic pluralism gives rise to rational indeterminacy, consider the suggestion by Berlin (which he traces to Machiavelli) that Christian and pagan cultures are distinct and irreducible forms of life between which at least some conflicts cannot be rationally resolved. Suppose that Christian virtues conflict with pagan values in the context of a domain of three possible outcomes x, y, and w. Let x be a wholly pagan Florence in which political leaders and citizens are expected to use ruthless cunning to promote the wealth and glory of the republic, y be a purely Christian Florence in which the church organizes all social practices to promote the glory of God, and w be a partly pagan and partly Christian Florence in which an attempt is made to mix ruthless cunning in public life with devotion to family and God in private life. Since the conflicting Christian and pagan values are by assumption rationally incomparable, there is no feasible option z at which pagan and Christian elements are rationally harmonized to produce a Florentine utopia. Any feasible mixture such as that at w saps the vitality of the ingredients and cannot be viewed as a best outcome. At the same time, since the conflicting values are by assumption equally ultimate, neither x nor y is a best option. Because there is no best option, there can be no complete and transitive ethical ranking that rationally integrates the directives of the competing values with respect to x, y, and w.

To clarify the latter point, let us first restrict attention to the pagan form of life. Clearly, it directs us to rank x ahead of w, and w ahead of y, to yield a complete and transitive "pagan ranking": $x > w > y$.[5] Given that

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[3] For a defense of Mill's vision of an ideal utilitarian liberal culture, in which pagan spontaneity in self-regarding conduct is integrated with Christian obedience to general rules of other-regarding conduct, see Riley 1998, n.d.

[4] It is not that equally ultimate values are equally valuable, in which case we could compare their conflicting rankings of options to determine a best option in terms of value overall. Rather, the values are equally basic, having no common source, and their directives cannot be reasonably compared at all when conflicts arise. Even Gray at times seems not to appreciate that indeterminacy must arise in an overall ranking of any two options if the directives of rationally incomparable values come into conflict with respect to that pair (e.g., Gray 1998, 27).

[5] The symbol $>$ denotes "better than." Strictly speaking, the symbol should be suitably indexed to make clear that the ranking is a pagan directive, e.g., $>_p$ denotes "better than in terms of pagan values."

it is reasonable for anyone who imagines herself in the position of a pagan to hold this ranking, it may be accepted by pluralists as a reasonable pagan directive for this domain. (It is possible to introduce rational indeterminacy at this point by assuming that paganism issues an incomplete directive or that rationally incomparable pagan values issue conflicting directives. But I shall ignore these complexities for ease of exposition.) Next, let us turn to the Christian form of life and restrict attention to it. Clearly, it directs us to form a complete and transitive "Christian ranking": $y > w > x$.

Now we have two conflicting fully determinate rankings, one rooted in paganism, the other in Christianity. Under tragic pluralism, the conflict cannot be rationally resolved. This failure of reason can be expressed as rational indeterminacy in an all-things-considered ethical ranking that takes account of pagan and Christian values. Such a ranking may be interpreted as the product of a process of rational judgment (or rule of rational choice) that aggregates or deliberates over the pagan and Christian rankings after correctly identifying them. For convenience, suppose that the process is internal to the individual. Its domain is profiles of value rankings of the options, one ranking for each value or form of life (paganism, Christianity) under consideration.

Following Sen (1987, 65–8), rational indeterminacy in the all-things-considered ranking of options may be interpreted in either of two ways, namely, incompleteness and overcompleteness. According to the incompleteness interpretation, no all-things-considered ranking of x, y, and w exists at all in this instance. Agreement among the directives of any rationally incomparable values is deemed necessary to determine an all-things-considered ranking of any pair of options implicating those values. But there is no agreement between any of the pairwise directives of the pagan and Christian values. If we consider x and y, for example, paganism directs $x > y$, whereas Christianity directs $y > x$. Because the directives conflict and there is no rational basis for settling the conflict, we must refuse to assert that either ranking is reasonable, all things considered. We must refuse to assert even that x is as good as y, all things considered. There is thus no all-things-considered ranking of x and y. Of course, we can arbitrarily pick x when y is available, thereby revealing a preference for paganism over Christianity with respect to the ethical choice between x and y, but we have no reasonable basis for doing so. Similarly, we can arbitrarily "reveal" a complete and transitive ranking of the three options, but we have no rational argument for doing so.

According to the overcompleteness interpretation, the all-things-considered ethical ranking is $x > w > y >$

$w > x$. Inconsistency appears in the ranking because conflict between the directives of any rationally incomparable pair of values is deemed sufficient for both directives to appear simultaneously in the all-things-considered ranking of any pair of options implicating those values. If we consider x and y, for example, both the pagan $x > y$ and the Christian $y > x$ are included in the all-things-considered ranking. Because the directives conflict and there is no rational basis for settling the conflict, we must assert that the inclusion of both is reasonable, all things considered. Although this might seem bizarre, Sen (1987, 66) insists that pluralists can reasonably accept "the compelling nature of two [or more] potentially conflicting principles of overall judgment with an overlapping domain." Indeed, overcompleteness is merely another way of interpreting the fact that there is no reasonable basis for settling the conflict between pagan and Christian values with respect to this pair of options. Again, arbitrary or "radical" choices may reveal a preference for one option (and value) over the other, but such revealed preferences are not reasonable judgments about the relative importance of the conflicting values involved.

Incompleteness and overcompleteness in the all-things-considered moral ranking of any pair of options x and y are distinct descriptions of the same phenomenon, to wit, an inability to choose rationally between x and y or to judge which is best. Whether described as incompleteness or overcompleteness, such rational indeterminacy means that it is not the case that y is better than x, or that x is better than y, or even that x and y are equally good, all things considered. True, we may have to pick x or y, but we have no reasonable basis for choosing. When we consider the pair, both x and y are what Sen (1997, 763) calls a "maximal" option, that is, one that is unbeaten by any other options, although it does not necessarily beat or even tie them because it may not be ranked against them.[6]

Thus far I have emphasized that rational indeterminacy can arise because of a conflict of rationally incomparable values. But it also may arise in situations in which there is no conflict of values, as illustrated by the story of Buridan's ass, which starved to death because it could not rationally determine which of two identical stacks of hay would be best to eat. Even if we cannot reasonably say that one stack is better than, worse than, or as good as the other, a rational beast would surely make a radical pick of one stack to eat, rather than starve. As Sen (1997, 765) puts it, "only an ass will wait" for a best option when no possibility exists of finding one.

Worthy of emphasis is that rational indeterminacy is irremediable when it arises because incompatible values are asserted to be incomparable. The indeterminacy is not contingent on lack of information or faulty deliberation. It reflects the supposedly genuine structure of moral value. The most penetrating examination based on perfect information cannot tell us how to weigh these values rationally to remove the conflict.

Indexing would remove the possibility of confusing the pagan value ranking with distinct types of rankings of the same possible outcomes, e.g., a Christian value ranking $>_c$, an all things (values) considered ranking $>_*$, a rational person i's ranking $>_i$, or a majoritarian social ranking $>_m$. Nevertheless, I shall not bother with indexing when discussing these and other types of rankings later in the text. The relevant type(s) of ranking will always be obvious from the context.

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[6] A best option is by definition also a maximal option, but a maximal option may not be a best option.

The intractability of the indeterminacy is important. Even monists have no trouble admitting indeterminacy that is contingent on mistakes in judgment or lack of information about the quantities of value (such as welfare) involved in some options, as is arguably the case in the story of Buridan's ass.[7] But monists and benign pluralists insist that any indeterminacy arising from these familiar sources is remediable. The possibility of complete and transitive all-things-considered orderings is left open because any competing values are always rationally comparable in principle.[8]

To sum up, agonistic pluralism holds that incommensurable values are not only plural and irreducible but also rationally incomparable. Given that conflicting forms of good cannot be rationally harmonized if they are rationally incomparable, there is no possibility of a best outcome in which all conflicts of values have been rationally resolved. Rather, there must be rational indeterminacy in any all-things-considered ranking that reflects the pluralist nature of value. Because agonistic pluralism claims that there is no best option and that radical choices must be made between maximal options that embody incomparable forms of good, it implies that all-things-considered deliberation genuinely cannot avoid irreparable losses of distinctive values. Some values lost with rejected maximal options cannot be replaced or compensated because the values associated with chosen maximal options are not comparable with the lost values.

Rational Liberal Limits on Agonistic Pluralism

As Sen (1987, 67) argues, the presence to some extent of incompleteness or overcompleteness (permanent or otherwise) in an all-things-considered ranking entails "no departure from rational choice." There is nothing incoherent about a rationalism that accepts limited zones of indeterminacy as reasonable in its all-things-considered judgments. It is rational to make a radical pick of any maximal option when restricted to the given subset of options comprising any particular zone. At the same time, rationalism cannot permit these zones to extend over the whole set of feasible outcomes without destroying itself. Such an extension would be forced on us if we were to accept an unlimited agonistic pluralism. But this would amount to "a kind of lunacy," as Berlin (1999, 145) states in his critical assessment of unbridled romanticism. In other words, only a lunatic can assert that all values (and cultures) are incomparable. A rational human can only accept agonistic pluralism within reasonable limits. The next step is to clarify how it is restrained in a theory such as

Berlin's, which brings us to the "liberal rationalist" part of agonistic liberal rationalism.

Berlin insists that there is a common moral horizon that establishes a lower bound on what can count as moral thinking and behavior among human beings. He rejects Nazi culture as immoral, for example, as falling below the common moral threshold. But his critics seem puzzled by this aspect of his thought, and they claim that the common horizon neither limits the field of agonistic pluralism nor provides any support for liberal rationalism (e.g., Gray 1995, 157–8; Kateb 1999, 1028). In contrast, I shall argue that the common horizon does both.

Earlier I called attention to Berlin's claim that the pluralist nature of value is a warranted inference from the everyday experience of human beings. This knowledge is part of the common moral horizon as Berlin envisions it, which anyone capable of rational thought may be expected to apprehend. A genuine rationalism thereby affirms the truth of agonistic pluralism, at least with respect to some limited field of ethical and political choices. But there is more to the common horizon than pluralism. As I discuss later, he suggests that another warranted inference from everyday experience is that liberal values are far superior to illiberal ones. In Berlin's view, reasonable people widely agree that basic human rights (or at least some minimum set of them) must be duly protected by any decent society (even if the language of rights is not always employed to affirm this). For him, it seems, a genuine rationalism affirms the moral priority of (minimal) liberalism in this sense. It follows that agonistic pluralism does not extend to conflicts between liberal and illiberal values. These two kinds of values (and cultures) are rationally comparable, one to another, and conflicts between them can be rationally terminated in favor of liberalism.

To escape Berlin's charge of "lunacy," in other words, an agonistic liberal rationalist maintains that fundamental liberal values are reasonably viewed as more important than any other ideals created by men. Of course, various ways of justifying the relatively immense importance of liberal norms are open to the rationalist. Berlin even hints at the possibility of a restricted utilitarian justification, according to which the superiority can be inferred by comparing liberal and illiberal values in terms of common welfare. "Utilitarian solutions are sometimes wrong, but, I suspect, more often beneficent. The best that can be done, as a general rule, is to maintain a precarious equilibrium that will prevent the occurrence of desperate situations, of intolerable choices—that is the first requirement for a decent society" (Berlin 1991, 17–8).

The structure of the common moral horizon can perhaps be clarified by considering what sort of reasoning process is implied by it for making all-things-considered ethical and political choices. Given that the force of agonistic pluralism is limited by the relative superiority of liberal values over illiberal ones, as Berlin suggests, any reasoning process that genuinely reflects the nature of value must, first, give suitable priority to liberal values when conflicts arise with

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[7] Not everyone accepts that the donkey's dilemma is properly interpreted as a case of indeterminacy. There are no conflicts of plural and incomparable values. Thus, it can be argued that indifference between two equally valuable haystacks leads to the beast's demise. Moreover, given that eating either haystack is more valuable than starving to death, the beast is clearly irrational on either interpretation.

[8] A similar distinction between "tentative" and "assertive" indeterminacy is drawn by Sen (1997, 763–4).

illiberal values; second, recognize that conflicts of incomparable values cannot be rationally resolved; and, third, permit radical choices when confined to the maximal options within any zone of rational indeterminacy associated with conflicts of incomparable values. In short, a (minimally) liberal rational choice process must be employed to generate all-things-considered rankings, which will typically exhibit some indeterminacy, from any set of particular value rankings (one ranking for each value) over a given domain of options. The relevant liberal procedure can be viewed as an ethical process of personal judgment if the plural values being considered are assumed to be located within one reasoning person. It can also be viewed as a political process of social choice if the different values are assumed to be located within different persons. In either case, certain liberal values, including basic human rights and correlative duties, must be seen as generally more weighty than competing values, perhaps even infinitely so.

It may be useful to say a bit more about a genuinely rational liberal political process in this context. To be at least minimally liberal, the political system must duly secure at least some minimum set of human rights (left unspecified for the moment). Perhaps this security can be provided without political democracy, even if equal citizenship provides the best security. A constitutional monarchy may be sufficient, despite the unequal privileges accorded to its subjects. For the sake of illustration, however, consider a more advanced liberal process: majority rule confined by suitable checks and balances within the limits of a highly developed system of equal rights and liberties. Unlike a mainstream rationalist, Berlin cannot say that this political process enables citizens to resolve all their value conflicts. Like the romantics, he is committed to the truth that conflicts of incommensurable ideals are forever beyond rational resolution. He must therefore reject any epistemic view of the political process as a method for generating a complete and transitive all-things-considered social ranking that is most likely to represent the true unobservable ranking of the options.

Young (1988, 1995) adopts an epistemic perspective, for example, when arguing on the basis of Condorcet's work that simple majoritarian procedures can be extended to generate a complete and transitive ranking that is a maximum likelihood estimate of the true ranking. But Berlin, despite his declared sympathy for Condorcet's rationalistic aspirations (Jahanbegloo 1992, 75–6), insists that objective rankings are indeterminate with respect to some subsets of options because rationally incomparable values come into conflict over those subsets. In his view, rather than try to make a best guess at how to remove this indeterminacy, rational people must recognize that it is genuinely irremediable and make radical choices that lack rational justification.

To illustrate, suppose, as a first approximation, that rationally incomparable pagan and Christian values coincide with, respectively, two separate individuals, Arnie and Jack. Arnie is entirely pagan, and Jack is purely Christian. With the same options described in

the earlier example, Arnie has the ranking $x > w > y$, and Jack has the ranking $y > w > x$. These two people have conflicting rationally incomparable complete and transitive rankings of the options. Any all-things-considered ranking is now a social ranking produced by a political process that considers the individual rankings of the options. Rational indeterminacy arises in the social ranking because the conflicts between pagan and Christian directives cannot be reasonably settled. The liberal political process cannot rationally resolve the issue of whether Arnie's ranking (reflecting pagan values) is more important than Jack's ranking (reflecting Christian values). But it can reasonably be used to make radical choices between different maximal options within the boundaries set by fundamental liberal norms. Since x, y, and w are all maximal options in the example under consideration, any one of them can be picked over the others by the liberal process. It is reasonable for the process to reveal any social ranking of these options in order to fill in this zone of rational indeterminacy without genuinely removing it.

Of course, it makes little sense to match separate values to separate persons on a one-to-one basis. In general, we must assume instead that each rational person will form an all-things-considered ranking that in her judgment reasonably takes account of the directives of the plural values that figure into her choices over options. (These values may be said to comprise the culture—or at least the relevant portion of it—that shapes her ethical and political choices). Given Berlin's claim that rationally incomparable values are known to conflict in the context of some zone of options, however, any all-things-considered personal ranking must exhibit rational indeterminacy—incompleteness or overcompleteness—over that domain. Still, agents may be forced by circumstances to choose between options within the zones of rational indeterminacy.

With this in mind, even if we assume that everyone has perfect information and makes no errors in judgment, different persons must be expected to reveal different complete and transitive rankings of the options. Rational agents who recognize the genuine nature of moral value will make the same comparisons of any rationally comparable values and to that extent form the same all-things-considered ranking over options involving those values. But when competing values are rationally incomparable, different persons may reasonably make different radical picks between options implicating those values: There are no criteria of rationality to determine anyone's choices. In other words, different rational persons will typically reveal competing all-things-considered rankings to the extent that they make competing radical choices to fill in the zones of rational indeterminacy inherent in the nature of value.

We can then speak of a liberal political process that generates social rankings of options from profiles of complete and transitive all-things-considered personal rankings of the same options (one ranking revealed by each person), with the important caveat that disagreement among personal rankings will typically be observed even if everyone is assumed to be fully rational

and moral. This political process cannot genuinely resolve conflicts of rationally incomparable values. Any social ranking produced by it will exhibit genuine rational indeterminacy, even if that indeterminacy is hidden in a complete and transitive "revealed" ordering generated from the set of complete and transitive personal orderings.

Agonistic liberal rationalists view the liberal political process as a reasonable device for making radical picks among maximal options, where any pick is compatible with liberal values (as reflected in the liberal procedure) but does not imply that underlying conflicts of rationally incomparable values have been rationally terminated. The point is that a political decision may be needed for reasonable public purposes now, at a given time and place, even though a best option cannot be rationally determined. But liberal rationalists of Berlin's ilk do not hope to find best options, let alone complete and transitive social rankings. Rather, they insist that many distinct options can be maximal for liberal purposes because conflicts of irreducible values cannot be rationally resolved. It is reasonable to confine the liberal political process to these maximal options, but no rational justification is available for picking one maximal option rather than another. Thus, the liberal democratic process itself can also be viewed in romantic terms as an inexhaustible creative process within liberal limits, a continuing public decision procedure in which citizens may freely participate in an indefinite series of maximal picks over time without ever being constrained to provide a fully determinate all-things-considered social ordering of the feasible options.

Against this dynamic romantic view, it may appear that any liberal democratic procedure must by definition rationally compare what are supposed to be the incomparable values held by different individuals. This is not so. In the case of majority rule constrained by a liberal system of basic rights, for example, majority rule is properly defined without making reference to equal weighting of different persons' rankings of options. Given that those rankings are rationally incomparable to the extent that they involve different radical picks within the zones of rational indeterminacy associated with conflicts of incomparable values, majority rule generates a social ranking from incomparable personal rankings. Indeed, given that each person's ranking can be represented by a purely ordinalist utility function because the ranking is complete and transitive, majority rule works with a set of incomparable personal utility functions. Because the personal rankings (whether represented by utility functions or not) are not comparable, it is meaningless to speak of equal weighting (or any other weighting) of them. Majority rule makes no rational comparison of the different personal rankings or of the rationally incomparable values reflected in them.

This is not to say that liberal democratic procedures will generally yield fully determinate social rankings if competing personal rankings are incomparable. Given three or more options, Arrow's (1963) impossibility theorem and related results show that every decision

rule (including majority rule) that satisfies some seemingly mild conditions of liberal democracy will generally fail to generate a complete and transitive social ranking of the options. To cite a familiar example, suppose that persons i, j, and k reveal the following rankings: for i, $x > y > w$; for j, $y > w > x$; and for k, $w > x > y$. Because majority rule aggregates over each personal ranking without assigning any relative weights (including equal weights) to them, it yields a cyclical social ranking: $x > y > w > x$. But the lack of a complete and transitive majority ranking is hardly unreasonable from Berlin's perspective; after all, the pluralistic nature of value precludes any genuine rational ordering of this utopian sort. To that extent, the romantic spectacle of majority preference cycles is quite compatible with Berlinian liberal rationalism. The fact that a majority's radical picks may alternate repeatedly among some given subset of maximal options more or less by accident is neither here nor there.

At the same time, there is a need for liberal institutional devices (including basic rights) to keep majority rule within liberal limits. These devices serve to veto illiberal options even if majority cycles over the maximal liberal options remain. Such devices can also be used, however, to fill in zones of rational indeterminacy in the social ranking. For example, each person may be given the right to cast one equally weighted vote for his first-place choice (ignoring the remainder of his ranking). Then x gets one vote, y gets one vote, and so does w in the above example. Majority voting restricted in this way yields a three-way tie among x, y, and w, that is, complete social indifference. In effect, the equal franchise artificially converts each of the maximal options into best options. The social ranking becomes fully determinate, and we can simply draw straws to pick one of these best options. Any standing tie-breaking rule is acceptable in this situation.

By employing majority rule constrained by such liberal devices, agonistic liberal rationalists can artificially remove indeterminacy in all-things-considered social rankings without pretending to make rational comparisons of genuinely incomparable values. In effect, weighty liberal values are extended, such that it comes to seem "reasonable" that the political process artificially converts one of many distinct maximal options into a "best" option, which must be accepted by the members of society (under threat of social coercion) despite underlying conflicts of rationally incomparable values. But "political reasonableness" so understood is not concerned with discovering a genuine resolution of the relevant conflicts of values. Rather, it is concerned with making decisions that serve liberal purposes for the moment without terminating rational debate about what is best, all things considered.[9]

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[9] Rawls's (1993) "political liberalism" also relies on a distinction between criteria of truth and standards of public reasonableness. I do not mean to suggest that Rawls's distinction plays out in the same way as the distinction I have attributed to Berlin.

Plural Liberty

Despite the liberal limits on its scope, agonistic pluralism extends even within liberalism's citadel: "It goes all the way down, right down into principles of justice and rights" (Gray 1995, 60). Given that liberal principles themselves cannot be insulated from pluralism, different basic rights may implicate rationally incomparable values that clash irreconcilably when the rights conflict with one another.

Berlin's distinction between positive and negative liberty is too well known to require much discussion. I shall emphasize merely that, for Berlin, liberty itself is plural. Its different aspects may be associated with conflicts of rationally incomparable values:

> If the claims of two (or more than two) types of liberty prove incompatible in a particular case, and if this is an instance of the clash of values at once absolute and incommensurable, it is better to face this intellectually uncomfortable fact than to ignore it, or automatically attribute it to some deficiency on our part which could be eliminated by an increase in skill or knowledge; or, what is worse still, suppress one of the competing values altogether by pretending that it is identical with its rival—and so end by distorting both (Berlin 1969, 1).

Berlin (1969) admits that positive liberty may be "at no great logical distance" (p. xliii) from negative liberty, and the two aspects of freedom "cannot be kept wholly distinct" (p. 131). But they are not the same thing. Democratic self-government, for example, is "logically uncommitted" to negative liberty (p. 165). Moreover, rights to participate in self-government may clash with property rights or rights to privacy, and the values attached to the respective rights may be incomparable.

Berlin (1969, xlvii) does not doubt that positive liberty is "a valid universal goal" and "something valuable in itself." Contrary to Taylor (1979), he seems to think that both positive and negative liberty can be rooted in a fundamental "opportunity concept" of freedom that comprehends the need for an agent to have internal opportunities (i.e., certain minimum rational capacities to make choices) as well as external ones (i.e., an absence of certain external obstacles to potential choices). Yet, he emphasizes that positive liberty (unlike negative liberty) was twisted historically into false ideals of rational self-perfection, the alleged desirability of which was used to justify forms of authoritarianism. Berlin (1969) is careful to point out that a similar history of perversion "could equally have been the fate of the doctrine of negative liberty" (p. xlvii), but that did not happen. "Hence, the greater need, it seems to me, to expose the aberrations of positive liberty than those of its negative brother" (p. xlvii). In his opinion, negative liberty does not possess the confusing historical connotations associated with positive liberty, and it is thus less likely to obscure the basic idea of freedom as opportunity (pp. lxi–lxii; see also Jahanbegloo 1992, 40–2). As a result, he tends to identify the basic idea with its negative aspect. But that should not blind us to the value of positive liberty, or to the possibility of irreconcilable clashes between it and the value of negative liberty.

Given that the idea of liberty itself is not immune from the force of agonistic pluralism, it follows that plural and rationally incomparable liberal cultures may come into conflict. Each liberal culture may be built on a distinctive conception of freedom or set of rights and liberties. More generally, each culture may be associated with a distinctive liberal political procedure. If conflicts arise between the directives of these liberal cultures with respect to some overlapping domain of possible outcomes, then rational indeterminacy can arise in any all-things-considered universal liberal ranking of the options. There is no best liberalism that could remove this indeterminacy.

The Priority of Basic Human Rights

Even though Berlin says that liberty is plural, he emphasizes that it is of immense importance relative to competing values. He argues that a capacity to recognize this is inseparable from "what we mean by being a normal human being."

> There must be some frontiers of freedom which nobody should be permitted to cross. Different names or natures may be given to the rules that determine these frontiers: they may be called natural rights, or the word of God, or Natural Law, or the demands of utility or of the "permanent interests of men" . . . . What these rules or commandments will have in common is that they are accepted so widely, and are grounded so deeply in the actual nature of men as they have developed through history, as to be, by now, an essential part of what we mean by being a normal human being. Genuine belief in the inviolability of a minimum extent of individual liberty entails some such absolute stand (Berlin 1969, 164–5).

In other words, our understanding of what it means to be a reasonable and decent person entails acceptance of moral rules that distribute equal rights for all. Certain equal rights must be recognized by any "normal human being" even if, as a result of prejudice or ignorance in particular social contexts, these equal claims do not receive equal protection under prevailing laws and customs. Also, the system of equal rights must be viewed as being of paramount or near-paramount moral value. The values attached to basic rights must generally override other values in cases of conflict, so that a minimum extent of individual liberty may remain inviolable.

The foregoing quotation is not an isolated statement by Berlin. In his view, it is a "general truth" that human rights are essential to any "decent, even tolerable way human beings can live with each other" (Jahanbegloo 1992, 114). He insists that every decent or civilized way of life suitably privileges human rights.

> The idea of human rights rests on the true belief that there are certain goods—freedom, justice, pursuit of happiness, honesty, love—that are in the interest of all human beings, as such, not as members of this or that nationality, religion, profession, character; and that it is right to meet these claims and to protect people against those who ignore or deny them. I think that every [normal or decent] culture which has ever existed assumed that there exist such rights—or at least a minimum of them (Jahanbegloo 1992, p. 39).

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By implication, any acceptable culture is at least minimally liberal in the sense that it suitably privileges at least some minimum core of equal rights for all. A form of life that does not do this is barbaric, unacceptable (if not "unintelligible") to "normal" human beings.[10]

When discussing this requirement of any decent society, Berlin typically uses the language of negative liberty to avoid the confusions surrounding the idea of positive liberty due to its historical perversions. But he does not deny that a reasonable conception of positive liberty also enters into the minimum core of basic rights. The great value of freedom extends to "both the positive demand to have a voice in the laws and practices of the society in which one lives, and to be accorded an area, artificially carved out, if need be, in which one is one's own master, a 'negative' area in which a man is not obliged to account for his activities to any man so far as this is compatible with the existence of organized society" (Berlin 1969, lx, emphasis added). Thus, the minimum core comprehends both negative and positive liberty: freedom from coercive interference by others and a minimum degree of control over one's life, including a (not necessarily equal) voice in the laws and practices of society.[11]

If the minimum core of equal rights comprehends both positive and negative liberty, however, then conflicts of liberal rights within the core can apparently implicate these irreducible aspects of plural liberty. It might be claimed that the liberal values attaching to conflicting rights can always be rationally compared, in which case these particular conflicts can always be rationally resolved to bring about an ideal system of harmonious rights. But this is contrary to the agonistic spirit of Berlin's approach. It is more consonant with his view to assume that the conflicting liberal values may at times be rationally incomparable, such that there is no possibility of an optimal system of rights and liberties. Thus, when liberal rights clash, we may be forced to make radical picks between them.

At the same time, we must be able to identify illiberal claims, so that they cannot masquerade as liberal rights and defeat the possibility of a decent society. Unless there is a reasonable way to distinguish

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[10] Berlin at times suggests that barbaric forms of life that sink below the common moral horizon are simply "unintelligible" to normal humans. Civilized people must be able to understand barbarism to some extent, however, in order to identify it, veto it as indecent, and communicate its indecency to one another.

[11] Berlin is not as clear as he might be about the priority of plural freedom in his doctrine. For discussion of some of the ambiguities, see Gray 1995, 5–37, and Morgenbesser and Lieberson 1991, 22–5. I believe he is saying that liberty (with its plural aspects) generally takes priority over other values in cases of conflict, except in extraordinary situations, when suppression of freedom may be justified to prevent imminent social catastrophe (Berlin 1969, lx–lxi). Although the minimum core of basic rights is not inviolable strictly speaking, "we recognize that under normal conditions, for the great majority of men, at most times, in most places, these frontiers are sacred, that is to say, that to overstep them leads to inhumanity" (p. lxi). Moreover, even in normal conditions, neither negative nor positive liberty has absolute priority. Given that these irreducible aspects of liberty can come into conflict, it may be reasonable to privilege negative liberty in some situations and positive liberty (including democratic extensions) in others. This is true whether we adopt benign pluralism or agonistic pluralism.

between liberal and illiberal claims, agonistic pluralism becomes unbridled again; for example, the right not to be enslaved and the claim to own another person as property can both be associated with liberty (as they were in the United States prior to the Civil War), with clashes between them seen as implicating rationally incomparable values. If the values attaching to these two conflicting claims are incomparable, however, then agonistic liberalism comes apart, because it must permit radical choices for proslavery options when antislavery options are available. That is, slavery is not vetoed as barbaric.

To keep tragic pluralism within liberal limits, it must be possible to distinguish liberal claims and values from their illiberal counterparts. Conflicts between liberal rights can implicate rationally incomparable values, as can conflicts between illiberal claims. But conflicts between a liberal right and an illiberal claim cannot implicate incomparable values. Rather, liberal rights (and, more generally, liberal political procedures) must be far more valuable than illiberal alternatives.

Admittedly, Berlin does not say much about how liberal claims can be reasonably distinguished from illiberal ones. As a result, it may seem difficult to draw a clear line between liberal culture and illiberal alternatives. Despite his reticence on this point, however, something can be learned from his insistence that every liberal culture must privilege at least some minimum of rights due equally to all humans. To escape from barbarism, he says, society cannot deny equal liberty altogether. But this means that some putative rights are not eligible to be liberal rights. A right to enslave others is not eligible, for example, nor is a right to sell oneself into slavery: These claims are incompatible with the preservation of any extent of equal liberty for all.[12] It makes no sense to say that liberal culture can involve equal rights to enslave one another. There is no equal liberty in a slave society. Rather, an equal right not to be enslaved is essential to minimal liberalism. Again, a putative right to kill innocent members of particular ethnic or racial groups is an illiberal claim. It denies that a minimum core of equal rights attaches permanently to each person by virtue of his humanity. An equal right to be free of such ethnic and racial violence must be an element of any liberal minimum.

It emerges that an agonistic liberal rationalist must assume the possibility of a minimum core of equal rights that do not conflict with one another. Any claims that annihilate a person's core right not to be enslaved, for example, are illiberal claims. Liberal claims cannot violate any of the basic rights included in the liberal minimum. I have called attention to a couple of equal rights that seem to be elements of a liberal minimum, and no doubt there are others. Even with just the two mentioned, however, the requirement of suitable priority for a minimum core of equal liberty has remarkably powerful implications for liberal culture. For example, given that an equal right not to be killed arbitrarily by others is essential, any minimally liberal culture may entail an equal right to be free of starva-

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[12] For a similar statement, see Mill [1859] 1977, 299–300.

tion caused (however indirectly) by social and economic institutions. In that case, liberal property rights would have to be tailored accordingly to permit redistribution of wealth when required to prevent starvation. The idea that private owners have an absolute right to retain their surplus wealth when others are starving would become an illiberal perversion of the idea of negative liberty. Myriad examples of this sort are conceivable. Thus, the requirement of at least a minimum extent of equal liberty may place many constraints on what claims can count as liberal rights in any given social context.

I shall not attempt to delineate with any precision the contours of a liberal minimum core of equal rights.[13] Regardless of what marks the boundary between minimally liberal culture and barbarism, claims that conflict with rights in the minimum core must be illiberal claims. Adding illiberal claims to the liberal minimum does not expand the extent of equal liberty but instead compromises the minimum. In contrast, many liberal rights can be added to those in the minimum core, recognizing that liberal rights outside the minimum are by definition equal rights that do not conflict with the minimum rights. Liberal property rights or political rights can be added, for example, without violating the basic right not to be enslaved or the right not to be starved arbitrarily. Adding liberal rights to the liberal minimum expands the extent of equal liberty, even though clashes of liberal rights outside the minimum may implicate rationally incomparable values.

AGONISTIC LIBERAL RATIONALISM

I shall now draw together the threads of my reconstruction of Berlin's agonistic liberal rationalism. Berlin certainly rejects the possibility of a rational liberal utopia, an implication of his tragic brand of pluralism. Thus, he warns against the vain pursuit of any ideal culture. Like Buridan's ass, liberals may lose everything if they persist in searching for the nonexistent ideal. Consistent with his warning, however, Berlin can also claim that a common moral horizon confines reason-

able humans to more or less imperfect liberal cultures, which assign "immense value" to at least some minimum core of equal rights, including rights not to be murdered, enslaved, and the like.

Even if all ideas of progress are rejected so that one liberal culture cannot be said to be any more advanced than another, each and every liberal culture can still be rationally justified as a maximal option that beats every nonliberal alternative (since all nonliberal options are nonmaximal) yet does not beat or tie any of the other maximal options. Any liberal culture is reasonably ranked above any nonliberal one because the latter fails to meet the common moral standards—it does not protect even a minimum core of equal rights. Yet, no liberal culture can be reasonably ranked above another, given that rationally incomparable values attach to the different rights in their distinctive systems of equal liberty when conflicts arise between the systems (keeping in mind that conflicts between them cannot arise with respect to the common minimum core of equal rights). It is not that any two liberal cultures are equally good. Rather, any two maximal options are rationally incomparable. Complete indeterminacy exists in the ranking of distinct liberal cultures.

Although agonistic pluralism reaches into liberal values themselves, agonistic liberal rationalism posits an all-things-considered partial ranking of feasible cultures. That ranking is completely indeterminate over a top set of at least minimally liberal cultures, but it does reasonably veto every illiberal option from the top set. Moreover, people immersed in the customary rules of a particular liberal society are free to affirm them radically, even though there is no rational justification for picking those particular customs instead of competing liberal options. In contrast, under an unbridled romantic approach, in which agonistic pluralism is unrestrained, every culture (liberal and illiberal alike) must be seen as a maximal option. No all-things-considered ranking of different cultures is possible. Highly illiberal options (including slave systems and Nazi ones) cannot be ruled out as inferior to liberal options, because liberal values are not rationally comparable to illiberal ones.

Despite Berlin's rejection of teleology, it may also be reasonable for an agonistic liberal rationalist to defend an idea of progress in the sense of movement toward a more extensive system of equal rights and liberties (although not necessarily a greater level of general happiness or justice). Berlin states: "I believe in working for a minimally decent society. If we can go beyond this to a wider life, so much the better" (Jahanbegloo 1992, 47). A particular liberal culture advances by recognizing and enforcing a greater number of equal rights, taking for granted that freedom to choose has immense importance in comparison to competing values. There is simply a greater expanse of equal freedom in the more advanced culture. Such progress requires an increasing number of radical political choices to be made, and made repeatedly. This occurs because as more and more liberal rights outside the minimum core are recognized, there are more and more conflicts of liberal rights that may implicate

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[13] Rawls (1999, 544–55) discusses a minimum set of human rights which he considers essential to any decent society, but his view of liberal culture is quite restrictive because he ties it to equal citizenship and state neutrality. Taylor (1994) suggests that liberalism can be committed to the survival and flourishing of particular group forms of life within a larger community, if adequate safeguards are provided for basic rights of persons outside the privileged groups. Habermas (1994) objects persuasively that group rights to cultural reproduction cannot be guaranteed by democratic liberalism. Even so, it may be a mistake to insist that liberalism is inseparable from democracy, as if no basic individual rights are recognized by non-democratic societies. What Rawls calls decent nonliberal societies may better be viewed as at least minimally liberal, to emphasize continuities between advanced democratic Western societies and nondemocratic societies (including pre-democratic versions of Western societies themselves) that honor some minimum of human rights. Surely any society that honors all of the human rights that Rawls considers essential to human decency is aptly described as being at least minimally liberal. Even if we accept Rawls's view to the contrary, however, Berlin's rationalism still endorses advanced democratic liberalism, given the idea of progress discussed later in the text.

rationally incomparable liberal values. In deciding which of the conflicting rights and values to enforce, each particular society is free to advance along its own peculiar path. There is no single rational path of progress toward a greater extent of equal liberty. Rather, different liberal cultures advance along incompatible and incomparable paths by picking different liberal rights and values when clashes occur outside the minimum core. Any pick is permissible because the conflicting values attached to the different rights are incomparable, and none of the added rights violates the liberal minimum.

This idea of progress is relative to each particular liberal society. Each pursues its own distinctive path of development toward a greater equal freedom. In effect, each exhibits its own more or less advanced cultural stages as it moves along a trajectory from a minimally liberal phase toward a maximal one. Any two cultures are on the same path if and only if, when the set of rights recognized by the less advanced of the two is considered, they give the same priority to the same rights when conflicts arise (whether or not the conflicts implicate rationally incomparable values). The more advanced culture recognizes all the equal rights recognized by the less advanced one and then some.

Given such a notion of progress, the top set of maximal options is narrowed to include only the more advanced liberal cultures. But myriad advanced liberal cultures can be maximal options, which cannot be reasonably compared or ranked vis-à-vis one another. Any maximal liberalism must give suitable priority to an extensive system of equal rights for all, far beyond the minimum core. But different maximal liberalisms will give priority to different equal rights beyond the minimum. Different cultures may reasonably make different radical picks among conflicting rights that implicate rationally incomparable liberal values, provided the minimum core is not compromised. Also, any maximal liberalism must generally refuse to recognize as legitimate any special privileges or immunities that conflict with its extensive system of equal liberty for all. This requirement makes maximal liberalism far more demanding than minimal liberalism, which merely protects the minimum core of equal rights by refusing to recognize the legitimacy of any claim to annihilate it through slavery, arbitrary killing, and so on.

In addition to being ranked above illiberal alternatives, any particular maximal liberal culture is ranked above less advanced liberal cultures along the same path of development (less advanced versions of itself, so to speak). A maximal liberal culture protects a greater extent of equal liberty, and a smaller (perhaps zero) extent of unequal privileges and immunities, as compared to a less advanced liberal culture that is moving along the same path. Any agent has a greater number of equal opportunities to make her own choices in the more advanced culture, which recognizes all the same rights as the less advanced society and then some. True, maximal options are path-dependent: A liberal culture that is maximal with respect to one path is not maximal with respect to other paths. Indeed, it cannot generally even be rationally compared to dis-

tinct liberal cultures that have made their own peculiar picks in cases of conflict between liberal rights that implicate incomparable liberal values. But this does not alter the fact that every less advanced liberal culture is vetoed from the top set because it is reasonably ranked as inferior to some more advanced culture on its same path.

The upshot is that rationalist agonistic liberalism posits an all-things-considered partial ranking of feasible cultures. Although still rationally indeterminate over a top set of more advanced liberal cultures, that ranking vetoes every other culture—less advanced liberal ones and illiberal ones alike—from the top set. Moreover, the partial ranking is indeterminate over much more than the top set. In particular, there will generally not be a determinate ranking of any pair of liberal cultures following different paths of development. If different rights are picked when conflicts arise that implicate incomparable values, then the two cultures are incomparable. Rational agents are free to affirm radically either one over the other. At the same time, a more advanced culture is reasonably ranked as superior to a less advanced culture traveling the same path.

It should be stressed that a maximal liberal culture is unlikely to stir much excitement in a mainstream utopian rationalist. The various equal rights that can be included in its extensive system of rights are not mutually harmonious, and their clashes may implicate incomparable values. Because one right rather than another must be picked in these clashes, tragic sacrifices of liberal values cannot be avoided. Within the same culture, one right may be chosen over another in some situations, whereas the reverse choice is made in other situations. Different cultures can make different picks in like situations and still remain maximal. Moreover, unless equal liberty is given something like lexical priority over all competing values, a maximal liberal culture may occasionally recognize unequal privileges and immunities as more important than equal rights, with the caveat that such unequal claims cannot go to the length of authorizing their holders to annihilate the basic human rights of others. In short, the liberal cultures justified as maximal in terms of our common moral horizon may be a far cry from rational liberal utopias. At the same time, they are surely distinguishable from, say, a feudal culture, whether the latter is viewed as a less advanced variant of liberalism or as an illiberal option.[14]

CONCLUSION

I argue that Berlin's liberalism is grounded in rational choice as opposed to "radical" choice unguided by reason. His agonistic pluralism does not subvert his liberalism. Rather, his pluralism emanates from his

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[14] A feudal order can be included among nonmaximal liberal cultures if it recognizes the immense value of at least some minimal core of equal rights. It is not necessary that lords and peasants enjoy equal liberty in all respects. But a feudal order in which some are in effect enslaved is an illiberal option, unacceptable in terms of the common moral horizon (or "natural law") of civilized human beings. For related discussion, see Galipeau 1994, 117–9.

liberal rationalism, which insists on reason's inability to resolve certain conflicts of values. As Lukes (1994, 714) emphasizes, "it is a pluralism intended to be compatible with the absolute, overriding, and universal value of liberty, the existence of a common human nature, rational criticism, and the tractability of many but not all value conflicts in public and private life." Berlin's doctrine is best seen as a rationalistic liberalism that affirms pluralism within rational limits rather than as an unbridled romantic liberalism that lacks any rational basis for barring the door against illiberal intruders.

If my reading is correct, Berlin does not favor toleration of illiberal groups that deny basic human rights to their members (including the right to exit from the group). Rather, he thinks it is reasonable to extend at least some minimum of basic rights to everyone across different cultures. Indeed, if (negative and positive) freedom is held to be of such immense importance that a greater extent of equal rights is more valuable than a lesser extent, ceteris paribus, then his liberal rationalism affirms the superiority of any advanced liberal democratic culture over nonliberal or minimally liberal versions of itself. Once an advanced system of liberal rights (some of which may clash irreconcilably) has been established, it is perfectly reasonable for society to employ due coercion to secure that system of rights.

I also suggest that a main concern of Berlin is to unveil what he considers a genuine rationalism in stark contrast to mainstream utopian rationalism, which he repeatedly attacks as false and dangerous to liberty. He emphasizes that reason is too weak ever to resolve certain conflicts of values. A genuine rationalism must make room to a limited extent for a tragic pluralist moral outlook of the sort implicit in romanticism. Such a rationalism is necessarily antiutopian: It must recognize that conflicts of rationally incomparable values will inevitably frustrate any rational vision of ethical and political perfection. Yet, although reason is weaker than its mainstream promoters would have us believe, it remains for Berlin the ultimate guide in ethical and political life. It apparently justifies a common moral horizon that is minimally liberal in content. In particular, liberal political procedures, including at least some minimum core of equal rights, should be given suitable priority over illiberal values. The radical freedom to create and pick among plural and incomparable ideals, so prized by the romantics, is thus kept within reasonable liberal limits.

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Machiavellian Democracy: Controlling Elites with Ferocious Populism
JOHN P. MCCORMICK *Yale University*

This essay demonstrates that Niccolò Machiavelli's political thought addresses the deficiencies of two opposite poles of contemporary democratic theory: As do formal or minimalist approaches, he specifies electoral mechanisms for elite control; and similar to substantive or civic culture approaches, he encourages more direct and robust modes of popular participation. On these grounds, I cull from Machiavelli's Discourses *a theory of democracy in which the populace selects the elites who will hold office but also constantly patrols them through extraelectoral institutions and practices, such as the tribunes of the people, public accusations, and popular appeals. Machiavelli adds to these institutional features of popular government an important cultural dimension: The people should despise and mistrust elites, and they should actively confront the injustice that elite governing inevitably entails. Finally, I explore the ramifications of this theory for debates over elite accountability in contemporary democratic theory.*

The control of elites by the general populace is an overlooked aspect of Niccolò Machiavelli's ([1531] 1997) greatest work, *The Discourses*.[1] Even scholars who understand Machiavelli as an advocate of popular government—as a "republican"— largely confine popular control in his theory to the selection of magistrates from among elite candidates. This essay shows that Machiavelli theorized more extensive, constant, and, especially, animated modes by which the people might control elites. To this extent, his theory combines the strengths of two opposite poles of contemporary democratic theory: As do formal or minimalist approaches (e.g., Dahl 1971; Przeworski 1991; Schumpeter 1942), he specifies and justifies electoral mechanisms for elite control; and similar to recent civic culture and participatory approaches (e.g., Gutmann and Thompson 1996; Putnam 2000; Sandel 1996), he encourages more direct and robust modes of popular engagement with politics. What is more important, by combining the strengths of each approach Machiavelli overcomes their respective weaknesses.

In minimalist theories of popular government periodic elections are the primary and often exclusive methods for assessing the performance of elites and rewarding or punishing them accordingly. But because Machiavelli argues that elites are motivated by a will to dominate, a position that I suggest contemporary democratic theory should adopt as fact or heuristic device,

John P. McCormick is Assistant Professor of Political Science, Yale University, P.O. Box 208301, New Haven, CT 06520-8301 (john.mccormick@yale.edu).

[1] This work is henceforth cited with book and chapter references within parentheses in the body of the text. Any emendations to the cited translation are based on the version of the *Discorsi* in Machiavelli 1997.

elections are not enough. Machiavelli adds procedures for the popular indictment of officials, popular judgment on many kinds of legal cases, and, generally, interprets the social and political institutions of republican Rome in more direct rather than representative ways. But this does not situate Machiavelli neatly in the camp of substantive or participatory democracy today. Contemporary democrats who focus on civic culture render the minimalist model more substantive by promoting political participation characterized by civility, trustworthiness, deliberation, and reciprocity. Yet I show that Machiavelli's preferred sociopolitical milieu is one of intense socioeconomic animosity and political contestation between elites and the people. According to Machiavelli, elites cannot be made responsive to or held accountable by the people through elections alone; auxiliary governmental institutions that facilitate direct political action and an antagonistic political culture are required as well.

In the first section of this essay I lay out Machiavelli's understanding of the elite-populace relationship in the Roman republic and evaluate his description of Roman political institutions. I then sketch specific aspects of that relationship and the institutions that correspond with, and may perhaps further inform, contemporary democratic theory, in particular the way that the Roman plebs rendered the senate and nobility responsive and accountable. Next, I consider Machiavelli's assessment of the drawbacks inherent in this model: Did the people become too aggressive in their attempt to control elites, such that they brought about Caesarism and ultimately the end of the republic? Finally, I offer some preliminary conclusions on the place of Machiavellian republicanism in the evolution of popular government, its advantages and disadvantages relative to minimalist and substantive conceptions of democracy, and its potential as a resource for contemporary democracy.

MACHIAVELLI, ELITES, AND THE ROMAN REPUBLICAN MODEL

Machiavelli is notorious for advising how to manipulate the people. Indeed, many consider this the main point of his most famous work, *The Prince*. But evi-

dence suggests that he considered his most important and most original piece of advice to be something quite different: how to control elites. Readers of *The Prince* know that Machiavelli advises princes to base their power on the people rather than the elite, the nobles, the "great" (*grandi*) ([1532] 1998, Book IX). He cautions against employing the elite as a base of power because they perceive a prince as merely one among themselves. Consequently, they will dispose of him very readily should he displease them. The people, however, will support the prince as long as he protects them from the elite. They want only not to be oppressed, whereas elites have an appetite to dominate, to oppress, an appetite that is insatiable. The people's desire not to be dominated can be satisfied. Thus, a prince should build his state on those whose demands he can meet.[2]

Machiavelli gives the same advice in *The Discourses* but is more specific about how a prince should treat the elite and secure himself with the people. Machiavelli provides the ancient Greek example of Clearchus (I.16): He came to power through the influence of the nobles, who hoped he could serve their desire to oppress the people. But once secure, Clearchus switched his allegiance to the people and disposed of the nobles by hacking them all to pieces. This imagery is used repeatedly and seems to be Machiavelli's favorite recourse against elites. In at least two other places, in both *The Prince* and *The Discourses*, he recounts with approval how a group of elites is explicitly hacked to pieces ([1532] 1998, XIII) or implies that they should have been (I.27). If a group of so-called nobles or best do not live up to that name, they need to be un-membered, dis-membered, from that association. Since the elite are so consumed with distinguishing themselves from "the multitude," Machiavelli suggests that when they do not justify such a distinction they must be rendered multitudinous—physically. The word with which Machiavelli refers to the nobles, *grandi*, means the great or, literally, the big. When they become too big for themselves, they need to be cut down to size—literally. This gives us an idea of Machiavelli's general attitude toward elites. He resents, despises, and distrusts them.[3]

Nevertheless, the vivid example above is only a last resort for rendering elites accountable in Machiavelli's theory. Much as Machiavelli may delight in the fate of such elites, the causes and consequences of this kind of outcome are precisely what need to be avoided. After all, the principal actor in this case is a prince, and the incident concerns elites who have become irredeemably corrupt. How should elites be controlled, or made accountable, in a republic? What is their place in a regime in which their power is both shared with and perhaps better responsive to the general populace? How should elites be treated when they have yet to become so corrupt? Indeed, a unitary-executive actor who subjugates or eliminates the nobility in the name of the people spells the failure and abolition of republican politics. As an outcome it is advantageous for neither the nobility nor the people, and it is reminiscent of the very development that destroyed the Roman republic: Caesarism. In Machiavelli's analysis, how did the Roman republic manage to distribute power between the people and elites in a manner that, in particular, controlled the latter? And how did the republic do so while staving off the emergence of Caesarism for as long as it did?

Popular Primacy and Machiavelli's Methods

Machiavelli's analysis of Rome is both sociological and institutional. As such, it prompts us to consider that an adequate analysis of popular government must be both. When discussing social class in *The Discourses*, Machiavelli asserts that the nobility ought to hold a diminished, not preeminent, role in a republic (I.5). Ancient wisdom recommended that the nobles be given the upper hand in a republic or mixed regime, a regime in which power is shared between aristocratic and popular elements (see Nippel 1980). Aristotle (1997, 190–1, 94) may have longed for a regime with a middle strata so wide that one could not discern the line between rich and poor, noble and popular, but absent that development, he assigns the lion's share of power to aristocrats in his best regime or *politeia* (1997, Book IV). For most observers, Machiavelli included, Sparta and Venice were the ancient and modern paradigms of this kind of noble-dominated republican arrangement.[4]

But Machiavelli promotes Rome as a model because he understands it to be a popularly dominated republic. Unlike other republics, Rome assigned a special role to the general populace: "the guardians of liberty" (I.5).

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[2] There are at least two persuasive interpretations of these desires, appetites, demands, or what Machiavelli calls humors (*umori*). Parel (1992) interprets them in terms of the effect of cosmological forces on physiological or natural phenomena, and Coby (1999) views them in terms of class motivations. The two interpretations are not necessarily incompatible: Parel privileges the supposed cosmological origins of the appetites that separate segments of society, whereas Coby privileges the actual effects of these appetites, namely, inequalities of wealth and political power. Because I am interested in applying Machiavelli's theory to contemporary debates in democratic theory, I follow Coby in focusing on the political ramifications of the class divisions rather than their origins, which may be more firmly bound to Machiavelli's context. Consult Parel (2000) for the drawbacks inherent in ignoring the relationship of Machiavelli's theory to Renaissance astrophysics or cosmology.

[3] This interpretation of Machiavelli's account of Clearchus and attitude toward elites is justified when we consider how much Machiavelli resented his inferior status in the Florentine republic and, of course, his imprisonment and torture under the Medici oligarchy. Although he was eligible to hold office in the republic that he served faithfully in ministerial, diplomatic, and military capacities

for thirteen years, he was of insufficiently high birth or great wealth to vote on, or stand for, the very best offices in the regime. Consult the excellent political biography by Viroli (2000). As can be seen from Machiavelli's reports to his supposed superiors in the republican government, he often found it difficult to contain his contempt for their arrogance and incompetence. As he writes in a letter from 1506: "Everyone knows that anyone who speaks of empire, kingdom, principate, [or] republic—anyone who speaks of men who command, beginning at the top and going all the way down to the leader of a gang—speaks of justice and arms. You, as regards justice, have very little, and of arms, none at all." Cited in Najemy 1990, 117.

[4] Machiavelli's relationship to the republican tradition both generally and in the Renaissance specifically continues to be a puzzling issue. See Viroli 1990, Nederman 2000, and Rahe 2000.

They are the ultimate arbiters on the freedom of the regime. According to Machiavelli, the people deserve this position simply because they are more trustworthy than the nobility or the great. In accord with the distinction between elite and popular appetites mentioned above, the people will not use such a power to dominate, but only to defend themselves from domination (I.5; I.46). Moreover, since they are less capable of usurping the liberty of a republic than the nobles, they will be more watchful of those who are apt to do so.

I address below more specifically Machiavelli's understanding of how the people exercised the guardianship of liberty in Rome, namely, the manner in which they contained noble ambition. It must be emphasized here, however, that his view is rather different from the evaluations rendered in the very accounts of Roman history that were Machiavelli's sources, as well as from the findings of most contemporary historical research. Polybius (1979, 314–5) emphasizes an equilibrium among social and political forces in Rome; depending on how you look at it, any of the social groups or political institutions in Rome could be considered dominant. Livy's account (1971, 1987) suggests that the Roman senate had the ultimate say, that they were more likely to manipulate the people into doing what the nobility wanted than the people were able to affect the behavior of the nobility. Moreover, historical research emphasizes the oligarchic and timocratic quality of the Roman republic, the dominance of the older and better propertied families (Jolowicz 1967; Nicolet 1980).

Machiavelli was intimately familiar with the ancient accounts and certainly could have anticipated contemporary assessments of power relations in Rome. In light of this, *The Discourses* should be read as something other than straightforward historical-institutional analysis. Rather, it is a combination of historical analysis of what was, in Machiavelli's estimation, the best republic in empirical reality, on the one hand, and a theoretical consideration on what arrangements might improve this particular model, on the other. As a merger of is (or was) and ought, Machiavelli's republicanism should not be read as a mere recapitulation of classical sources, or sloppy history, or an entirely metaphorical exercise. Defying a more recent social scientific imperative to distinguish descriptive from normative aspects of analysis—an imperative whose origin is often credited to Machiavelli—*The Discourses* intertwines the two in a generally suggestive but often analytically frustrating manner.[5]

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[5] Coby (1999) impressively details Machiavelli's faithfulness to Roman history in *The Discourses*, whereas Sullivan (1996) treats the discrepancies in great detail and with considerable care. I do not follow Sullivan, who herself follows Strauss (1958), in attributing these differences almost exclusively to Machiavelli's purported strategy of promoting grand-scale epochal change, that is, the invention of "modernity." I interpret Machiavelli to be engaged in "applied" political philosophy addressed at more mundane practical problems, such as control of elites.

Popular Docility or Ferocity?

Machiavelli wants to show that the people are capable of lively and active defense of their liberty, even if their motivations are fundamentally passive or negative: They want only not to be dominated. But this passive or negative disposition corresponds rather well with the reactive role that classical authors (Polybius 1979, 314) and contemporary historians (Nicolet 1980, 318, 320, 387, 393) ascribe to popular participation in the Roman political process. Moreover, the people's power to ratify policy and select officials but not initiate or formulate policy also conforms well with arbitration theories of popular government (Manin 1997, 47; Wantchekon and Simon 1999). But Machiavelli wants to go farther. Consequently, the passive/reactive versus active/spirited quality of the people becomes somewhat problematic in his account of Rome's development as a republic early in *The Discourses*. How can the people be active guardians of republican liberty but not exhibit the aggressively dominating appetite that is supposedly the exclusive disposition of the nobles?

In Machiavelli's account, the people had to earn their place of prominence in the Roman republic. Neither political founders nor political philosophers had ever granted the general populace such a place. By "people" Machiavelli generally means the plebs, that is, Roman citizens who were not of the patrician class, and excluding such noncitizens as women, slaves, and resident aliens. The interaction of chance and the plebs' own actions—fortune and virtue, one might say—gained the people their prominent position in the republic. Rome was founded as a monarchy by Romulus and the early kings (I.1–2), and it only developed into a republic through the accidents that resulted from the disunion of the plebs and the senate, the people and the nobles (see McCormick 1993). Machiavelli recounts how together the plebs and the senate expelled the kings, but when the senate began to abuse the plebs, the people instituted the tribunes to act in their interest. Machiavelli does not acknowledge that the tribunes were likely selected from the nobles as well as the plebeians.

The tribunes functioned as intermediaries between the plebs and the senate and, most important for Machiavelli, they held back the insolence of the nobles, thus preserving the free life of the republic (I.3, III.11). The creation of the tribunes is important for Machiavelli institutionally and historically. Unlike the general populace in the Spartan or Venetian model, the Roman people actually participated in the emergence and development of a mixed regime by actively helping to eliminate the monarchy and create the tribunate (I.6). They were not assigned their positions by the elites or circumstances, before or after the fact. Popular participation in the development of the republic itself ensured that the nobles did not have an unhealthy predominance of power in Rome.

Livy's accounts of how the tribunes were created and later restored after suspension give credence to the passive, negative, or reactive quality of popular behavior that Machiavelli initially contrasts with aggressive

proactive noble behavior. Machiavelli discusses only the second reinstatement of the tribunes (I.40, I.44), perhaps because the first is considered in Livy merely an allegorical foreshadowing of the second. But both incidents bear mentioning since they confirm Machiavelli's early distinction between the motivations and behaviors of the nobles and the people. In 494 BCE, after suffering the abuse of the nobles upon the expulsion of the kings, the plebs reportedly left Rome en masse (Livy 1971, 141–2). The nobles, fearing for the defense of the city, called them back and agreed to establish the tribunes. In 450, toward the conclusion of the crisis involving the Ten, discussed below, the plebs again repaired to the outskirts and demanded reinstitution of the tribunes (pp. 240–2). On the basis of these examples, we might conclude that, when threatened, the people did not lash out or seek to dominate those who threatened them. Rather, they sought the best way of avoiding domination—they fled or seceded from the city.

In this spirit, the powers of the tribunes, created and restored as a result of these episodes, were, in many ways, reactive or preemptive rather than constructive. The tribunes could veto most official acts through the *intercessio*; invoke the *auxilium*, a form of habeas corpus, on behalf of individual plebs; and could not be touched physically, since their bodily integrity was declared inviolable (*sacrosanctitas*). All these are protections against, or recourse from, aggressive action or encroachment on the part of the nobility or the magistrates.[6] Machiavelli certainly assumes familiarity with these facts on the part of his readers. In particular, he uses the second episode of "secession" to emphasize the distinctive character of the Roman people. But this character transforms from one of initial passivity to one of indignant aggression once the people have suffered abuse by the nobility or other elites.

From the "sacred mount" to which they retired the plebs demanded of the senate not only the restoration of the tribunes but also the execution of those who had offended them (I.44). Machiavelli restates the assessment of the plebs conveyed by observers at the time: The plebs resorted to cruelty in response to cruelty (I.44). Moreover, they freely expressed their violent intentions when threatened, rather than keep them hidden (I.44). But the plebs were advised by friendly nobles to conceal their intentions until these were more readily achievable. Thus, the episode confirms that popular ferocity, in contrast to noble aggressiveness, is reluctant or provoked. Moreover, it also demonstrates that the plebs are guileless: They are incapable of the deception and treachery advised and practiced by the nobility. We will observe below how Machiavelli lauds increasingly aggressive manifesta-

tions of the people's "negative" desire not to be dominated.

The Roman Constitution

Like Polybius, Machiavelli identifies the maturation of the Roman republic with the establishment of its three principal parts: a tamed princely power in the consuls, a somewhat chastised aristocratic power in the senate, and a virtuous popular power in the tribunes. Unlike classical sources, however, Machiavelli understands the most beneficial result of Rome's republican structure primarily in terms of the containment of noble ambition (I.5). But, since Machiavelli is not specific about the functioning of these Roman institutions, some explanation may be necessary. The two consuls were elected annually by noble-dominated assemblies and initially could only be members of the nobility. The consuls were executive magistrates charged with administrative and military duties and might be swayed by either noble or popular influence. The possibility of popular leverage against the consuls increased when the prohibition on class intermarriage was lifted (445 BCE) and when plebs were finally permitted to serve as consuls (300 BCE).

The senate functioned as the more or less direct institutional expression of the nobility. It was ostensibly just an advisory body, although it had substantial fiscal control. Senatorial influence on the election of the consuls, plus the prospect of former consuls joining the ranks of the senate, meant that this body held great sway over the magistrates. The two (and eventually more) tribunes were charged with popular advocacy. They reflected popular preferences but not always directly; Machiavelli notes how they often attempted to act in the interest of the populace against the people's immediately expressed wishes. Thus, even though the tribunes were not always directly or immediately responsive to the people's wishes, in delegative terms they were largely "representative" of them. As Machiavelli asserts, the most important function of the tribunes was to keep noble elites accountable. As bearers of the veto, and chief agents of accusations, the tribunes had the means to block proposals and sanction the actions of consuls or senators. Unlike later versions of republicanism, or more specifically the contract-legitimated arrangements of liberal democracies, the Romans also allowed for the participation of the people in their collective capacity through the council of the plebs (*concilium plebis*). All citizens minus the noble class attended the council, where they decided appeals and accusations, elected tribunes, and eventually made law. This formal assembly grew in importance during the life of the republic. Together with the informally convened deliberating assemblies of the plebs, the *contiones*, these councils presumably constitute what Machiavelli means by "the people" (Adcock 1964; Millar 1998; Taylor 1990).

Rome did not rely extensively on one of the chief mechanisms of elite control in contemporary liberal democracies: the incentive of reelection (Manin, Przeworski, and Stokes 1999, 34). The consuls and tribunes,

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[6] I rely upon Jolowicz (1967) for legal and institutional details of the Roman republic not laid out explicitly in Polybius, Livy, or Machiavelli (1967, 52–3). There is considerable scholarly dispute among historians regarding these facts, and valuable details have simply been lost to history. Elster (1999, 253, n. 1) discusses the dilemmas of analyzing the accountability institutions of Athenian democracy given the lack of clarity over specifics.

like most magistrates, were initially elected for one-year, nonrenewable terms. They could stand for reelection only ten years after the end of their term. What incentives did this provide for magistrates to be responsive to the people, and what account-rendering sanctions could be invoked, if reelection was not an immediate possibility? The public accusation and subsequent punishment of officials, discussed below, were the most powerful institutional means in this regard. Through the power of *coertio* the tribunes could attempt to punish consuls for their conduct in office once their term was over, but this was only an ex post punishment. In fact, the consuls could not be removed during their tenure; except by the dictator under the most dire circumstances. Generally, the expectation that former magistrates would become senators induced a certain degree of good behavior, but good behavior presumably assessed according to the criteria of the nobility. The promise of being accepted by and the hope of getting along with prospective colleagues must be expected to incline a magistrate toward pleasing that set of actors. This is no doubt why the Roman people considered the consuls to be the agents of the nobility and sought to have plebs elect and serve as consuls.

The case of the tribunes is more complicated and more important for Machiavelli. Since they were not officially magistrates, there was no guarantee that they would enter the senate when their terms were over. One might argue that this tended to discourage collusion with the nobles. The opening of the senate to former tribunes roughly coincided with the growing power of the concilium, an assembly in which former tribunes likely had considerable influence. Again, we might speculate that these developments offset each other, such that the tribunes were not coopted by the nobility. In addition, reputation for good behavior in office was important if former magistrates wanted to be considered for special positions in the future, such as the dictatorship. In any case, in the general absence of eligibility for reelection, incentives for good behavior in the republic were largely informal. We should note that Machiavelli never mentions the term limits of the consuls or the tribunes with respect to the inducement of magistrate responsiveness. Perhaps by omitting this fact Machiavelli leaves open the possibility of re-electable and hence more controllable officials in the ideal model that he derives from Roman reality. After all, reeligibility for office was not ruled out necessarily in the republics of his own time.

When Machiavelli does mention term lengths, he condemns their indefinite extension. In his estimation, the prolongation of offices, such as the tribunate and the consulate, contributed to the eventual demise of the republic. He considers the extension of terms to be one of the two causes of the downfall of the republic, along with the crises associated with the agrarian laws, discussed below (III.24). The senate and the people both wanted to prolong office tenure when they thought a person particularly advantageous to their own interests held the position. Generals-cum-tyrants would later use extended tenure to cultivate personal loyalty from increasingly proletarianized soldiers.

Moreover, fewer and fewer people gained experience in governing, which undermined the overall civic culture of the republic (III.24). These conflicts raise a possible solution to the omission of any discussion by Machiavelli of eligibility for reelection in the republic: It was not so important that a particular individual retain an office, but it was desirable that another member of the same class take his place. This emphasizes the primacy of class over individual interest in republican Rome and in Machiavelli's theory.

The Advantages of Social Discord

The combination of extensive participation by the people in Roman political life, on the one hand, and the nobility's need to dominate them, on the other, necessarily resulted in class discord and social tumult. According to Machiavelli, this tension was the principal cause of Rome's greatness: Class rivalry resulted in the active preservation of liberty at home and territorial expansion abroad (I.6). To be sure, he concedes that military expertise and good fortune contributed to Rome's unprecedented, and still unsurpassed, success. But Machiavelli attributes these factors to Rome's order itself, which was "almost savage" (I.4). He recounts with approval how the people protested against the senate, the senate closed down shops, the people called for the exile of certain senators and even evacuated the city. Machiavelli never fully concedes that the senate initiated, and the consuls performed, most of the day-to-day governing of the city, but he does accentuate how the people compelled the creation of favorable laws through public demonstrations and by withholding military service.[7]

Machiavelli argues that Venice and Sparta were successful noble-dominated and domestically tranquil republics because of their size (I.6). Small republics can be sustained without the extensive inclusion of the people; presumably, the proportion of nobles to commoners is so large as to keep the latter pacified. Machiavelli notes how Venice formed its aristocracy before a populace had developed there in any real sense of the term. Thus, the people never had a hand in the formation of the regime as they did in Rome. Sparta neutralized class conflict by maintaining economic equality and tolerating only inequality of status.

[7] The ferocious populism of Machiavelli's republicanism is underestimated in most contemporary interpretations, especially in the reigning Cambridge and Straussian schools. Skinner (1981, 65–6; 1990, 130, 136) acknowledges the originality of Machiavelli's republicanism with respect to social discord, but he interprets this discord in terms of an "equilibrium" between equally dangerous motivations, those of the rich and those of the people. He normatively equates noble and popular motivations in a very un-Machiavellian way, and he renders closed and docile the open-ended, dynamic, and "wild" quality of social discord described by Machiavelli in *The Discourses*. Socioeconomic discord brings salutary results, but these cannot be predicted and certainly are too volatile to be adequately captured by the notion of equilibrium. Skinner is much closer to the Polybian view (Polybius 1979, 317–8) that Machiavelli is attempting to radicalize. In a suggestive interpretation that unfortunately defies standards of falsifiability, Mansfield (1979, 45–8, 152–5) argues that Machiavelli does not really mean what he says with respect to the superior political virtue of the Roman people.

Moreover, the people there had no cause to defend themselves actively against the nobles because the kings took it upon themselves to protect them. Sparta also inhibited the development of a diverse populace by excluding foreigners. Venice undermined social dynamism by prohibiting popular participation in military matters (I.6).

In total, Machiavelli's endorsement of Rome over Sparta or Venice does not disparage the accomplishments of noble-dominated republics. He acknowledges that the latter model may ensure even greater longevity than that enjoyed by Rome (I.2). Sparta lasted 800 years as a republic; Rome lasted only 300. (Indeed, Machiavelli could not have known that the Venetian republic would ultimately last twice as long as Rome.) For Machiavelli, whatever longevity might be gained by the noble-dominated and socially harmonious model is lost in the substance of political culture, the quality of public policy, and the extent of military expansion. These can only be achieved as a result of an antagonistic relationship between elites and populace. Sparta, no matter how successful, was not as great as Rome because it did not have as rich a civic life, and it did not acquire as much empire (I.6). The active civic life enjoyed by Machiavelli's (perhaps romanticized) popularly based Rome is not—contemporary neorepublicans and communitarians take note—a peaceful, bucolic, or tranquil arrangement of social interaction.[8] Although Machiavelli never makes the distinction, discord seems to be good for two reasons—as a preferred way of conducting public life and as a means to better policy and military success.

Machiavelli's emphasis on antagonism or discord does not mean there was no place for political cooperation in Rome. The tribunes and the senate could act together: For instance, Machiavelli admires the way they could compel the two consuls to agree when in discord (I.50). He views intrainstitutional conflict as harmful, unlike cross-institutional discord, which is beneficial. He asserts that one institution should never possess the sole authority to perform a function in case it should try to be obstructionist. There should always be other means, perhaps more arduous, of performing a task, such as distributing honors and rewards, or creating a dictator. According to Machiavelli's account, Rome practiced not only what would come to be called a separation of powers but also a rudimentary form of checks and balances (I.50) (Manin 1994).

The possibility of institutional cooperation apparently motivated by the common good, as illustrated by many accounts of positive interaction between the people and the nobles discussed below, casts doubt on the sustainability of Machiavelli's distinction between the motivations of the two classes. Machiavelli draws the distinction so sharply that his subsequent examples

raise a series of questions: What would make elites who rise from the ranks of the plebs interact with those who come from the nobility? Or do plebs become "elites" once they ascend to power, such that they develop an appetite to dominate, a will to power, that makes them part of the nobility? If this is so, then the distinction between classes would revert to an argument about the opportunity to rule, rather than a question of disposition to rule. On this basis, Machiavelli would be suggesting that the people are virtuous only when they are not ruling, or only when they merely aspire to rule, not when they actually participate in governing. Once they gain power, we might presume that they behave like nobles.

One way to avoid or minimize such difficulties would be to distinguish between socioeconomic and political elites in the following way: Nobles who hold office behave like socioeconomic elites who seek to lord their privilege over others and pursue a particular class agenda. Plebs might be conceived merely as political elites who exercise the power of their office to protect their class and the overall good of the regime. But, as I suggest below, a distinction between socioeconomic and political elites is not sustainable: In republican Rome, and in our own time as well, the former always exert excessive influence on the latter. Therefore, political elites need to be treated with the same distrust as socioeconomic ones, no matter the class from which they emerge.

Moreover, if Machiavelli's initial distinction is to have any teeth, then there should not be any displays of good behavior on the part of the nobility. His distinction gives no account of the appearance of populace-friendly nobles throughout the history of the republic—not only those who would exploit the people in Caesarist schemes, but also those who apparently have the people's best interest at heart. Why do the nobles sometimes exhibit a capacity for moderation and compromise (as shown below)? Why do tribunes faithfully protect the people if they themselves are members of the nobility or once they become political elites? We must conclude that some nobles are capable of resisting their desire to dominate the people.

Machiavelli may be exaggerating rhetorically when distinguishing between the people and the nobles in a way that even his own account cannot sustain. His intention may be to obliterate any vestige of the classical legacy that attributes good motives to the nobility a priori. There is no categorical distinction in Machiavelli's work between aristocrats and oligarchs, such that the latter may take refuge behind the illusion of being the former. His approach seems to assume that all elites are bad. By starting from there, Machiavelli justifies three things: the populace's unqualified preeminence over the nobility in his model; the rather nasty relationship between the two classes; and greater vigilance over the nobles than might otherwise be required. If the nobles rise above Machiavelli's negative description of them—that is, if it turns out there are good and bad nobles, or aristocrats and oligarchs—so be it. But republicans will no longer be put

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[8] I do not mean to imply that there is no emphasis on conflict, contestation, or "agonism" in contemporary, especially poststructuralist, critiques of liberal democracy (see, e.g., Connolly 1995; Honig 1993; Mouffe 2000; Young 1990). While largely unconcerned with control of elites, this literature promotes conflict over identity "recognition" rather than economic "redistribution." For critical evaluations, see Barry 2001; Benhabib 1996; Fraser 1997.

at a disadvantage by claims on the part of "the best" to have better insight into the common good.

Whatever the contradictions within his "class analysis," which are still to be sorted out, Machiavelli claims that the ambition of the nobility would have corrupted the republic long before the people themselves were corrupted, or long before the regime as a whole corrupted itself. In other words, elites left to their own devices cannot manage themselves and thus are a danger to themselves and their regime. Contrary to conservative "wisdom" on the nature of the "masses," Machiavelli asserts that it is not the people who have base and unlimited desires. He interprets Roman history to suggest that the people are more keenly aware of their own deficiencies than are the nobles and are more inclined to the common good of the regime. Contrary to later republican practice, and especially the practice of liberal democracy, Machiavelli suggests that a direct manifestation of the people within government, alongside a representation of them, is necessary to carry out successfully an appropriate patrolling of elites. Whereas most classical political science, conservative and liberal, is concerned with controlling the people—either first and foremost, or with equal vigilance devoted to elites—Machiavelli gives highest priority to the control of elites.

In short, Machiavelli's theory may omit the criteria by which we might distinguish, on the one hand, elites who exert power with the motivation to dominate and, on the other hand, those, whether tribunes or well-meaning nobles, who do so only to participate in the people's effort not to be dominated. But whatever the origins of his theory of the respective appetites of the elites and the people—whether it be cosmology, capabilities, or something else—and whatever the difficulties in demonstrating how the dominating appetite obtains in reality—that is, how to explain good nobles and ferocious plebs—I argue that the theory is nevertheless an excellent "as if" proposition for contemporary democratic theory. Ascribing to the people a desire not to be dominated prioritizes as more just their desire to be free, that is, undominated. At the same time it facilitates the people's active contestation of elites lest their own liberty be threatened or eliminated. We might say that Machiavelli's theory legitimizes the people's "natural" disposition of passivity and also justifies an "unnatural" active political posture. Conversely, the assumption that the elite appetite to dominate is insatiable, whether this can be demonstrated as true in every case, necessitates extraelectoral safeguards against them, such as accusations and plebiscites, and beyond these, it promotes participation that is not only active but also antagonistic.

Before proceeding any farther, however, it might be asked: Should we understand "elites" as the same thing in Machiavelli's context and in our own? Is it appropriate to think of them as having anything in common, such that we can draw insight from Machiavelli for our contemporary circumstances? After all, in Rome there was little or no distinction between political and socioeconomic elites: The senate was effectively the nobility. Contemporary liberal democracies exhibit a more dif-

ferentiated socioeconomic circumstance in which political elites are functionally and often socially distinct from economic ones. Theoretically, these political elites, seeking votes, may serve the poorer or, more likely, middle classes against the wealthy classes. Yet, one need not invoke Marx to observe that socioeconomic elites very often still are the political elites or, in any case, control the latter to such an extent as to render the distinction problematic (see Domhoff 1998; Lindblom 1977; Mills [1956] 1999).

Machiavelli would have considered the institutions of contemporary liberal democracy elective oligarchy and would have found its social bases insufficiently antagonistic along class lines to make up for these institutional deficiencies. Whether contemporary political elites act on their own motivations to dominate or merely carry out those of the economic elite (again, they are quite often one and the same), Machiavelli shows us that elections are an inadequate means to direct, control, and curtail their behavior. Indeed, as we will see, as far as Machiavelli is concerned, public accusations and popular appeals are inadequate as well; agents of will to domination, such as the Roman nobility in the past, or corporate magnates, entrenched bureaucrats, and government officials today will generally find ways to circumvent them. Therefore, widespread antielitist antagonism is necessary as well.[9]

Exactly how do Machiavelli's injunctions to subordinate elites through popular means obtain in Roman reality and/or in Machiavelli's prescriptions? The next section critically catalogs the many specific qualities attributed by Machiavelli to the people that contribute positively to a republic, such as their fairness in distributing offices (I.47), their justice in deciding cases of public accusations (I.7), and their ability to recognize the best argument from among public speeches (I.58). Generally, he identifies in the Roman people a peculiar capacity that corresponds neatly with the function of the electorate in minimalist theories of democracy. What he adds to these theories is a more animated and participatory quality in the selection and control of elites. Concretely, this means ensuring some active governing role for the people in their collective capacity, even if most popular control of the nobles is exercised through the more representative and reactive institution of the tribunes.

INDIRECT AND DIRECT POPULAR MECHANISMS FOR CONTROLLING ELITES

Consider again the differences between actual Roman practices as we know them and Machiavelli's somewhat

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[9] Although Machiavelli shares Marx's indignation over elite power and class subordination, he does not think that elites as a group can be eliminated or classes in general overcome (Marx [1848] 1996). In this sense, Machiavelli anticipates his fellow Italian theorists of the "iron law of oligarchy" (Michels [1911] 1990; Mosca [1896] 1980; Pareto 1987). But unlike them, and the Schumpeterian democrats with whom they have so much in common, Machiavelli does not provide elites the expansive space within which to carry out their domination of the rest of the people by celebrating their supposedly inevitable and irresistible ascendance. For critiques of elitist democracy, see Bachrach 1967, Skinner 1973, and Shapiro n.d.

free interpretation of them. A few general issues demonstrate Machiavelli's normative preference for a republic more extensively inclusive of the populace, a republic that allows greater direct popular control of policy formation, lawmaking, and magistrate activity. His republic fosters the expression of a popular will not always mediated through representatives or curtailed by exclusionary procedures. In this regard, Machiavelli never mentions the Roman practices of weighing and ordering votes in ways that favored the better propertied in such assemblies as the *comitia centuriata* and *comitia tributa*. In addition, he fails to acknowledge how difficult it was for plebs to gain office. Instead, he consistently expresses his admiration for their desire to hold an increasing number of offices and for eventually winning the privilege to do so. Although the nobility held vast agenda-setting power over issues that were presented to the people, Machiavelli emphasizes the way that the people could influence what elites initiated.

Machiavelli has a habit of speaking of the populace at large when describing the functioning of specific popular assemblies. He remarks that the tribunes proposed laws before "the people," who could speak out one by one for or against them (I.18). More accurately, the tribunes conducted wide-open popular deliberations in bodies known as contiones, which could not enact law. The republic maintained a strict separation between deliberative and legislative bodies. The plebs eventually obtained lawmaking power for their assembly, the concilium plebis, but this is a much more complicated story than Machiavelli lets on. The concilium excluded the nobles, began life outside the boundaries of official politics, and gained parity with and ascendancy over other institutions only with great difficulty and only very late in the history of the republic. Machiavelli speaks as if the law produced by the concilium, the *plebiscita*, was always generally applicable throughout Rome. Actually, the plebiscita originally applied only to the plebs, may in fact have required senatorial confirmation, and was extended over the whole population rather late, in 287 BCE.

Machiavelli devotes most specific attention to the institution of public accusations, presumably because it was most democratic. Any citizen could level a public accusation against another, especially a magistrate. But for reasons discussed below, this may have been the least attractive popular institution in the Roman republic by contemporary standards. Finally, I will observe that Machiavelli's mode of interpreting Roman history places him in a particularly awkward position: By demonstrating what he sometimes calls the many "sins" of the nobles, he often reveals how successful they were at manipulating the very people whose virtue and talents he generally extols. Yet, when he focuses on the spirit of the people, he is forced to raise the specter of the "popularly legitimated" way that the republic was eventually destroyed.

The Beneficial Collective Action of the Populace

Recall that Machiavelli claims the people have less desire to usurp freedom than do the elites and adds the

qualification that they also have less "hope" of doing so (I.5). The populace has neither the inclination nor the ability to threaten a regime. Devoid of elite direction, in fact, the people are "headless" and thus harmless left to themselves (I.44). They are weak and cowardly when isolated, thinking only about their own individual fears, but they are mighty when united under leaders (I.57). The ideal arrangement is one in which elites govern but are prevented from manipulating the people into helping them carry out their more sinister designs. The tribunate is the institution that generally serves as a "head" for the people; it is responsive to popular concerns except when the people seem unreasonable. It also serves as a direct check on noble machinations to enlist the latter in uncivil schemes.

Machiavelli also observes that the people recognize the truth in assemblies (I.4). This implies that they are able to choose the better arguments among elite proposals, whether submitted by the consuls in the noble-dominated comitia or by the tribunes in the concilium and those aired on their own in the contiones. Machiavelli understands the popular ability to discern better policies in terms of their desire not to be dominated. An elite proposing a policy may have a hidden and self-serving agenda, but the populace evaluates it to see if it conforms with the common good. A cynic might wonder whether this aptitude resulted from their legal exclusion from full participation in government: They developed a capacity to select policy precisely because they were cut off from making it. This may be largely true, but we can think of the popular element within mixed government in Machiavelli's formulation as itself a mixture of direct participation and popular representation, such that the people do make policy. Machiavelli seems to read back into the early republic a more general directly popular element from its middle and late periods. In this way, he may exaggerate the policymaking powers of the plebs. The fact that he seldom specifies whether the assemblies of which he speaks are the wealthy-dominated comitia or the exclusively plebeian concilium further blurs the issue.

Popular Distribution of Offices

Machiavelli argues that the people are better than elites at distributing offices: "A prudent man ought to never depart from the popular judgments especially concerning the distribution of ranks and dignities, for in this only does the people not deceive itself. If it does deceive itself at some time, it is so rare that the few [i.e., the nobles] who make such distributions will deceive themselves more often" (I.47). The Roman populace did not fully govern itself as do the people in a simple democracy, but it did select the officers who ruled better than the candidates for those offices would have if left to choose among themselves. As we will see, Machiavelli's Roman examples show that the nobles could be confident that they would be given offices by the people when they were qualified, and even when plebs themselves were eligible for the same offices (I.47). According to Machiavelli's account, the people chose to let nobles serve as magistrates; they were not,

as empirical evidence generally suggests, compelled legally to do so.

Machiavelli explains that elites have no humility regarding their governing abilities, and they seldom defer to other elites and never to the populace when it comes to officeholding. They must be forced to do so by placing the distribution of offices in the hands of an arbitrating general populace. The comitia selected magistrates, such as the consuls, and the concilium chose such officers as the tribunes. Machiavelli notes that the tribunes mediated relations not only between the nobles and the people but also among the nobles themselves (I.50). When senators or consuls could not reach agreement they were known to consult with the tribunes. Thus, the people arbitrated among elites at two levels: not only by selecting officers but also by having their own officers actively mediate conflict among the elites while in office.

Machiavelli acknowledges that when disputes over offices between the nobility and the people become especially intense, autocratic means of distribution have an allure of efficiency. It is important to emphasize here that he specifically rules out empowering one, purportedly neutral, person to split the difference between the nobility and the plebs in such disputes (III.34). Selection of officers by a unitary executive is as bad as, or worse than, the selection of magistrates by nobles themselves, and it is certainly inferior to popularly inclusive methods. Machiavelli claims that the people are a better distributor of offices than a prince, because they base decisions upon a candidate's good reputation until they learn otherwise from his deeds. Unitary executives, in contrast, tend to fear a man of good reputation as a rival. Furthermore, they are inclined to remain stubborn about their decisions on such matters, but when the people lean toward an inappropriate choice they can be dissuaded by good arguments and a trustworthy speaker (III.34).

Machiavelli adds that a good populace does not allow officials to get away with bad behavior just because they have performed their duties well in the past. This prevents certain figures from becoming excessively insolent. The best method for preventing such insolence is the subject of the next section. Finally, Machiavelli insists that the people are inclined to give rewards, even if from meager resources, for good service (I.24). Thus, magistrates, even without the promise of reelection, may be induced to good behavior by the expectation of monetary or honorific rewards from the people.

Accusations, Calumnies, and Capital Appeals

Machiavelli treats the institution of public accusations as a direct form of popular participation, one that renders all citizens, but especially elites, accountable (I.7). He identifies this as the best instrument for guarding freedom in a republic. In Rome, anyone, but most often the tribunes, could accuse citizens before the general populace or before a diverse institutional body. In Rome, as in democratic Athens, accusations

could be leveled for political plans or proposals as well as concrete actions and on grounds of malfeasance as well as treachery (see Elster 1999). Fear of public exposure was as much a deterrent as exile, imprisonment, and fines. In particular, Machiavelli admires the way that accusations crushed uncivil action instantly and "without respect" (I.7). Since the threat of electoral sanction has much less force when reelection is not likely, accusations are an efficient and relatively immediate way of holding elites to account. Most of the nobility could be targeted at any time, and magistrates, such as the consuls, had immunity for only a year or less.

In Athens, direct democracy and the practice of lot rendered class divisions relatively less salient in government. But in the mixed Roman regime, magistrates might not be inclined to express the interest of the general or poorer portion of the populace, which necessitated additional and immediate means of compelling them to do so. Along these lines, Machiavelli believed that accusations have a benefit beyond deterrence and punishment; they provide an outlet for the ordinary venting of social "humors" generated by class antagonism (I.7). If such conflicts are expressed extraordinarily, that is, extralegally, they bring republics to ruin. Machiavelli is adamant that the "alternating humors" of the people and the nobles should be ordered through laws, such as those providing for public accusation (I.7). We again observe the entwinement of social conflict and institutional design in Machiavelli's theory.

The importance of accusations is illustrated by Coriolanus, who was compelled by the tribunes to reveal and explain publicly his plan to starve the plebs into submitting to the nobility (I.7). Were it not for this display, Machiavelli suggests, the plebs would have killed him immediately outside the senate, which would have set in motion a disastrous chain of events resulting in excessively violent class warfare. Machiavelli complains that in his own republic of Florence there was no way for the multitude to "vent its animus ordinarily" against a citizen (I.7). He claims that the Florentines should have been able legally to check an ambitious, audacious, and spirited would-be usurper such as Francesco Valori, but they were forced to deal with him extraordinarily. That led to the development of factions on both the popular and noble sides and the elimination of many nobles rather than just Valori, who was the guilty one (I.7).

The specific case of accusations raises the general issue of the requisite size and diversity of an effective political arbitration body. Machiavelli points to a Florentine example to show that an appellate body must be sufficiently large and diverse, even if it cannot encompass the whole populace: Piero Soderini was accused before a body of only eight citizens, an insufficient number in a republic (I.7). The judges need to be many because "the few always behave in the mode of the few" (I.7). An insufficient number will reflect only the interest of some elite group and cannot arbitrate fairly, that is, outside their own interest. Machiavelli asserts that if the Florentines had been

able to judge Soderini institutionally, the Spanish army need not have been brought into Italy to settle matters definitively. This event led to the restoration of the Medici family and the demise of the republic in 1512.

There is, for Machiavelli, an inverse relationship between the ability to appeal to a sufficiently large and preferably diverse domestic body and the necessity to appeal to foreign arbitration. Unlike the Florentine parties, who had imperfect internal institutional recourse, neither the senate nor the plebs in Rome ever availed themselves of foreign forces (I.7). Machiavelli adds that there is no need to worry that accusations will be made casually so long as accusers fear being indicted themselves should the charges be revealed as frivolous. Thus, Soderini's accusers would not have acted casually for fear of being accused themselves.

Later designers of popular government dispense with the practice of accusations because of the demogogic or factional excesses to which they believe it tends. Whatever retribution may await someone who levels a frivolous charge, accusations could be leveled strategically at certain times to prevent particular policies from taking shape and/or being enforced. Moreover, charges that may never be definitively proven might still smear or damage a public official. Machiavelli distinguishes between accusations and calumnies, which are frivolous charges leveled anonymously and unconfirmed factually by witnesses (I.8)..He focuses on the example of Marcus Manlius, who was jealous of the glory that Camilus gained by defeating the Gauls (I.8). Manlius spread rumors that Camilus hoarded war booty for himself rather than use it to alleviate the economic burden of the plebs. The senate was forced to appoint a dictator and confront Manlius. Had his charges been made through official channels, publicly, and supported by witnesses, the senate would not have needed to resort to more drastic measures to address them. Thus, high standards for evidence and widespread publicity are antidotes for calumnies.

Machiavelli asserts that calumnies only cause anger rather than punish citizens legitimately. They often enable demagogues to exploit the people's prejudices against the nobles in an illegitimate and unhealthy way, whereas accusations always serve the republic as a whole. Machiavelli refers to the contemporary Florentine example of Giovanni Guicciardini, who was accused of accepting bribes from the Lucchese to refrain from attacking their city. If Guicciardini could have appealed to the people, instead of the chief executive of Florence alone, Machiavelli argues, he would not have mobilized the impassioned partisanship of the nobles in his cause (I.8). This suggests that, despite class animosity, the general public will give a noble a more fair hearing than a magistrate acting in the name of the people. The Florentine republic, which Machiavelli served as citizen and official, is consistently criticized in *The Discourses* as insufficiently equipped to accommodate popular necessities and is the constant foil for Rome's greatness. In these examples, Florence is guilty of being susceptible to calumnies, not allowing popular arbitration of accusations, and inviting foreign powers to settle domestic disputes.

Machiavelli also admires the Roman practice of placing the final decision over capital execution in the hands of the people. Again, he makes no distinction between the people as a whole and their assemblies, or among the very different kinds of assemblies in Rome. It appears that capital cases were tried at various points before the more oligarchic comitia centuriata and eventually the more popular concilium plebis. It is not clear to which of these assemblies the "appeal to the people," or *provocatio*, was directed specifically. In any case, capital cases are especially important in a mixed regime: The people are inclined to interpret a death sentence against one of their own as an act of class oppression by the nobility. They need the opportunity to overturn or reduce such sentences, and in Rome they could do so in a variety of ways. Capital sentences pursued or rendered by consuls, the comitia centuriata, and even, after the fact, the dictator might be overridden or commuted to exile by invocation of the provocatio and/or the decision of the concilium.

Finally, for Machiavelli, the imperative of efficiency is no argument against these kinds of popular arbitration mechanisms. In Rome, if the hearing of popular accusations or the appeal process in capital cases proved to be too slow for especially pressing cases, the consuls and senate appointed a dictator to handle the matter.[10] But never did they enlist a foreign power. Thus, for Machiavelli, time constraints may be factored into the deployment of popular arbitration mechanisms: Neither adhere to them so firmly that the general security of the republic is put at risk, nor use expediency as a pretext for invoking foreign intervention (Wantcheken and Nickerson n.d.). Whatever the institutional specifics of the Roman practice, Machiavelli laments the fact that Florence put accusations in the hands of the elite and capital appeal in the hands of purportedly objective foreigners (e.g., the pope, the king of France). In reality, elites and foreigners are easily influenced and corrupted by particular interests in the city (I.49).

Elite Persuasion or Manipulation of the People?

Machiavelli does not suggest that the people's ability to discern political reality is always clear and prudent or that the nobility's inclination to show them what is in their best interest is in all cases malicious. The Roman nobility often misled or manipulated the people in Machiavelli's account. In one instance, fear of the gods was used to frighten the people into electing nobles as tribunes (I.13). On another occasion, when the nobles

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[10] The Roman dictatorship was a temporary emergency measure to preserve the republic, not a permanent authoritarian opposition to institutional diversity and turnover of offices. On the distinction see Rossiter 1948 and McCormick 1998. Twentieth-century analysts of Roman dictatorship emphasize that it was a device by which the senate and consuls brought the plebs back into order (Fraenkel 1969, 10, 213; Kirchheimer 1969, 42). This charge must be taken seriously, since the dictator was appointed without the consultation of popular institutions and in practice temporarily revoked the popular right of appeal.

were forced to give the plebs a stipend to march far afield and besiege towns for long periods, they made this appear to be a result of their own magnanimity rather than sheer necessity (I.51). The people rejoiced with gratitude for the nobles, even though the tribunes argued that it would mean higher taxes. Machiavelli recounts how the senate often manipulated the people into letting nobles keep positions that the plebs wanted (I.48), usually by putting first-rate nobles or second-rate plebs on the slate. The plebs would defer to the excellence of the former or would be too ashamed of the incompetence of the latter to select them. This example betrays a certain gullibility on the part of the people but confirms their virtue: They do not see through the nomination strategy but cannot bear to elect inferior magistrates—especially if they will reflect poorly on their own class.

Yet, the nobles do not have the monopoly on unfair or dangerous intentions. Machiavelli notes that the people sometimes desire their own ruin when they are deceived by false conceptions of the good (I.53). For instance, the plebs wanted to move half the population of Rome to Veii as a way to maximize the city's wealth in their favor (I.53). Machiavelli reports that the nobles would rather have suffered death than accept this alternative. He does not say why, but presumably this eventually would have created a city to rival Rome. But the people were so enthusiastic about the idea that they would have obliged the nobles with death had the latter not used "as a shield" some old and esteemed citizens for whom the people had deep respect. Despite their animosity for the nobility as a whole, they had faith in and reverence for some of them. We only learn these serious qualifications of Machiavelli's initial characterization of noble malice and intransigent class hatred in the course of his narrative.

Machiavelli maintains that the people can be misled not only by false notions of material gain but also by an overly active spirit (I.53). For instance, they decried as cowardice Fabius's moderate strategy during the Punic Wars, a strategy that Machiavelli endorses. Consequently, they risked a crushing Roman defeat at Cannae under the more reckless leadership of Varro. The senate acquiesced to a similar overly aggressive policy proposed by Penula because it feared an uprising by the people, who were always suspicious of weakness in the face of Hannibal (I.53). In other words, the people could coerce the senate into conforming its agenda to the popular will. The senate could not dismiss Scipio's ambitious appeal to the people for an invasion of Africa for similar reasons (I.53). Machiavelli adds the contemporary example of the Florentines, who were mistaken about the conquests of Pisa by Ercole and Soderini (I.53).

All these examples illustrate how the people can be misled in dangerous ways by the seductions of grand enterprises. This, in and of itself, is less unsettling for Machiavelli than the popular response to the resulting failures: He notes that when such enterprises collapse, the people do not blame fortune or incompetence but the purported malice of their leaders. They may imprison or kill them, irrespective of past success, as did

the populace in Carthage and Athens (I.53). It is nevertheless unsatisfying that Machiavelli makes no attempt to reconcile these tendencies toward excess with the passivity and benevolence, or even the provoked or defensive ferocity, that he attributes to the people earlier in *The Discourses.*

Machiavelli also cites numerous episodes in which both the people and the nobles demonstrate their virtue in the midst of crises: Esteemed senators often persuade the people not to follow a course ruinous for them and the republic (e.g., I.53). During the Punic Wars, the plebs of Capua weighed the option of killing their entire senate and restaffing it from their own ranks (I.47), but they demurred when presented the concrete opportunity to do so by Pacuvius Calanus (Calavius). They even laughed at the prospect of filling senatorial roles themselves (I.47). In the same chapter, Machiavelli describes how the Roman plebs wanted to seize consular power for themselves on the grounds that they were greater in number, fought the wars, and protected freedom. But when it came time to supply individuals for these positions, they selected nobles because they were better qualified (I.47).

In other words, the people have a sense of their own limitations and their need for the governing expertise of their class antagonists. As Livy (1971, 277) notes, the people really wanted the opportunity to stand for office, even if they ultimately decided to choose nobles. In one sense, this conforms with modern liberal democracy—everyone is nominally eligible to run but few are interested in doing so, and the "best" of those so inclined are selected. In another sense, in contrast to contemporary representative democracy, the Roman citizenry at large may have been eager to hold office. Unlike later consent or contract models, Machiavelli's model seems to suggest that class antagonisms and the genuine possibility of direct participation inspired enthusiasm among the people.

Machiavelli reinforces the argument concerning the populace's good judgment with ancient and contemporary examples of a change of mind for the better. The people of Rome and Florence eliminate certain institutions after wrongly blaming them for mismanaged war efforts (I.39), but both groups demonstrate the ability to learn by later restoring these very institutions. Machiavelli also notes that the plebs ultimately refused Spurious Cassius's attempt to gain their allegiance by distributing enemy property among them (III.8). They were not yet corruptible and susceptible to such "Caesarist" temptation. They also condemned Manlius Capitolinus to death for similar reasons; in fact, the people, the tribunes, and the senate all resisted powerful inducements to help him (III.8).

There is an obvious danger that great citizens in a republic will put their skills to less than republican ends. Nevertheless, "a republic without reputed citizens cannot stand, nor can it be governed well in any mode" (III.8). But such reputation can serve as the genesis of tyranny. Machiavelli's proposed solution is to favor reputation earned for public goods over those earned for private goods (III.28). Yet, it is not clear how this criterion rules out, for instance, a Julius

Caesar, who gained a reputation precisely for "public" goods, such as military glory and economic redistribution.

DRAWBACKS OF POPULAR MECHANISMS FOR CONTROLLING ELITES

Machiavelli understands appropriate popular ferocity to be any animosity toward elites that stops short of enlisting either a Caesar or a foreign power to subjugate or dispose of them. The Roman people never resorted to the latter but ultimately succumbed to the former. This section is devoted to moments in Machiavelli's *Discourses* when the people of Rome flirt with abandoning their methods for controlling elites or seem to go too far in pursuing class conflict against them. The crisis associated with the Ten raises institutional questions, and that of the agrarian laws raises issues of political culture. Both episodes draw attention to the circumstances that established the precedent for the collapse of the republic and the emergence of Julius Caesar. It is important to note that the Roman populace undermined its methods of elite control because of animosity for the nobility. Contemporary civic-culture or neorepublican critics of minimalist democracy complain about the apathy toward public matters that might eventually corrode and corrupt popular institutions and perhaps lead to unfree results (e.g., Barber 2000; Putnam 2000). In any case, the issue of Caesarism continues to be a serious one. Tocqueville, most notably, pointed out the danger of modern populaces that become so ferocious in their quest for equality that they endorse militarily or plebiscitarily legitimated tyrants (de Tocqueville [1848] 2000). Tocqueville had in mind a Bonaparte, but we could imagine worse (see Baehr and Richter n.d.).

The Temptation to Forsake Institutions for Controlling Elites

Machiavelli recounts an alarming instance when the people of Rome temporarily abandoned key mechanisms by which they kept elites responsive and accountable. Because of the "disputes and contentions" between the nobles and the people, Rome tried to imitate Athenian law, and created the Ten (the *decemvirate*) to codify Roman law (I.40). Livy explains that the institution of the Ten and the legal reforms that they set out to enact were motivated by the popular desire for greater participation and equality: The people wanted a system of laws "which every individual citizen could feel he had not only *consented* to accept, but had actually himself *proposed*" (1971, 221, emphasis added). The people no longer wished merely to acclaim or consent to law that was presented to them but wanted to take part in its formulation. Machiavelli never impugns their motivations but does express concern over the attempted means for realizing them. The Ten diminished the power of both the tribunes and the consuls and abandoned direct appeals to the people (I.40). This streamlining of institutions appears to be a historical regression from the perspective of republi-

canism, for it undermines the institutional diversity of mixed regimes that had developed over time in the ancient world and Rome in particular. The results hearken back to the kind of political corruption inherent in the regime types famously described by Polybius, and they anticipate the simple people = one-man arrangement of Caesar's tyranny.

Machiavelli describes how Appius Claudius became the leader of the Ten through popular consent, even though formerly he had been quite cruel to the people (I.40). The nobles tried to curb his growing power but to no avail. Eventually war and Appius's crimes restored the authority of the nobles, who initially would not eliminate the Ten. They preserved the institution in the hope of forestalling indefinitely the reestablishment of the tribunes. It is noteworthy that Machiavelli focuses on the bad behavior of the nobility in this instance, although there is quite a bit of unruly behavior on the part of the people to consider (Livy 1971, 236–49). After the second evacuation of the plebs from the city, Appius is arrested and commits suicide, the Ten step down, and both the consuls and tribunes are restored.

Machiavelli finally concedes, on the basis of these events, that the people are a better support for tyranny such as Appius's than the nobles because they provide the potential for more violence (I.40). But for Machiavelli, despite its dangerous implications, the episode illustrates how the mechanisms of popular participation in Rome worked well in both containing the nobles and preventing the rise of demagogues (I.40). He argues that a magistrate created by the people needs some institutional cause for hesitation about becoming a criminal; there needs to be recourse to modes of accountability such as the consuls, tribunes, accusations, or the popular appeal. But these are precisely the institutions that were suspended with the consent of the people. With these out of the way, there was no effective way to check someone like Appius, who would exploit the people's animosity toward the nobility. Short of war and Appius's excessive behavior, the nobility themselves could not touch him. Machiavelli suggests that everyone realized these institutions ought not to be suspended or abandoned so readily in the future. Because the nobles and the people wanted so badly to remove the institutional agents of their rivals, they were tempted to eliminate the very buffers that prevent tyranny (I.40). Machiavelli presents the episode as an anomaly in the history of Rome, even as it foreshadows the republic's ultimate demise. But it is hardly an isolated case, as we will see.

The Agrarian Laws

Early in *The Discourses*, Machiavelli remarks that "every city should have modes by which the people can vent its ambitions" (I.4). Neither are the people always so passive nor are their desires so inherently benign as a superficial reading of his account might suggest. He admits that one advantage of noble-dominated republics like Sparta and Venice is that they keep authority away from "the restlessness of the plebs that causes

infinite dissensions and scandals" and that wears down the nobles and makes them "desperate" (I.5). Once the plebs were granted the tribunes, they wanted one and then both of the consuls, as well as all kinds of other magistrates.

Is this a repudiation of Machiavelli's ascription of a benignly passive disposition to the people? Or are these merely manifestations of the defensively ferocious posture of the people? Machiavelli later describes how difficult it is to distinguish aggressive behavior that is appropriately defensive from that which is dangerously offensive (I.5). In either case, the people's appetite not to be dominated by the nobles, combined with their attempt to seize institutional guarantees against the latter, eventually leads to Caesarism and the downfall of the republic. Machiavelli recounts how the plebs "furiously" began to adore men such as Marcus Marius, who could beat down the nobility, and thereby hastened the ruin of the republic (I.5). He notes that the two Marcuses were made, respectively, dictator and master of the horse by the people to surveille ambitious nobles (I.5). Thus, the people were not satisfied with simple freedom from oppression; or rather, it is not easy to guarantee "negative" freedom from noble domination without the "positive" freedom exercised by securing legislation and offices against the nobles. Machiavelli points out that the establishment of the tribunes and accession to the consulate were not enough for the plebs; they also wanted to share in the honors and spoils of the nobility (I.37).

In particular, the agrarian laws of the fifth century threatened the nobles by limiting the amount of land one could own and by distributing among the plebs land seized from vanquished enemies (I.37, III.24–5). This legislation enraged the nobles, for it sought to take what they already had and denied them access to the means of getting more. Machiavelli claims that the senate responded by sending armies farther afield to places the plebs would not covet, thus ensuring that these spoils were the exclusive pleasure of the nobility. Machiavelli need not point out that this practice leads to Caesarism as steadily as does the popular worship of "one man" who will beat down the nobles. As the armies were sent farther and farther from Rome, generals and not the republic began to take responsibility and credit for the army's material sustenance and hence commanded its ultimate loyalty. According to Machiavelli, were the nobles or the plebs more responsible for laying the groundwork of Caesarism?

Machiavelli emphasizes that the agrarian laws led to a cycle of excessive disorder: civil conflicts, recourse to "private remedies" by individuals, the establishment of party heads (e.g., Marius for the people, Sulla for the nobles), and ultimately more blood and violence than is healthy for a well-ordered republic (I.37). The nobles initially gained the upper hand, but the way was established for a popular party leader such as Caesar to emerge as tyrant. Yet, Machiavelli does not condemn the people through these examples; he concludes that the nobility caused the agrarian law crisis and the 300-year decline that it set in motion (I.37). The

ambition of the nobles needed to be checked and would have brought Rome down much sooner if the people had not sought to halt them. According to Machiavelli, it was the nature of elites to behave in such a way as to provoke the people to undertake harmful measures like the agrarian laws. Therefore, we must understand popular ferocity as the righteous indignation of a normally passive inclination not to be dominated that has been violated, abused, and threatened. The people's aggressive behavior is revealed to be a legitimate response to the nature of elites and their inevitable behavior.[11]

MACHIAVELLI'S HISTORICAL PLACE AND RELEVANT LESSONS

Does my reading of *The Discourses* alter our assessment of how Machiavelli fits into the history of reflections on popular government? Clearly, he is neither an epigone of classical republicanism nor a pioneer of modern antimoralism, unconcerned with institutional form. Machiavelli may pose the question of elite control and popular government as forcefully as any other political philosopher, yet contemporary democratic theory generally looks to the contract tradition as a resource for holding elites accountable. Why? One reason is that Machiavelli's answers seem imprecise analytically: The combination of normative prescription, historical description, and textual commentary renders his conclusions less than readily transparent. Moreover, his conclusions, when specified, do not seem immediately transferable to contemporary circumstances in an obvious way. Consider the example of public accusation. The institution of "special prosecutors," for instance, seems to serve elite as much as popular interests.

If we think about these issues historically, in the classical age there were the socially specific institutions of the simple regimes: monarchies, democracies, and oligarchies, or rule by the one, the poor, or the rich. Today there are the completely agnostic socioinstitutional arrangements of modern liberal democracy. In between reside the socially reflective institutions of Rome and Machiavelli's interpretation of them. In his model, the popular element is represented by the tribunes and the largely timocratic popular assemblies. But it is also embodied by the concilium, which is composed of all nonnoble citizens and directly expressed through such practices as the accusation and the provocatio. In general, most Roman institutions were socially specific in a way that is intolerable by modern representative standards. Those institutions,

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[11] In an excellent recent work, Baehr (1998, 287ff) details the socioeconomic changes that made Julius Caesar a successful usurper of the republic, whereas the earlier attempts of Cassius, Marius, Appius, and others were failures. The increasing debt and diminishing property shares of the urban and especially rural plebs encouraged them to seek sustenance in military ventures alone. Consistent with Machiavelli, Baehr demonstrates that the ensuing corruption could have been minimized or forestalled by the senate had it adopted programs of debt relief and land distribution, which they considered but dismissed (p. 289). The senators, after all, were the primary lenders and landowners.

whether representative or direct, were identified with particular social classes. Liberal democracies, in contrast, presume that all citizens are "equal" and assume the general influence of all on the institutions of government.

In a sense, Machiavelli's reinterpretation of Rome combines some of the more direct elements of classical politics and the nascent representative quality of Roman practice. It is important for Machiavelli that the popular element be both mediated and expressed institutionally. His republic is a mixed regime that holds within the popular element a further mixing—a mixing between representation and direct expression. The modern republican cum liberal-democratic blueprint, The Federalist Papers (Madison, Hamilton, and Jay [1788] 1998), eschews such an arrangement for two possible reasons. The first is a postfeudal/preindustrial faith in the emergence of a variegated social pluralism that presumably would outstrip categories of two, or even three, opposing classes. The second is a Hobbesian distrust of any kind of socially specific claims upon institutions of government. The Federalists likely were aware of exactly what Machiavelli knew and celebrated: Institutional class specificity encourages political class conflict.

The crucial difference between Machiavellian republicanism and contractarian liberalism, however, is not simply a preference for class conflict per se but for a particular kind of institutional facilitation of it. Machiavelli seeks to control elites first and foremost, despite the risk of allowing certain excesses on the part of the populace. His frequent omissions and equivocations regarding their behavior suggest as much. Contractarianism seeks to control both elites and the public through constitutional and electoral arrangements that exclude the populace as such from governance (Holmes 1995). The result of the latter arrangements, civic-culture or participatory-minded critics charge, is a stultification of the populace, a structural encouragement of their disinterest in politics that includes the concern for holding elites to account. How seriously we evaluate this charge will inevitably depend on whether we judge the supposed passivity of the electorate in contemporary representative democracy to be a result of the commercial/private attentions of citizens in capitalist societies, or whether we judge political arrangements to be the principal cause of such economic fixations and political passivity. This is, of course, a recurring debate in democratic theory (see, e.g., Barber 1990; Gutmann 1991; Przeworski 1991; Shapiro 1996).

As an heir to contractarian theories, minimalist democracy certainly appears stunted or sterile by criteria that favor high levels of substantive participation. From such a perspective, the proposition that a populace or electorate should serve merely as an arbitrator among elite actors seems to be a rather stultified framework for popular government. If it has a ready correlate in traditional political theory, the Hobbesian scenario seems the most apt, if normatively unflattering, comparison: Subjects consent to a particular power among others to impose order upon society at large. When we add to that formula merely the periodic

reaggregation of consent through elections, we have a fair approximation of minimalist democracy. Yet, minimalist democracy may be undermined without a diverse and attentive populace, which may, in fact, be homogenized and infantilized by a lack of more substantive and direct participation. Machiavelli's Discourses raises serious questions for advocates of minimalist conceptions of democracy along these lines: Does elite control require class conflict in addition to general elections? Is social or liberal democratic politics sufficiently conflict-engendering to sustain vigilant control of elites?

Civic-participatory and neorepublican prescriptions for renewing substantive democracy (e.g., Macedo 1998; Rosenblum 1998; Warren 2000) seem to offer a cultural supplement that would render minimalist democracy more sustainable. But such approaches are rather pacific in comparison with the ferociousness of Machiavellian popular government. It is not merely the fact that Roman citizens belonged to different groups that renders republican politics healthy and dynamic for Machiavelli; rather, they belonged to fiercely competitive groups. In this sense, the constructively participatory and tranquillity-inclined disposition of civic-culture approaches may not generate the requisite animosity to encourage better responsiveness and greater accountability among elites. Contentiousness over different conceptions of the common good, over more or less just forms of domination, provides this in Machiavelli's Rome. In his account of the rise and decline of associations in the United States, Putnam (2000), for instance, does not substantially distinguish between the social benefit sort of associations, such as those involved in the civil rights movement, and those devoted to hobbies or sports. Yet, it is precisely attention to or pursuit of social justice, which is often underemphasized in civic-culture approaches, that may promote a more vigilant populace.

As Shapiro (1999, 30) argues, in addition to the practices of collective self-government, which is relatively well served by electoral politics, democracy must be concerned with diminishing the arbitrary exercise of domination and with ameliorating asymmetries of power. The imperfectly just, that is, inevitably unjust exercise of power by political and social elites can only be checked by a populace with a disposition toward distrust, suspicion, and even resentment of them. The Roman people in Machiavelli's account realize that they will never be remotely free from noble domination without remaining suspicious of, and making claims upon, the wealth and political authority of those elites. In this spirit, Shapiro (1996, 10; 1999, 15) argues that democracy is not sustainable if it does not breed democratic habits of interaction and does not reduce injustices of common institutions—interactions and reductions achieved through dissensus, not necessarily consensus.

A Machiavellian paradox perhaps lost on civic-culture theorists of democracy is that socioeconomic and political conflict may breed stronger allegiance than the active pursuit of a consensually derived common good (see Shapiro 1996, 108). Along these lines, how

might our political and perhaps socioeconomic elites be handled more aggressively? How might we begin to fashion Machiavellian mechanisms of participation, responsiveness, and accountability? Manin, Przeworski, and Stokes (1999, 5, 13, 49–50) propose the development of multiple "accountability agencies" that could supplement elections in the effort to control elites more directly; these might take the form of independent campaign, information, and auditing bodies. Pettit (2000) theorizes contestatory practices, such as the veto or better appellate institutions, through which electorates might review or amend decisions of elected elites. These institutions could function in a manner reminiscent of the tribunes, the accusations, the appeal, and the concilium in Machiavelli's account of Rome. In more general terms, Shapiro (1999) proposes extraelectoral democratic procedures to deal with social issues often considered "private," such as child rearing; gender, sexual, and marital relations; issues of the workplace; and health care, retirement, and death. If rendered more democratically accessible, these policy spheres might no longer be the free domain of quasi-autonomous elites.

Machiavelli's *Discourses* raises questions and proposes solutions with respect to the adequacy of minimalist democratic arrangements in achieving one of their most important goals—control of elites. Moreover, it does so without an appeal to consensus over a common good that is voiced or presupposed by contemporary adherents of civic participation and neorepublicanism. In this way, Machiavelli contributes to the prospect of taking advantage of the respective strengths of both formal democratic theory and civic participatory approaches.

CONCLUSION

Minimalist theorists of democracy now concede that elections might not be sufficient to render elites responsive and accountable, in particular, and to make democracy self-sustainable, in general (Przeworski 1999). Although they do not appeal for more substantively participatory practices characteristic of civic-culture, civil-society, and neorepublican critics of liberal democracy, democratic minimalists do call for more direct "accountability agencies" through which elites might be made more controllable. I have shown that a theorist who mixes representative-electoral institutions with more direct forms of elite control is Niccolò Machiavelli.

Machiavelli's democratic theory of elite control can be summarized as follows. In some respects the people are confined simply, if not exclusively, to selecting elites for office and choosing among their policy proposals. In other respects, they are active competitors with the established elites for such offices, and they constantly patrol the latter through the institutions of the tribunes and practices such as public accusations and popular appeals. Moreover, the Roman plebs could meet collectively in the contiones and concilium plebis to discuss and make laws, respectively. Machiavelli adds to these institutional features of popular

government an important cultural dimension: The people should despise and mistrust the elites, and they should be wary of and actively confront the injustice that elite governing inevitably entails. This disposition serves to fuel popular efforts to render the elites more fully responsive and accountable. When Machiavelli calls the Roman people the "guardian of liberty," he has in mind this fuller conception of their control of elites. The merely reactive, ratifying, and manipulable quality of the Roman plebs presented in classical histories and contemporary historical research is not commensurate with the kind of virtue necessary to keep the craven and unscrupulous Roman nobility in check. However much elites may have changed their forms, history does not provide any solid evidence to suggest that their disposition has altered very much since Machiavelli's time.

Machiavelli confirms for us, quite simply, that elections are not enough. Popular primacy in his republic means more than just choosing elites through elections. Merely electoral standards certainly make it possible to interpret a republic according to the traditional Aristotelian/Polybian criterion: A good republic should appear to be both an oligarchy and a democracy, depending upon how you look at it. Electoral standards of democracy allow us to say that elites rule but that the people choose which elites do the governing. The system is therefore oligarchic and democratic. In Machiavelli's estimation, the electoral standard, like most of the great standards of political philosophy, traditional or modern, humanistic or formal, only serves to favor the elite. Machiavelli advocates an unambiguously popularly dominated republic. According to the standards of his day, that would not have meant a democracy per se but a democratically tilted mixed regime. By today's standards we could do worse than call such a regime a democracy.[12]

Ultimately, Machiavellian democracy can be characterized concretely as an institutional mix of popular representation and direct popular participation, as well as a political culture driven by an active rather than passive sociopolitical orientation. At first blush, contemporary liberal democracies seem wanting in comparison. What form might institutions and practices of Machiavellian democracy take today? Strategies focused on democratic justice, accountability agencies, and contestatory republicanism are certainly appropriate starting points for bridging the gap, in a Machiavellian manner, between minimalist democracy and civil-society/participatory approaches to popular government.

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[12] An important continuity in the metamorphosis of ancient into modern republicanism has been socioeconomic and political elitism. In this light, Pocock's (1975) otherwise magisterial study of the Renaissance-Florentine conduit of this transformation would have been more appropriately titled *The Guicciardinian Moment*. It perhaps ought to have been named after Machiavelli's more oligarchically indulgent contemporary and interlocutor, Francesco Guicciardini, and not the populist, elite-despising subject of this essay.

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State Supreme Courts in American Democracy: Probing the Myths of Judicial Reform

MELINDA GANN HALL *Michigan State University*

I address the controversy over how judges should be selected by analyzing the electoral fortunes of incumbents on supreme courts from 1980 through 1995 in the 38 states using elections to staff the bench. Court reformers argue that partisan elections fail to evidence accountability, while nonpartisan and retention elections promote independence. Thus, issue-related or candidate-related forces should not be important in partisan elections, and external political conditions should not be important in nonpartisan and retention elections. Results indicate that reformers underestimated the extent to which partisan elections have a tangible substantive component and overestimated the extent to which nonpartisan and retention races are insulated from partisan politics and other contextual forces. On these two fundamental issues, arguments of reformers fail. Moreover, the extraordinary variations across systems and over time in how well incumbents fare with voters, which bear directly upon the representative nature of elected courts, merit further explanation.

W hat determines how well judges seeking re-election fare with voters? Remarkably, we do not know, despite the fact that most judges in the United States must seek voter approval regularly to retain their seat. Perhaps this lack of knowledge reflects the general perception among scholars that judges are "infrequently challenged and rarely defeated" (Dubois 1980) or widely held beliefs derived from the legal culture that courts, which lack a formal representative function, are highly insulated from politics. Nonetheless, with few exceptions, we have little more than "unsystematic personal accounts and anecdotes of observers" (p. 34) about the forces that structure the electoral fortunes of judges.

Inextricably linked to considerations of judges' electoral performance is the debate currently raging in the states over the politics of institutional design and, specifically, the relative merits of the three types of election systems (partisan, nonpartisan, and retention elections) for staffing the bench.[1] Judicial reform advocates charge that partisan elections fail to achieve their goal of promoting accountability and thus should be abandoned. Instead, reformers favor nonpartisan elections and the Missouri Plan (also called the "merit plan") on the assertion that such schemes guarantee the preferred goal of independence. Yet, these arguments and the premises underlying the three election

Melinda Gann Hall is Professor of Political Science, Michigan State University, 303 South Kedzie Hall, East Lansing, MI 48824 (hallme@msu.edu).

[1] States use partisan elections, nonpartisan elections (no party labels in the general election), and the Missouri Plan to select judges. Under the Missouri Plan, nominees for each vacancy are identified by a commission appointed by the governor, who makes the initial appointment from the list provided by the commission. The newly appointed justice immediately assumes office and then appears in the next general election to seek a regular term. Of course, variations from state to state occur with all three methods.

systems have not been subjected to scientific scrutiny, although they have guided the choices of state governments in recent decades.

To address this important controversy and to enhance our understanding of judicial elections, I evaluate the factors that govern how successfully state supreme court incumbents garner electoral support, measured as the percentage of the vote received. Data are drawn from all 643 elections to state courts of last resort from 1980 through 1995 in the 38 states that use elections to staff the high court bench.[2] My primary focus is on accountability and independence in alternative election systems, but I estimate general models of electoral outcomes that take into account characteristics of candidates, issues and other contextual forces, and institutional arrangements. Following standard practice, I define accountability as incumbent vote shares systematically influenced by issue-related or candidate-specific forces, ceteris paribus, and independence as electoral performance impervious to external partisan forces. Stated differently, accountability and independence are evidenced through the correlates of support for incumbents.[3]

Several qualifications to this research should be noted at the outset. First, I make no claim of assessing the overall success of the reform movement, and I do not address all concerns expressed by judicial reform advocates. Rather, I take a defined set of testable propositions fundamental to the reform movement and submit them to empirical evaluation. If these propositions are not supported by objective evidence, then the results will lend credence to the need for a more careful examination of the tenets of institutional design that have influenced state governments over the past several decades. Even so, this work will not demon-

[2] I will use "state supreme court" and "court of last resort" interchangeably.
[3] Lowry, Alt, and Ferree (1998) provide a recent example of this approach. They define accountability as a relationship between changes in aggregate public revenues and spending, on the one hand, and vote and seat share in state gubernatorial and legislative elections, on the other.

strate the general wisdom or folly of either the reformers or partisan election advocates.

In addition, this study is conducted at the macro level and does not present evidence or draw conclusions about the behavior of individual voters. As such, this approach is consistent with studies of presidential (e.g., Erikson 1989; Hibbs 1987; Tufte 1978), congressional (e.g., Born 1986; Erikson 1990; Jacobson and Kernell 1983; Kramer 1971; Tufte 1978), and other state elections (e.g., Chubb 1988; Turett 1971) that use aggregate data to examine retrospective voting as a form of accountability. Although a macro approach does not permit the exploration of which individual voters respond to contextual information or why such responses occur, aggregate analysis facilitates an inquiry into whether election results are sensitive to certain stimuli, which is the basic question addressed here.

Finally, I do not examine the decisional propensities of individual justices or court majorities. Accountability and independence can be defined with reference to the individual and collective behavior of justices (Brace and Hall 1995, 1997; Brace, Hall, and Langer 1999; Hall 1987, 1992, 1995; Hall and Brace 1992), but these aspects are beyond the scope of my inquiry.

THE JUDICIAL SELECTION CONTROVERSY

One of the most enduring issues on the American political agenda is how to select judges for the state court bench. Because intense controversy is necessarily associated with decisions about who controls access to political power, judicial selection has generated endless debate (largely within the legal community), replete with claims and counterclaims about the various selection schemes. Almost universally, this discussion is framed as a conflict over the goals of electoral accountability and judicial independence (e.g., Champagne and Haydel 1993; Culver and Wold 1986; Dubois 1980; Hall and Aspin 1987; Lovrich and Sheldon 1983; Sheldon and Maule 1997; Watson and Downing 1969), and each election plan attempts to balance, albeit differently, these competing goals.[4]

Among the most significant controversies is whether partisan elections promote accountability, the presumed raison d'être of such systems. Reformers argue that partisan elections, characterized by lackluster campaigns devoid of issue content, are disconnected from substantive evaluations of candidates or other meaningful considerations relevant to the judiciary, which renders them ineffective as a means of accountability. As succinctly summarized by Dunn (1976, 285), the charge is that partisan elections do not "effectively or consistently serve the intended purpose for which they were designed." Indeed, Dubois (1980, 28) describes the reformers' arguments about the failure of partisan elections to secure popular control over the

courts as "the most fundamental and damning of the criticisms leveled against popular judicial elections."[5] Interestingly, reformers have not been concerned with whether nonpartisan and retention elections promote accountability but, rather, with the alleged failure of partisan elections to do so.

There is a serious conflict in the literature between judicial reform advocates and political scientists over the ability of partisan elections to promote accountability. In pathbreaking work, several political scientists have documented that, at least under certain conditions, judicial elections reflect informed judgments by the electorate (e.g., Baum 1987; Dubois 1980; Lovrich and Sheldon 1983; Scheb 1983). Accordingly, election results in judicial contests vary systematically with assessments of the candidates and issues, which manifests linkages that constitute accountability.

In addition to these contradictory assertions about accountability, the literature is dominated by a host of unsubstantiated claims, particularly regarding the Missouri Plan, which now is favored by reformers. Indeed, many of the arguments by reformers "have been accepted largely without contradiction, not because of positive demonstrations of their validity but in the absence of research data to the contrary" (Dubois 1980, xi).

Of particular importance are discussions about judicial independence. Reformers assert that the Missouri Plan, of which retention races are a part, and to a lesser extent nonpartisan elections remove judges from the vicissitudes of interpartisan competition (Herndon 1962) and the "deleterious political influences present under the [partisan] elective approach" (Dunn 1976, 286). As expressed by the Missouri Plan's most vigorous advocates, its "greatest accomplishment has been to remove the judiciary from the contamination of politics" (p. 286). Nonpartisan elections also are presumed to have such properties, but retention races are touted as superior for promoting independence, despite the fact that judges must face voters. The general argument is that when partisan labels are removed from ballots and when challengers are not allowed to engage incumbents in what might easily become partisan brawls, judicial elections will center fully upon the professional qualifications of judges.

Fundamentally, there are reasons to doubt the claims of the reformers, despite the increased popularity of the Missouri Plan. One of the most basic arguments of those who favor the plan is that it produces a more qualified bench. To the extent that quality can be measured objectively, however, the evidence to date suggests that the Missouri Plan does not fulfill its promise. In a comprehensive study of state supreme courts, Glick and Emmert (1987) conclude that the professional credentials (e.g., prestige of legal education, legal and judicial experience) of judges are quite similar, regardless of the method of selection.

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[4] Two sources that effectively encapsulate the reform controversy and its dominant perspectives are Winters 1973 and Champagne and Haydel 1993. Atchison, Liebert, and Russell (1999) provide an extensive annotated bibliography of this vast literature.

[5] Legislative selection and gubernatorial appointment, used in twelve states, have received scant attention from court reformers. Nonpartisan elections and the Missouri Plan are designed to replace partisan elections rather than appointment schemes.

Moreover, their findings are consistent with earlier studies (e.g., Canon 1972). Most basically, the Missouri Plan does not appear to result in the recruitment of judges who differ in any obvious ways from judges chosen by other methods. Therefore, in the absence of solid evidence, other assertions about the virtues of the Missouri Plan are suspect.

The lack of empirical verification notwithstanding, numerous states have abandoned partisan judicial elections and replaced them with nonpartisan and retention elections.[6] Since the 1960s, sixteen states have changed the way in which they staff their highest court, and of these sixteen, twelve have chosen the Missouri Plan (Council of State Governments, 1960–98). Five states adopted the plan prior to 1960, and now a total of 17 states select the high court bench by this method. Moreover, the American Judicature Society (1991, 236), which believes that "instituting merit selection in every state is urgent," continues the campaign for nationwide adoption of the Missouri Plan.

The charges about accountability and independence can be formulated into testable hypotheses. If the reformers are correct, two patterns should be apparent. First, the electoral performance of incumbents in partisan elections should not be systematically influenced by issue-related or candidate-related forces. Second, external political conditions should remain unimportant in determining how well incumbents perform in nonpartisan and retention races, although such forces may be significant in partisan elections. More generally, there should be pronounced differences among election systems in the types of forces that structure election returns.

PATTERNS OF COMPETITION IN CONTEMPORARY JUDICIAL ELECTIONS

To examine the character of contemporary state supreme court elections and to test the propositions discussed above about accountability and independence, I gathered data on all elections to state courts of last resort from 1980 through 1995 from a variety of sources. Most important were official reports of secretaries of state, obtained by mail or visits to state capitals. If official reports were not available, state "blue books" were searched. As a final measure, telephone conversations with sitting justices and clerks of court as well as newspaper searches on Lexis-Nexis were used.

The result is a complete set of returns, for both primaries and general elections, for every election over a sixteen-year period to every high court in all states which use elections for these positions (including both civil and criminal courts of last resort in Texas and Oklahoma). Sheldon and Maule (1997, 22) attribute the failure to understand selection processes to the lack of comprehensive data, observing that "judicial recruitment studies tend to focus on one or two states during

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[6] The Missouri Plan has won approval in seventeen states. Ten states still use partisan elections, and eleven use nonpartisan elections to staff their highest court.

TABLE 1. Percentage of State Supreme Court Incumbents Challenged for Reelection, 1980–95, by Type of Election System

Year	Nonpartisan Elections % (n)	Partisan Elections % (n)	All Elections % (n)
1980	56.5 (23)	52.6 (19)	54.8 (42)
1982	60.0 (20)	41.7 (24)	50.0 (44)
1984	42.1 (19)	56.3 (16)	48.6 (35)
1986	16.7 (24)	61.1 (18)	35.7 (42)
1988	33.3 (15)	73.7 (19)	55.9 (34)
1990	37.5 (24)	70.6 (17)	51.2 (41)
1992	54.5 (22)	65.0 (20)	59.5 (42)
1994	56.3 (16)	81.8 (11)	66.7 (27)
Total	44.2 (163)	61.1 (144)	52.1 (307)

Note: Odd-year data are merged with previous year. Results from multimember elections are calculated according to the Jewell (1982) procedure. See text and note 7 for greater detail.

one or two selection cases." My study overcomes that deficiency and is the first research on supreme court elections that is national in scope. Overall, from 1980 through 1995, there were 643 elections to state supreme courts: 234 retention elections, 201 nonpartisan elections, and 208 partisan elections.

To supplement the election data and to identify such important characteristics as incumbency and dates of accession, I collected biographical data on incumbents seeking reelection and other winners from state blue books, *The American Bench, Martindale—Hubbell*, newspaper articles located through Lexis-Nexis, state supreme court websites, and telephone calls to clerks of court.

Tables 1 through 6 summarize various aspects of competition essential to discussions of accountability and independence. Because of the small number of elections in odd years, those data were merged with the previous year, only for the purpose of calculating descriptive statistics. Also, in the descriptive tables, results from multimember elections were adjusted using the procedure developed by Jewell (1982) to render results comparable to those for single-member races.[7]

Like accountability and independence, electoral competition is a complex concept that can be measured in a variety of ways. One indicator is the extent to which incumbents seeking reelection are challenged, a possibility precluded in retention elections. Table 1 displays the percentage of state supreme court justices challenged from 1980 through 1995 in nonpartisan and

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[7] Jewell (1982) first divided each candidate's vote by the total votes in the district and then multiplied the result by the total number of seats within the district. As state politics scholars generally recognize (e.g., Tucker and Weber 1987; Weber, Tucker, and Brace 1991), this procedure produces vote totals equivalent to those for candidates in single-member districts. With these supreme court elections, the method simply requires multiplying the votes by two, since all multimember races involve two seats only per district.

TABLE 2. Average Vote for Incumbents and Winners of Open Seats in State Supreme Court Elections, 1980–95, by Type of Election System

Year	Retention Elections	Nonpartisan Elections			Partisan Elections			All Elections
		Open	Inc*	All	Open	Inc*	All	
1980	72.4	52.3	77.8	73.2	58.1	75.3	68.6	71.4
1982	74.4	66.6	74.8	73.4	61.1	82.6	77.2	75.2
1984	75.4	55.2	81.5	76.0	59.7	76.6	72.0	74.6
1986	69.9	71.3	91.2	88.3	58.9	71.9	69.0	75.8
1988	75.9	56.4	86.2	79.8	57.7	69.8	66.2	73.4
1990	67.1	53.4	80.5	77.5	66.5	62.5	64.2	69.2
1992	69.2	52.0	73.7	69.1	55.0	71.1	67.4	68.5
1994	66.3	57.3	75.2	69.7	52.5	61.4	57.9	65.2
Total	71.2	57.6	80.2	75.9	59.3	72.3	68.3	71.8
N	234	38	163	201	64	144	208	643

Note: Inc* = Incumbent seeking reelection. Odd-year data are merged with previous year. Results from multimember elections are calculated according to the Jewell (1982) procedure. See text and note 7 for greater detail.

partisan elections. As the table reveals, many justices did not draw opponents, although challenge rates vary substantially over time and across systems. The absence of challengers is especially characteristic of nonpartisan elections; a majority of incumbents in four of the eight election cycles examined did not face the possibility of losing their seat, and up to 83% (in 1986) of the justices seeking reelection in a single election cycle did not inspire challenge. Partisan elections were much more competitive by this standard. With the exception of 1982, most incumbents in partisan elections attracted opponents, and the challenge rate reached almost 82% in 1994. Even so, large proportions of justices in nonpartisan elections and partisan elections have little reason to feel insecure, since incumbents in these systems cannot be unseated if they are not challenged.

A second indicator of electoral competition is the percentage of the vote received by the incumbent or the winner of an open seat. Generally, the literature suggests that candidates win by large margins, although Dubois (1980) notes that about 22% of supreme court races through the 1970s were won with 55% of the vote or less. Similarly, Griffin and Horan (1979) report that incumbents in retention elections consistently receive up to 30% negative votes. Table 2 describes the average percentage of the vote garnered in state supreme court elections by incumbents or winners of open seats from 1980 through 1995. With this measure, larger vote shares indicate less competition. Thus, a justice not challenged for reelection in a partisan or nonpartisan election would receive 100% of the vote.

Based only on the overall means for the various election systems in Table 2, partisan elections are the most competitive, followed by retention elections and nonpartisan elections, respectively. In fact, retention races have become more difficult for incumbents by this measure than nonpartisan elections, both overall and, on average, year to year. Like patterns of electoral challenge, however, these data reveal a great deal of

variation over time and within systems. In 1982, for instance, incumbents in partisan elections received larger portions of the vote than incumbents in nonpartisan or retention elections. Overall, however, candidates in all three election systems can expect to win with sizable proportions of the vote.

Table 2 also demonstrates that the average vote for winners of open seats is significantly less than for incumbents, a pattern common to nonjudicial elections as well. This result is partially explained by the fact that many incumbents are not challenged, whereas open seats almost always involve opposing candidates.[8] This general pattern does not describe partisan elections in 1990, however, in which incumbents did less well than winners of open seats.

Table 3 reports the percentage of supreme court races from 1980 through 1995 won by relatively thin margins, or 55% of the vote or less. Overall, 20.4% were hotly contested. Separately, 35.6% of the partisan

TABLE 3. Percentage of Close Races for State Supreme Court, 1980–95, for Open Seats and Incumbents Seeking Reelection

	Open Seats	Incumbents	All Elections
Retention	—	2.6 (234)	2.6 (234)
Nonpartisan	60.5 (38)	17.2 (163)	25.4 (201)
Partisan	37.5 (64)	34.7 (144)	35.6 (208)
Total	46.1 (102)	15.5 (541)	20.4 (643)

Note: "Close" races are those won by 55% or less of the vote. Results from multimember elections are calculated according to the Jewell (1982) procedure. See text and note 7 for greater detail. Open seats do not occur in retention elections.

[8] Of the 38 open races in nonpartisan elections from 1980 through 1995, three (7.9%) attracted only one candidate. In partisan elections, four (6.3%) of sixty-four open races failed to generate competition.

TABLE 4. Percentage of State Supreme Court Incumbents Defeated, 1980–95, by Type of Election System

Year	Retention Elections	Nonpartisan Elections	Partisan Elections	All Elections
1980	0.0 (35)	4.3 (23)	26.3 (19)	7.8 (77)
1982	0.0 (28)	20.0 (20)	4.2 (24)	6.9 (72)
1984	0.0 (26)	15.8 (19)	0.0 (16)	4.9 (61)
1986	9.1 (33)	4.2 (24)	22.2 (18)	10.7 (75)
1988	0.0 (28)	6.7 (15)	15.8 (19)	6.5 (62)
1990	0.0 (34)	4.2 (24)	29.4 (17)	6.7 (75)
1992	4.3 (23)	9.1 (22)	25.0 (20)	12.3 (65)
1994	0.0 (27)	6.3 (16)	36.4 (11)	9.3 (54)
Total	1.7 (234)	8.6 (163)	18.8 (144)	8.3 (541)

Note: Odd-year data are merged with previous year. Results from multimember elections are calculated according to the Jewell (1982) procedure. See text and note 7 for greater detail.

TABLE 5. Competition in Races for State Supreme Court and the U.S. House of Representatives, 1980–94

	Percentage of					
	Incumbents Challenged		Vote for Incumbents		Incumbents Defeated	
	Courts	House	Courts	House	Courts	House
1980	54.8	86.9	67.0	65.5	7.8	9.3
1982	50.0	86.0	67.3	64.1	6.9	9.9
1984	48.6	83.7	69.0	65.9	4.9	4.6
1986	35.7	81.9	65.8	68.2	10.7	2.0
1988	55.9	80.9	69.0	68.2	6.5	1.7
1990	51.2	79.1	62.3	63.5	6.7	3.9
1992	59.5	91.0	62.0	63.1	12.3	11.7
1994	66.7	86.6	61.5	62.8	9.3	9.8
Total	52.1	84.4	65.4	65.2	8.3	6.5

Sources: For House races only: Abramson, Aldrich, and Rohde 1995; Jacobson 1999.
Note: Odd-year data are merged with previous year. Results from multimember elections are calculated according to the Jewell (1982) procedure. See text and note 7 for greater detail. Average votes for the justices were recalculated to be comparable to averages reported for the House, which omit uncontested races. Rather than exclude retention elections, in which incumbents never can be challenged by other candidates, the statistics reported include all retention races as well as contested nonpartisan and partisan races. If retention elections are excluded, state supreme court incumbents in contested races enjoy *less* support than representatives in similar circumstances. The average vote for incumbents in contested nonpartisan and partisan races from 1980–81 through 1994–95 was, respectively: 59.1%, 58.1%, 59.7%, 57.8%, 58.9%, 55.8%, 55.5%, and 54.3%.

elections, 25.4% of the nonpartisan elections, and 2.6% of the retention races were highly competitive. Generally, partisan elections are more likely to produce narrow margins of victory than nonpartisan or retention elections. In nonpartisan elections, however, close contests tend to be for open seats far more frequently than for seats held by incumbents, a pattern not present in partisan elections.

Table 4 examines the most extreme form of electoral competition: defeat. As the table reveals, justices in state courts of last resort on average are reasonably secure, especially in retention elections, in which the likelihood of defeat is quite low. Overall, among 541 incumbents seeking reelection, only 45 (8.3%) were ousted by voters. Yet, electoral insecurity varies significantly by time and place. For example, incumbents in partisan elections lose in one of every five or six elections, and 36% of incumbents running in such elections in 1994 were defeated. In addition, 20% were ousted in 1982 nonpartisan elections and 9% in 1986 retention elections. This is a radical departure from the widely held belief that justices have little to fear from voters.

The results in tables 1 through 4 are intriguing, but they are better appreciated when compared to elections for other offices. Table 5 provides this perspective by placing judicial elections side by side with elections to the U.S. House of Representatives, which is arguably the most highly accountable American institution by formal design. In the absence of such comparisons, competition in state supreme courts, on average, may seem low—perhaps too low to establish any meaningful connections between the electorate and the bench. Many incumbents are not challenged, many candidates garner large proportions of the vote, and justices seeking reelection, on average, have little reason to fear losing their seat. The fact of the matter, however, is that supreme court justices face competition that is,

by two of three measures, equivalent if not higher to that for the U.S. House.

With respect to the percentage of incumbents who attract opponents, members of the U.S. House face greater electoral peril than supreme court justices. Even in the least competitive year (1990), only 21% of House incumbents escaped challenge. Yet, average winning votes for supreme court justices and House incumbents are quite similar, whereas the overall defeat rate in supreme court elections is higher. Although justices are less likely to be challenged than House members, remarkably, on average, justices have a greater risk of being tossed out of office. The overall defeat rate for House members is 6.5%; it is 8.3% for Supreme Court justices.

Comparisons to other state elections lead to similar conclusions. Jewell (1994) reports that, from 1968 to 1988, 90.3% of the senators and 92.3% of the representatives in state legislatures retained their seat. Similarly, Monardi (1995) documents that races for secretary of state and treasurer typically produce incumbency success rates higher than 90%. Others have noted that electoral margins for state legislative incumbents are high and rising (Garand 1991; Weber, Tucker, and Brace 1991).[9]

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[9] Many elections in the United States simply do not produce

TABLE 6.   States Ranked by Average Vote for Incumbents and Winners of Open Seats in State Supreme Court Elections, 1980–95

State	Election Type	Vote	Standard Deviation	N
Pennsylvania	Partisan/Retention	53.9	8.7	10
Ohio	Nonpartisan	54.7	8.3	20
Texas	Partisan	59.6	18.3	55
Tennessee	Partisan/Retention	60.1	28.4	13
Michigan	Nonpartisan	60.6	10.9	17
California	Retention	60.7	13.0	18
West Virginia	Partisan	62.5	21.0	6
Kentucky	Nonpartisan	63.4	24.9	17
Indiana	Retention	63.5	4.7	6
Alaska	Retention	64.4	6.3	8
Oklahoma	Retention	64.4	5.3	40
Montana	Nonpartisan/Retention	64.5	13.6	17
Missouri	Retention	65.1	5.9	14
Nevada	Nonpartisan	65.6	15.9	14
Illinois	Partisan/Retention	66.5	13.5	16
New Mexico	Partisan	66.8	21.8	11
Florida	Retention	69.6	5.2	19
Colorado	Retention	71.3	2.7	9
North Carolina	Partisan	71.6	25.7	23
Nebraska	Retention	72.9	5.1	16
Alabama	Partisan	74.5	20.7	26
Wyoming	Retention	74.8	8.4	13
South Dakota	Retention	75.3	5.5	6
Kansas	Retention	75.5	5.7	19
Louisiana	Partisan	75.8	22.7	14
Arizona	Retention	76.0	5.6	12
Wisconsin	Nonpartisan	77.1	23.0	11
Iowa	Retention	78.0	2.2	20
Arkansas	Partisan	78.1	20.5	16
Washington	Nonpartisan	78.6	23.1	29
Mississippi	Partisan	79.7	22.5	20
Utah	Nonpartisan/Retention	81.6	10.1	9
Maryland	Retention	84.3	3.8	12
Georgia	Partisan/Nonpartisan	86.9	19.1	24
Minnesota	Nonpartisan	87.2	18.2	17
North Dakota	Nonpartisan	89.0	23.4	10
Oregon	Nonpartisan	89.7	17.8	23
Idaho	Nonpartisan	96.7	11.7	13
All states	All elections	71.7	18.8	643

TABLE 7. Description of Variables Used in State Supreme Court Election Models

Variable	Variable Description
Dependent Variable	
Vote	Percentage of the vote for the incumbent
Political Context/Accountability	
Ideological Distance	Net difference between the justice's ideology and state citizen ideology at the time of the election
Murder Rate	State murders and nonnegligent manslaughter per 100,000 population, lagged one year
Partisan Context/Independence	
Ranney Index	Ranney Index of state partisan competition, as calculated and reported by Holbrook and Van Dunk (1993)
Partisan Challenger	1 if candidates of the opposite party compete in a partisan or nonpartisan race, or if a governor of the party opposite the incumbent justice in a retention race is elected on the same ballot; 0 otherwise
Candidate Characteristics	
New Appointee	1 if the election involves a justice·initially appointed and facing his/her first election; 0 otherwise
Minority Candidate	1 if the incumbent justice or winner of an open seat is female, Hispanic, or African American; 0 otherwise
Institutional Arrangements	
District Election	1 if seat represents a district rather than the state; 0 otherwise
Term Length	Length of term (in years) for supreme court justices
Control Variables	
Multimember Race	1 for multimember elections or general elections with more than two candidates; 0 otherwise
1982–83 . . . 1994–95	1 if election occurred in the designated year; 0 otherwise

Table 6 arrays the 38 states according to the average vote for incumbents and winners of open seats in supreme court races from 1980 through 1995. Quite clearly, the states vary significantly along this dimension. Pennsylvania, Ohio, and Texas are the most competitive, with votes that average less than 60%. At the other extreme, North Dakota, Oregon, and Idaho virtually guarantee success for incumbents, with votes averaging 89% and higher. Interestingly, the five most competitive states (Pennsylvania, Ohio, Texas, Tennessee, Michigan) use all three types of election systems (nonpartisan, partisan, and retention), and the five least competitive (Idaho, Oregon, North Dakota, Minnesota, Georgia) all·use nonpartisan elections.

In sum, looking only at overall patterns of competition in state supreme court elections and without placing these results in the broader political context, one reasonably might conclude, as have many in the past, that judicial elections are not likely to promote accountability and are not influenced by external po-

litical forces. A detailed exploration of the data reveals a more complex truth. Competition, measured in numerous ways, varies significantly across states and over time. Also, when compared to the quintessential representative institution, the U.S. House of Representatives, competition in judicial races on several dimensions is not low. Furthermore, the extraordinary variations across systems and over time, which bear directly upon the representative nature of elected courts, warrant explanation.

SPECIFYING ELECTION MODELS FOR STATE SUPREME COURTS

Table 7 describes the variables used in the elections models estimated below. Because my goal is to assess the determinants of how incumbents fare with voters, open seats are excluded from the multivariate analysis.

The dependent variable (*Vote*) is the percentage of the vote cast for the incumbent, either as a percentage of the total vote for all candidates in each nonpartisan and partisan election, or as the percentage of positive votes to retain a justice in a retention election. In identifying each incumbent's vote share, both primary and general election results were used; this takes into account

opposition (or therefore defeat). Judging solely by these indicators, elections in the United States arguably are a failure. According to Pomper (1980, 29), however, viewing democracy and elections from this perspective is "missing the point" since opposition and defeat are not necessary conditions for accountability.

the handful of situations in which competition occurs only in the primary and exclusion of these data would introduce systematic bias against largely one-party states.

Consider, for example, the hypothetical situation in which Justice A competes in a primary against multiple candidates and emerges with a plurality but not majority of the vote. She already has appeared in one election with multiple challengers and now must face an opponent in a second primary before being placed on the ballot in the general election. In the general election, because of a lack of partisan competition in the state, Justice A does not have an opponent. Looking only at general election results, she would be classified as having faced no opposition and as receiving 100% of the vote, a score identical to that for a justice who never was challenged at all. Clearly, these situations must be accounted for in comparative models of state elections. Therefore, for a justice challenged in the primary but not in the general election, the vote recorded is the primary vote (the final one if there was more than one primary). Of the 541 elections with incumbents, 30 (5.5%) fell into this category.

The critical independent variables test the assertion that partisan elections fail to promote accountability and that nonpartisan and retention elections protect independence. Regarding accountability, two variables indicative of the electorate's sensitivity to candidate-related and issue-related forces are included. The first is a demanding standard: The electorate should respond to ideological differences between themselves and the justices. Reformers assert that voters in partisan elections have not demonstrated the capacity to make informed choices, but case studies contradict this assertion (Baum 1987; Lovrich and Sheldon 1983; Lovrich, Sheldon, and Wassmann 1988; Squire and Smith 1988). Measuring *Ideological Distance* at the time of each election is straightforward using the Brace, Langer, and Hall (2000) supreme court justice ideology scores and the Berry et al. (1998) state citizen ideology scores. If the literature on the ability of voters to make reasoned choices in judicial elections is correct, then greater distance should be associated with lower levels of electoral support for incumbents across electoral systems. Alternatively, if reform advocates are correct, then distance should have no effect in partisan elections, because party affiliation of judges may exhaust the ideological information voters use.

In contrast, predictions about ideological distance in nonpartisan and retention elections can work in either direction, depending on which reform advocates are referenced. While some might consider ideological distance to bear upon the qualifications of candidates, others would disagree. As noted earlier, some believe that by removing partisan labels from ballots in nonpartisan elections and by eliminating labels and challengers in retention elections, voters will cast ballots largely based upon the justices' professional qualifications, defined solely with reference to legal experience and skill. Therefore, these advocates would expect no relationship between ideological distance and the electoral success of incumbents. Overall, however, reformers believe that removing partisan labels forces the

electorate to seek information beyond partisanship about the acceptability of incumbents, thus enhancing the quality of the electorate's knowledge and judgment. This is particularly important in largely one-party states where partisanship is not an accurate surrogate for political preferences.

The second indicator of accountability relates to issue evaluations. In gubernatorial and state legislative races, voters hold public officials accountable for poor economic conditions and increased taxes. Although studies are not without some contradiction (Svoboda 1995), research on gubernatorial elections (Atkeson and Partin 1995; Carsey and Wright 1998; Niemi, Stanley, and Vogel 1995; Svoboda 1995), subgubernatorial statewide elections (Monardi 1995), and legislative elections (Lowry, Alt, and Ferree 1998) reports retrospective voting based on the economy. Of course, this phenomenon at the national level has been well documented.

In judicial elections, arguably the most important policy focus for voters is crime. Whereas judges have no direct responsibility for economic conditions, it is not unreasonable to blame them, at least in part, for the public safety. From the perspective of justices' voting behavior, I (Hall 1995) demonstrate that when murder rates are high, elected justices are less likely to overturn death sentences precisely because they fear electoral retribution. Reform advocates have serious doubts about the ability of voters in partisan elections to respond to issues at election time and would consider such responses illegitimate in nonpartisan and retention contests for the bench. To explore these contentions, the models include a measure of each state's murder rate, lagged one year (*Murder Rate*), identified using the *Statistical Abstract of the United States*.[10] Increases in the murder rate should have a negative effect on the electoral performance of incumbents if the electorate renders retrospective judgments characteristic of other elections.

Two measures of partisan politics, which tap the concept of independence, are included in the models. Both reformers and political scientists expect states characterized by higher levels of partisan competition (see, e.g., Dubois 1980; Patterson and Caldeira 1983) to have competitive judicial elections when partisan labels appear on the ballot. This, of course, is precisely the sort of external politics that nonpartisan and retention elections are designed to preclude. To examine this question, including whether nonpartisan and retention elections are immune to partisan context, the models use the Ranney Index (*Ranney Index*) as recalculated by Holbrook and Van Dunk (1993). Higher scores on the index indicate higher levels of partisan

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[10] Violent crime rate is not used because the measure presents a comparability problem across states. Violent crime is a composite measure: murder and nonnegligent manslaughter, rape, robbery, and aggravated assault. The mix of crimes varies significantly across states and electoral systems. Correlations between murder rates and violent crime rates in retention, nonpartisan, and partisan election systems are, respectively, .82, .82, and .43. Thus, in partisan election states, violent crime includes fewer murders and a higher proportion of crimes such as aggravated assault, about which citizens may not be as concerned as they are about murder.

TABLE 8. Predicted Relationships between Indicators of Accountability and Independence, and the Electoral Performance of Incumbents in Judicial Elections, Derived from the Judicial Reform and Political Science Elections Literatures						
	Retention Elections		Nonpartisan Elections		Partisan Elections	
	Reform	Elections	Reform	Elections	Reform	Elections
Ideological Distance	$\beta < 0$	$\beta < 0$	$\beta < 0$	$\beta < 0$	$\beta = 0$	$\beta < 0$
Murder Rate	$\beta = 0$	$\beta < 0$	$\beta = 0$	$\beta < 0$	$\beta = 0$	$\beta < 0$
Ranney Index	$\beta = 0$	$\beta < 0$	$\beta = 0$	$\beta < 0$	$\beta < 0$	$\beta < 0$
Partisan Challenger	$\beta = 0$	$\beta < 0$	$\beta = 0$	$\beta < 0$	$\beta < 0$	$\beta < 0$

competition, which should have a negative effect on incumbent vote shares.[11]

Furthermore, election-specific partisan competition (that is, candidates of opposite parties directly challenging each other) should weaken the electoral performance of incumbents in partisan elections, as both political scientists and judicial reform advocates assert. This variable is not constant for partisan elections. In many races, competition occurs only in the primaries or is absent entirely. Alternatively, given the absence of partisan labels on the ballot, election-specific partisan competition should have little or no influence in nonpartisan or retention elections. To estimate these effects, a variable is included to indicate whether candidates of opposite parties appear in each general election (*Partisan Challenger*). For both partisan and nonpartisan elections, this variable is coded 1 if a partisan challenger is present, 0 otherwise. In nonpartisan elections, the partisan affiliations of challengers were identified through newspaper coverage of the elections. In both primaries and general elections in nonpartisan states, challengers frequently are from the same party as the incumbent.

Given the absence of challengers by design in retention elections, a different indicator is necessary to capture the presence of election-specific partisan pressures. Because voters tend to associate justices with governors who appoint them (Squire and Smith 1988), partisan challenger is coded 1 in retention elections when a governor of the opposite party to the justice is on the same ballot and wins, 0 otherwise. Obviously, this is an imprecise measure, but it seems a better alternative than ignoring the effect altogether.

To summarize this complex set of contradictory assertions, Table 8 compares the predicted relationships between the two sets of variables indicative of accountability and independence, on the one hand, and the proportion of the vote for incumbents in partisan, nonpartisan, and retention elections, on the other. In several situations, the reform literature and political science literature converge. There are important ways in which expectations diverge, however, and the models below provide explicit tests of these hypothesized relationships.

Other Candidate-Specific Forces

Other forces can be expected to influence the ability of incumbents to garner votes in state supreme court elections. One is the type of incumbent present. Many incumbents will have an electoral advantage, but this may not be the case for justices initially appointed and seeking their first election to the court. In retention election states, all justices acquire their seat by appointment, and many justices in partisan and nonpartisan systems also are initially appointed to fill unexpired terms (Dubois 1980; Herndon 1962). Given their inexperience with supreme court campaigns and on the high court bench, appointed justices (*New Appointee*) appearing before voters for the first time in any type of judicial election may not fare as well as their more electorally experienced colleagues. In the elections examined here, 48.7% of the incumbents in retention elections, 33.7% in nonpartisan elections, and 27.8% in partisan elections were newly appointed.

In addition, minorities have not been particularly successful in gaining representation on state high courts. Glick and Emmert (1987) found that minorities under the Missouri Plan are disadvantaged with respect to initial appointment, although other work (Alozie 1988, 1996) fails to find a connection between gender or racial representation in courts of last resort and selection systems. More to the point, racial cues have been identified as important in judicial elections (Lovrich, Sheldon, and Wassman 1988; Squire and Smith 1988). Given that there may be prejudice against minority candidates, I hypothesize that female, African American, and Hispanic candidates will garner less support from voters than nonminorities across all three election systems (*Minority Candidate*).[12]

Institutional Arrangements

Institutional arrangements have been shown to be crucial forces in shaping electoral politics. In the context of state supreme courts, the primary institutional feature that should structure electoral outcomes

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[11] The Holbrook and Van Dunk electoral competition measure is not used primarily because my concern is with partisan competition and not electoral competition.

[12] Volcansek (1981) established that bar poll results and newspaper endorsements influenced vote shares in trial court elections held in the 1960s and 1970s in Dade County, Florida. Because such data are not available across the states or over time, these factors were not examined.

is the type of election. Generally, partisan elections should be more competitive than nonpartisan and retention elections, and nonpartisan elections should be more competitive than retention elections. Moreover, the election scheme should mediate the effects of candidates, issues, and other external pressures on the relative success of incumbents. Because it is critical to observe the effects of the independent variables across systems, separate models for each election system are estimated, which precludes an estimation of the direct effects of election system on electoral performance. Nonetheless, by observing differences across models, we can ascertain whether conditional effects are evidenced.

Aside from basic election scheme, the literature generally finds that smaller constituencies are less competitive than larger constituencies. For example, races for the U.S. Senate are less competitive in smaller states (e.g., Hibbing and Brandes 1983). Extending this generalization to courts, I hypothesize that justices chosen from districts (*District Election*) will face less competition, and thus receive a larger proportion of the vote, than justices selected statewide. I examine district versus at-large systems rather than state size mainly because the former have been shown to influence judicial voting behavior (Hall 1992, 1995).

Also, term length (*Term Length*), which can affect the decisions of judges (e.g., Brace and Hall 1995, 1997), should influence election results. A longer term of office should make a supreme court seat more attractive, enhance competition, and reduce the electoral performance of incumbents. In state supreme courts, terms range from six to twelve years.

Control Variables and Estimation Technique

The models include two sets of important control variables. First, a dummy variable is included to control for multimember elections and the unusual situation in which more than two candidates are present in the election (*Multimember Race*). By definition, these races necessarily result in lower vote percentages. Second, dummy variables for each election cycle minus one (1980–81) are included to control for the unique effects of each election and other temporal effects in the pooled models (*1982–83, 1984–85, 1986–87, 1988–89, 1990–91, 1992–93, 1994–95*).

Given the continuous nature of the dependent variable, ordinary-least-squares regression (OLS) was used to estimate the models. To address potential complications with pooling data across states, OLS standard errors were replaced with robust standard errors. Huber/White robust variance estimators, set to recognize the panel structure of the data, are robust to assumptions about within group (i.e., state) correlation and thus are preferred.

RESULTS

An intriguing portrait of state supreme court elections is presented in tables 9, 10, and 11. Perhaps the most important conclusion from Table 9 is that retention

elections are not impervious to partisan pressures, contrary to the claims of reformers. Supreme court justices who stand for retention in states characterized by competitive party politics or in elections with partisan cross-pressures (measured with reference to gubernatorial races) receive a significantly lower proportion of positive votes. In fact, the effect of statewide partisan competition (Ranney Index) is substantial, whereas partisan challenges in the particular races have a more modest effect (reducing votes to retain by about 3%). By these indicators, independence is not evidenced.

Accountability, however, at least by one measure, is present in retention elections. Although variations in ideological distance between citizens and incumbents do not influence the electoral fortunes of justices, higher murder rates do.[13] Generally, a 10% increase in the murder rate reduces votes to retain by about 11%, a pattern consistent with aggregate-level retrospective voting. Of course, in the strictest sense, reformers would object to these types of associations.

Otherwise in retention races, candidate characteristics do not play much role in how incumbents fare with voters, and the effects of institutional arrangements are mixed. Neither minority candidates nor appointees seeking their first regular term do any better or worse with voters than do their colleagues. Also, contrary to expectations, incumbents with longer terms of office do not garner vote shares that differ significantly from incumbents with shorter terms. The type of geographic constituency is important in retention races, however. As predicted, justices elected from districts receive a higher level of support than incumbents elected statewide, by about 8%. Finally, several of the time-point dummy variables are significant, which indicates greater opposition to incumbents in these years relative to the baseline year, 1980–81. What is unique about these years warrants further investigation.

Nonpartisan elections bear a striking resemblance to retention elections, as Table 10 reveals. More specifically, nonpartisan judicial elections also fail to insulate incumbents from partisan politics or other contextual forces. Both state-level partisan competition and election-specific partisan competition have dramatic effects on the vote received by incumbents. In fact, the simple act of having a partisan challenger reduces the vote share of incumbents by about 22%, even though partisan labels do not appear on the ballot. Clearly, partisan considerations have not been eliminated from these races.

Some measure of accountability is present in nonpartisan elections. Support for incumbents varies with state murder rates: A 10% increase in the murder rate

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[13] To test for a stationarity problem with vote and murder rate, I created a trend variable and substituted it in the models for the time-point dummy variables. *Trend* is coded 8 in 1980–81 and decreased by one for each election cycle to 1994–95. In nonpartisan and partisan elections, trend is not statistically significant, which indicates no need for further diagnostics. In retention elections, trend is significant. Because these data are not a balanced panel, first-differencing, which is one possible solution, is problematic. Instead, I divided the data into two samples based on time and reran the models in retention election states. In both models, murder rate remains significant.

TABLE 9. Electoral Performance of State Supreme Court Incumbents in Retention Elections, 1980-95

	Coefficient	Robust Standard Error	t	Prob t
Ideological Distance	−0.047	0.035	−1.348	0.179
Murder Rate	−1.075	0.154	−6.989	0.000
Ranney Index	−18.931	6.455	−2.933	0.004
Partisan Challenger	−2.989	1.162	−2.572	0.011
New Appointee	0.716	0.985	0.726	0.468
Minority Candidate	−2.650	1.619	−1.637	0.103
District Election	8.252	1.501	5.497	0.000
Term Length	0.130	0.266	0.488	0.626
Multimember Race*	—	—	—	—
1982-83	2.947	1.564	1.884	0.061
1984-85	1.871	1.483	1.261	0.209
1986-87	−2.730	2.106	−1.296	0.196
1988-89	1.684	1.510	1.115	0.266
1990-91	−5.100	1.691	−3.016	0.003
1992-93	−5.876	1.977	−2.971	0.003
1994-95	−3.539	1.703	−2.077	0.039
Constant	95.865	5.673	16.897	0.000

Number of observations 234
F (15, 218) 13.96
Prob F 0.000
R^2 0.416
Root MSE 7.175

*Multimember races do not occur in retention elections.

reduces incumbents' vote share by approximately 11%. As in the case of retention elections, ideological distance does not significantly influence the relative success of incumbents in nonpartisan contests for the state high court.

Table 10 also provides some evidence—not particularly reliable—that minority candidates for the state high court in nonpartisan elections garner fewer votes than their colleagues (by about 7%). Furthermore, incumbents elected from districts receive a significantly lower proportion of the vote (about 18%) than justices chosen statewide. In fact, the effects of geographic constituency are opposite in retention and nonpartisan elections, which suggests that the effects are conditioned by election system. Why this occurs is not immediately apparent and merits further investigation. Finally, compared to 1980-81, elections in 1986-87 were significantly less competitive, as incumbents received about a 12% increase in their vote share. Otherwise, as in the case of retention elections, new appointees and incumbents with longer terms fared no better or worse in nonpartisan elections than their colleagues.

Table 11 presents the results for partisan elections. Overall, these reveal some dramatic departures from the stereotype offered by the judicial reformers. Generally, the electoral fortunes of incumbents are substantially less affected by state-level patterns of partisan competition than by partisan competition in each race. The presence of a partisan challenger lowers the incumbent's vote share by about 30%, whereas state-level partisan competition has no statistically significant effect.

The most fascinating results in Table 11 are those indicating electoral accountability in partisan elections. Unlike retention and nonpartisan systems, in partisan elections a variation in ideological distance is associated with changes in the electoral performance of incumbents. Overall, an increase of 10% in ideological distance reduces incumbent vote share by about 4%, a significant but not substantial change. Furthermore, an increase in the murder rate has a negative effect on how incumbents fare at the polls: An increase of 10% in the murder rate decreases electoral support by about 21%.

Moreover, except for the control variable for multimember races, no other variable is statistically significant, including the temporal variables. In sum, some forms of partisan politics are important in partisan judicial elections, thereby impeding independence, but

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TABLE 10. Electoral Performance of State Supreme Court Incumbents in Nonpartisan Elections, 1980–95

	Coefficient	Robust Standard Error	t	Prob t
Ideological Distance	0.108	0.089	1.214	0.227
Murder Rate	−1.182	0.362	−3.261	0.001
Ranney Index	−71.208	20.296	−3.509	0.001
Partisan Challenger	−22.105	4.748	−4.656	0.000
New Appointee	−2.689	3.003	−0.896	0.372
Minority Candidate	−7.362	3.926	−1.875	0.063
District Election	−18.226	8.200	−2.223	0.028
Term Length	−1.898	1.516	−1.252	0.213
Multimember Race	−29.444	3.781	−7.786	0.000
1982–83	0.317	5.101	0.062	0.951
1984–85	5.563	4.510	1.234	0.219
1986–87	11.992	4.036	2.971	0.003
1988–89	4.093	5.034	0.813	0.418
1990–91	3.796	4.998	0.760	0.449
1992–93	−1.145	5.478	−0.209	0.835
1994–95	0.331	6.835	0.048	0.961
Constant	163.841	15.555	10.533	0.000

Number of observations   163
F (16, 146)   29.40
Prob F 0.000
R^2 0.550
Root MSE   17.09

concerns about accountability in these elections appear to be misplaced.

DISCUSSION

One conclusion from this research is inescapable: Court reformers underestimate the extent to which partisan elections have a tangible substantive component and overestimate the extent to which nonpartisan and retention races are insulated from partisan politics and other contextual forces. As Dubois (1980, 96) succinctly notes about accountability, "the principal inquiry is whether the voting behavior of the electorate in judicial elections is 'irrational' as the critics assert, or whether it bears some relationship to the process of holding judges accountable for their decisions." Evidence now documents that judicial elections, including partisan elections, are affected by candidate- and issue-based forces. On this point, arguments of the reformers fail.

Concerning independence, court reformers assert that "judges not identified by party will escape the ebb and flow of partisan tides" (Herndon 1962, 67) and that nonpartisan and retention elections will take politics out of the judicial selection process. Evidence now suggests that this is not the case. State-level patterns of

partisan competition as well as competition unique to each contest penetrate retention and nonpartisan elections. As Squire and Smith (1988, 170) observe, retention elections may be "easily turned into partisan contests in the minds of voters," and it seems that nonpartisan elections similarly may be reconstituted. Moreover, in both nonpartisan and retention elections, the electoral performance of incumbents varies with fluctuations in murder rates, a pattern consistent with retrospective voting and inconsistent with norms of judicial independence. Perhaps as Glick (1978, 519) suggested more than two decades ago, "the Missouri Plan has produced a selection system that is much less visible than judicial elections. Yet the insulation seems only to obscure, not remove, many important partisan features and influences in judicial selection."

Through the lens of judicial reform, there may be excellent reasons not to choose judges in partisan elections. The high defeat rate, for example, may have negative consequences of all sorts. I simply assert that several propositions traditionally used to criticize partisan elections and to promote nonpartisan systems and the Missouri Plan do not survive scientific scrutiny. Therefore, it seems imperative to reassess other premises upon which the politics of institutional design have rested and to give more careful and informed

TABLE 11. Electoral Performance of State Supreme Court Incumbents In Partisan Elections, 1980–95

	Coefficient	Robust Standard Error	t	Prob t
Ideological Distance	−0.390	0.186	−2.100	0.038
Murder Rate	−2.102	1.033	−2.034	0.044
Ranney Index	−18.033	36.010	−0.501	0.617
Partisan Challenger	−30.042	4.375	−6.867	0.000
New Appointee	−3.951	3.663	−1.079	0.283
Minority Candidate	−0.702	4.876	−0.144	0.886
District Election	1.271	8.428	0.151	0.880
Term Length	−2.462	2.118	−1.162	0.247
Multimember Race	−29.798	6.880	−4.331	0.000
1982–83	1.723	6.333	0.272	0.786
1984–85	−3.458	7.118	−0.486	0.628
1986–87	−1.602	6.667	−0.240	0.810
1988–89	5.423	7.593	0.714	0.476
1990–91	−8.877	8.538	−1.040	0.300
1992–93	3.828	6.412	0.597	0.552
1994–95	−7.536	7.786	−0.968	0.335
Constant	147.625	25.810	5.720	0.000

Number of observations 144
F (16, 127) 17.14
Prob F 0.000
R^2 0.551
Root MSE 17.727

thought to the actual, not presumed, consequences of choosing one form of selection over another. For example, reformers have asserted that the negative effects of justices having to seek campaign contributions are lessened in retention elections. Yet, no systematic comparisons among systems have been made.

Regarding scholarship, this work demonstrates the importance of studying judicial elections comparatively. Quite obviously, much remains to be explained, including the effects of judicial voting records, off-bench behavior, and campaign expenditures on the ability to garner votes. Ascertaining why certain variables in this analysis did not perform as expected (such as term length) also would be an excellent start. More broadly, such crucial matters as why incumbents retire or resign rather than seek reelection, why challengers (especially quality challengers) decide to take on particular incumbents, and why incumbents lose are fruitful areas for inquiry and would add much to the dialogue about democratic processes and the American judiciary. Finally, considerations of why states adopt particular selection systems in the first place, a fascinating inquiry in its own right, also may provide important clues about significant patterns and trends that subsequently emerge in these alternative schemes.

Key (1961, 459) stated it best: "Elections matter, and

they serve in the political system as a basic connection between public opinion and government. The problem is to indicate how this linkage occurs and on what kinds of questions it seems most clearly controlling." Judicial politics scholars should take up this challenge.

APPENDIX A: DETAILED CODING OF THE RANNEY INDEX AND IDEOLOGICAL DISTANCE

Ranney Index

The original Ranney Index (Ranney 1976) was calculated for each state over a given period as an average of the proportion of seats won by Democrats in state legislative elections, the Democratic percentage of the vote in gubernatorial races, and the percentage of time the Democrats controlled the governorship and state legislature. Folded to remove partisan direction, the measure ranges from .5 (no competition) to 1.0 (perfect competition). The Ranney Index has been criticized, however, because the gubernatorial election component is a dimension distinct from the others (King 1988). Holbrook and Van Dunk (1993) recalculated the folded Ranney Index for the 1981–88 period using outcomes in state legislative elections only, thus eliminating the multidimensionality problem and creating a more robust measure.

Ideological Distance

Ideological distance is the absolute value of the difference between each justice's ideology score, measured using the Brace, Langer, and Hall (2000) party adjusted ideology (PAJID) scores, and citizen ideology at the time of each election, measured using the Berry et al. (1998) citizen ideology scores. For example, if PAJID is .75 and citizen ideology is .50, ideological distance equals .25.

More specifically, Berry et al. (1998) constructed an annual measure of the ideological preferences of each state's citizenry and each state's governmental elite by combining a variety of indicators. They calculate citizen ideology annually for each state as a function of the proportion of the electorate preferring the district's congressional incumbent (measured using election returns), the ideology of the district's incumbent (based on interest group ratings), the proportion of the electorate preferring the challenger (measured using election returns), and the ideology of the challenger (measured using election returns). These scores, calculated separately for each district, are averaged to produce a single score for each state. Berry et al. (1998) calculate elite (or government) ideology by aggregating ideology scores for the governor and the major party delegations in each house of the state legislature (generated from information about the ideology of members of Congress), based on a series of assumptions about power relationships among the various actors. Both citizen and elite ideology scores range from 0 (most conservative) to 100 (most liberal).

Brace, Langer, and Hall (2000) create a PAJID score for each party adjusted justice by weighting Berry et al. ideology scores by the justices' partisan affiliations. Specifically, for each justice Brace, Langer, and Hall first identify either the elite ideology of the state at the time a justice was first appointed or citizen ideology at the time a justice was first elected, using the Berry et al. ideology scores. They assume that the preferences of the justices will be consistent with the political context at the time of their initial selection and will more closely mirror the actor (state government or electorate) actually making the initial choice. Brace, Langer, and Hall then generate partisan weights for the Berry et al. scores by using logit to predict the partisan affiliations of the justices as a function of the initial ideology score, computing probabilities and pseudo residuals, and then multiplying the pseudo residual by ideology and adding the product to the ideology measure. Finally, they scale the scores to range from 0 (most conservative) to 100 (most liberal). Brace, Langer, and Hall demonstrate that PAJID is a valid measure of preferences that significantly outperforms partisan affiliation.

APPENDIX B: SUMMARY STATISTICS FOR EXPLANATORY VARIABLES BY TYPE OF ELECTION SYSTEM

	Mean	S.D.	Minimum	Maximum
Vote				
Retention	71.24	9.08	33.84	88.94
Nonpartisan	74.01	24.61	20.32	100.00
Partisan	67.27	23.31	16.92	100.00
Ideological Distance				
Retention	20.71	13.20	.60	55.23
Nonpartisan	18.86	16.35	.05	53.32
Partisan	8.54	7.78	.11	30.20
Murder Rate				
Retention	7.16	3.31	1.3	14.6
Nonpartisan	6.13	3.54	.6	17.5
Partisan	11.39	3.10	4.8	20.3
Ranney Index				
Retention	.84	.07	.74	.99
Nonpartisan	.88	.09	.73	1.00
Partisan	.75	.07	.64	.97
Partisan Challenger				
Retention	.29	.45	0	1
Nonpartisan	.19	.39	0	1
Partisan	.50	.50	0	1
New Appointee				
Retention	.49	.50	0	1
Nonpartisan	.27	.45	0	1
Partisan	.20	.40	0	1
Minority Candidate				
Retention	.16	.37	0	1
Nonpartisan	.19	.40	0	1
Partisan	.16	.37	0	1
District Election				
Retention	.14	.35	0	1
Nonpartisan	.08	.28	0	1
Partisan	.21	.41	0	1
Term Length				
Retention	8.09	2.21	6	12
Nonpartisan	6.91	1.37	6	10
Partisan	7.55	1.61	6	12

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Why Are Japanese Judges So Conservative in Politically Charged Cases?

J. MARK RAMSEYER Harvard University
ERIC B. RASMUSEN Indiana University

*T*heory suggests that Japanese politicians have weaker incentives than U.S. politicians to keep lower court judges independent. Accordingly, we hypothesize that Japanese lower court judges who defer on sensitive political questions will do better in their careers. To test this, we assemble several new data sets and measure the quality of the assignments received by about 400 judges after deciding various types of cases. We find that judges who deferred to the ruling party in politically salient disputes obtained better posts than those who did not, and that judges who actively enjoined the national government obtained worse posts than those who did not. We also hypothesize that judges with forthrightly leftist preferences do worse in their careers. We measure the speed at which the 500 judges hired during the 1960s moved up the pay scale and find indications that judges who joined a leftist group were promoted more slowly than their peers.

Although the judiciary is as much a branch of government as the executive and legislature, in most modern democracies it prides itself on its independence from voter preferences. In turn, many voters take pride in their lack of power over the judiciary, at least until it does something they dislike. This is interesting in itself, but of even more interest is how the organization of courts affects their independence.

Modern governments use a variety of ways to structure courts. Some appoint judges for life to a single position, some subject them to elections, and others appoint them at a young age to a judicial bureaucracy that rotates them through a variety of posts. Scholars have studied appointment-for-life regimes most closely, if only because that is the U.S. federal court organization. Indeed, the social scientific literature on U.S. courts is voluminous. Much excellent work focuses on how politicians decide whom to appoint (e.g., Cameron, Cover, and Segal 1990; de Figueiredo and Tiller 1996), how and when members of a court may act strategically with respect to one another (e.g., Cooter and Ginsburg 1996; Segal 1997; Spiller and Gely 1992), and how and when the court as a whole may act strategically with respect to statutory reversal by the legislature (e.g., Atkins and Zavoina 1974; Caldeira, Wright, and Zorn 1999; Easterbrook 1982; Revesz 1997; Songer, Segal, and Cameron 1994).

Outside the United States, by far the most common judicial systems are bureaucratic. In such systems, the

government generally taps young jurists by examination rather than political connection. In the course of their training, their performance is monitored by more experienced judges to prevent slacking or bias. Yet, precisely because senior judges have that power, the courts are potentially vulnerable to indirect political pressure.

We will examine the Japanese judiciary. Unlike many countries with bureaucratic courts, Japan does not send politically charged disputes to special constitutional courts. In cases involving the government, however, Japanese judges routinely validate what the government has done. The Japanese Supreme Court is legendary for seldom voiding statutes. Although lower courts defer slightly less, they also parrot the moderately conservative positions of the longtime incumbent Liberal Democratic Party (LDP).

The reason Japanese Supreme Court justices uphold LDP positions is straightforward: For most of the postwar period they have been recent LDP appointees. Why lower court judges would uphold LDP positions is less obvious, since the government appointed them straight out of law school with relatively little information about their political leanings. All else equal, the government should have found itself saddled with at least a substantial minority of heterodox judges. Yet, heterodox opinions generally did not follow, and we argue that the explanation lies in the career structure of the courts. We know that the Japanese courts use job postings as incentives. Elsewhere, for example, we have found that judges who write administrative law opinions that are reversed receive worse transfers, as do those who acquit criminal defendants on formalistic grounds or who acquit leftist politicians of violating electoral campaign laws (Ramseyer and Rasmusen 1997, 1999b, 2001b).

Using new data on about 400 judges, we will explore the career effect of controversial opinions in a range of politically charged headline-grabbing disputes. We first locate proxies for a judge's seniority, intelligence, effort, and ideology. Holding those proxies constant, we examine the careers of (1) judges who held either the Self-Defense Force (SDF) or U.S. bases unconstitutional; (2) judges who rejected electoral apportionment schemes advantageous to the LDP; and (3)

judges who often enjoined the national government in administrative law suits. Systematically, we find that they suffered in their careers.[1] We conclude by exploring whether judicial salaries are correlated with political affiliation. Using career data on the 500 or so judges hired between 1959 and 1968, we find evidence that leftist judges are indeed promoted more slowly than conservatives.

We know of no other scholars who have used a multivariate approach to test systematically the effect of politics on judicial careers in a bureaucratic system. Moreover, to our knowledge this article is the first to use Japanese career data to study judicial independence in a range of disparate but politically sensitive disputes. It is also the first to use the data to test for a political bias in pay.

TOWARD A POSITIVE THEORY OF JUDICIAL INDEPENDENCE

The Puzzle

Before we explain the institutional structure of the Japanese courts and explore the connection between public law opinions and judicial careers, we will outline the conditions under which a government could be expected to keep judges genuinely independent. Although voters elect politicians to deliver policies, they do not expect them to do all the work themselves. They expect them to hire agents—generally, bureaucrats—to see the policies through. They also expect the politicians to prevent the agents from promoting policies they dislike.

Given the desire to remain in office, elected officials monitor their bureaucratic agents with care. Not every tax agent will perform every audit perfectly; the optimal level of agency slack in government is not zero any more than it is in private business. As do managers of private firms, however, politicians devise mechanisms (or "fire alarms," McCubbins and Schwartz 1984) to alert them to serious slack. In private business, managers do this to increase shareholder profits. In government, politicians do it to deliver policies voters want.

Judges are just another set of agents. They can be elected, as in some U.S. state judiciaries, or appointed by politicians, as in Japan, the U.S. federal judiciary, and most of the rest of the world. Politicians can readily discipline misbehavior by most appointed bureaucrats, subject to the constraints of civil service laws that the politicians themselves pass. The puzzle of judicial independence is why politicians apparently do not discipline judges similarly. Why would politicians find it advantageous to control one set of agents (bureaucrats) but let another set (judges) run free? Why would voters reelect politicians who do nothing to

stop judges from blocking the policies for which they elected the politicians in the first place?

Research suggests several reasons rational politicians might not use career incentives to control judges. First, perhaps politicians find it hard to make their promises credible. Whether in selling regulatory rents to lobbyists or in promising policies to voters, they have an incentive to renege on their commitments after the fact. By delegating dispute resolution to independent judges, they may increase the credibility of their initial promises (Landes and Posner 1975).[2]

Second, perhaps by giving citizens the right to sue misbehaving bureaucrats, politicians can use the courts to keep bureaucrats in line. Suppose politicians worry that bureaucrats may try to deflect this "fire alarm" by leaning on judges. If so, then they may want to keep courts strictly independent (McCubbins and Schwartz 1984).

Third, perhaps politicians hope to mitigate their losses from losing elections. Although they could increase their power as majority politicians if they constrained judges, that power would come at a cost. What they now do to the opposition, the opposition may do to them later (Ramseyer 1994).

All else equal, we therefore expect courts to be less independent if the majority party (1) can credibly commit to policy through means other than the courts, (2) can detect misbehaving bureaucrats through mechanisms other than the courts, and (3) can expect to continue winning elections.

Why Study Japan?

The Empirical Problem. A straightforward way to test these hypotheses would involve regression analysis on data across countries and time, but scholars have not yet collected the necessary information for countries other than the United States. Therefore, we focus on one country: Japan. Data availability is crucial, because official pronouncements cannot be taken at face value. On the issue of judicial independence, modern governments present a united front: They are for it. They maintain a constitutional framework that promises judges independence from politics, and they collect no data that show the contrary. Politicians claim, and most local law professors agree, that the judges are indeed independent.

The U.S. Federal Example. We do not wish to exaggerate the risk of political bias. The three hypotheses above largely suggest that U.S. federal politicians would want to keep judges independent, and evidence suggests they usually do. Caveats aside,[3] no matter how

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[1] Elsewhere (particularly Ramseyer and Rasmusen 1999a), we explore more fully whether these effects result from the politically charged character of the disputes, from a nonpolitical bureaucratic response to innovation by the judges involved, or from inaccuracy in judicial interpretation. We find that the motivation for punishment is primarily political.

[2] This argument hinges in part, of course, on whether courts interpret statutes in light of the enacting politicians' preferences rather than those of current politicians, a proposition for which empirical studies (e.g., Eskridge 1991) tend not to find evidence.

[3] One can overstate the point. Congress does have some controls (e.g., the reach of court jurisdiction, the number of judges, the timing of increases in judicial salaries; George and Epstein 1992; Wasby 1988) and has manipulated judicial careers on a few occasions (e.g., the Jefferson-Adams battle over the midnight appointments; Rosenberg 1992, 380).

U.S. federal judges decide their cases, most will spend the rest of their career deciding the same kinds of cases, sitting in the same cities, and earning no more and no less than their peers. A few dream of promotion to a higher court, but the effect of that incentive remains modest.[4]

The Japanese Example. The Japanese Constitution also guarantees independence: "All judges are independent in the exercise of their conscience and bound only by this constitution and the laws" (Art. 76, Sec. 3). Most observers would agree with Japanese law scholar John Haley (1998, 98) that "the political branches of government have long ignored the courts and the judge-administrators of the system have worked hard to preserve that judicial autonomy."

Yet, each of the three hypotheses suggests that Japanese politicians would seek to constrain judges. First, the majority LDP maintains an internal structure that readily enables it to make its commitments credible: Party affairs are centralized under the control of senior politicians from safe districts, control over policy is delegated to them, and they are regularly paid enormous amounts of legal and illegal cash. The result was a majority party controlled by leaders who earn efficiency wages in a long-term indefinitely repeated game (Ramseyer and Rosenbluth 1997, 7). Necessarily, these are players with incentives to maintain their reputation. Necessarily, when these leaders promise policy their promises are credible.

Second, through its local organizations the LDP maintains its own fire alarms for detecting bureaucratic misbehavior. For decades, Japanese voters elected their politicians from multimember districts under a single nontransferable vote system. As a result, to capture a majority of the Diet, a majority party needed to elect multiple representatives from most districts. That in turn required it to divide its supporters among several candidates. Rather than do this by ideology, the LDP used candidate-specific support groups that dispensed pork and provided ombudsman services. In part, therefore, candidates gave voters some bureaucratic interventionist services directly (Ramseyer and Rosenbluth 1997, chap. 2, 113).

This system left the LDP with little reason to encourage citizens to use the courts to complain about bureaucrats in any dispute of moment. Indeed, because voters from LDP districts often could obtain the help they needed from their representatives, those who sued the government over substantial issues may have hailed disproportionately from non-LDP districts. By disabling the courts as a means of controlling bureaucrats in cases that raise significant policy issues, the LDP may even have not decreased but increased voters' gains to returning its candidates to office.

Third, the LDP could rationally expect to stay in power. The probability was less than 1, to be sure, as it discovered in 1993. But that loss was a surprise, the result of brinkmanship by party factions over how to reposition the party (Ramseyer and Rosenbluth 1997, Preface, chaps. 2, 5). From 1955 to 1993, the LDP maintained steady control over the Diet and could rationally expect that situation to continue.

THE STRUCTURE OF THE JAPANESE COURTS

The Supreme Court

Even the U.S. Supreme Court does not invalidate legislation as a matter of course, but the Japanese Supreme Court is deferential in the extreme. As of 1993, it had held legislation unconstitutional only about a half-dozen times in its entire history (Haley 1998, 179–80; Okudaira 1993, 20). The reason is straightforward. Almost all the justices were recent LDP appointees, and the party passing the legislation was the LDP.

Given the frequent political turnover in America, U.S. presidents try to stack the Supreme Court with relatively young justices to take advantage of lifetime tenure. This produces the motley ideological array that Americans take for granted: The Court includes both Democrats and Republicans as well as justices (because they often serve 20 years or more) who dramatically change their political preference since their appointment.

LDP leaders faced a different political environment. During most of the postwar period, they tightly controlled the party, which controlled the Diet, and no opposition party had a significant chance of coming to power. Virtually all justices except a few carryovers from Katayama's short-lived Socialist cabinet of 1947–48 were conservative appointees. Because the LDP expected to stay in power, its leaders could afford to appoint justices old enough (generally in their early 60s) not to change their views before mandatory retirement at age 70 (Ramseyer and Rosenbluth 1997, chap. 8).

Although the prime minister largely rubber-stamps Supreme Court nominees selected by a group within the career judiciary, that is irrelevant. The group only nominated people they knew the prime minister would approve. Many postwar justices came from the bar and the universities, but they were hardly a random sample of talented lawyers. During much of the period, the bar and especially the universities in Japan—as in other wealthy democracies—were disproportionately left of center. Had the nominating judges looked only to raw talent, they would regularly have proposed leftists, but they never tried. The prime minister could safely rubber-stamp nominees because the nominators knew he could just as easily reject them.

The Lower Courts

The real puzzle is not the conservatism of the Japanese Supreme Court but of the more than 2,000 judges in

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[4] Compare, e.g., Higgins and Rubin 1980 (potential promotions do not affect judicial behavior) with Anderson, Shughart, and Tollison 1989 (a positive relation between state supreme court justice salary and the tendency to overturn statutes), Cohen 1991 (potential promotions do affect judicial behavior), Rosenberg 1992 (U.S. Supreme Court responds to a wide variety of threats from Congress), and Toma 1991 (U.S. Supreme Court budget affects opinions).

the district courts, high courts, and family courts. Unlike U.S. federal judges, Japanese lower court judges are not appointed with verifiable political histories. Instead, they are ordinarily named in their late 20s, straight out of the national law school, the Legal Research and Training Institute (LRTI). It is hard to predict what a young appointee's political beliefs will be decades later.

From time to time, observers have tried to explain lower court deference to government by the prewar autocratic legacy or the purportedly greater deference in civil-law judiciaries generally. Neither explanation works. Japanese politics and government differ radically today from prewar years. The legacy argument cannot explain why this aspect of modern government is affected but not others. Some modern European courts defer to the government less than do Japanese courts, notwithstanding their shared civil-law tradition.

Appointment and Reappointment

The reason for Japanese lower court deference lies in the internal structure of the courts. American federal trial judges have a job for life. Absent egregiously bad behavior or senility—and perhaps even then—they can work as long as they want. Unless they quit by choice or are promoted to the Court of Appeals, they will sit in the same court in the same city for their entire judicial career and collect the same salary as all other federal trial judges.

By contrast, after their initial appointment Japanese judges are reassigned every few (generally three) years.[5] A hypothetical judge, Ichiro Tanaka, illustrates the typical pattern. During his first three years he works as a trial judge in the Osaka District Court. He then is transferred, first to the Sendai High Court, then to the family court branch office in Miyazaki, and from there to the Ministry of Justice in Tokyo. He may spend a stint teaching at the LRTI or working in the Secretariat, the administrative offices of the court system.[6] So long as Tanaka is reappointed every ten years, as are virtually all judges, he will have a job in the judiciary until age 65. Yet, the quality of this job will depend crucially on how the judges in the Secretariat rate his work. Even his pay may hinge on their evaluation. They cannot constitutionally cut his salary, but they have no obligation to give him prestigious jobs or move him up the pay scale at the same rate as everyone else.

By all accounts, most Japanese judges find administrative duties prestigious and branch office assignments embarrassing. Like the vast majority of professionals, they want to live in Tokyo if possible and in Osaka if not. Like the vast majority of humans, they prefer higher pay to lower. The fact that prestige, geography,

and pay depend on performance should induce them to work hard and carefully, and by all accounts they usually do.

At least indirectly, however, this system presents the potential for political manipulation. LDP prime ministers appoint moderately conservative justices to the Supreme Court; they give them the job of supervising the Secretariat; and they usually keep on the Court at least one justice who previously headed the Secretariat and knows its workings intimately. The Secretariat, in turn, decides which judges will go to what cities, who will hold which prestigious administrative jobs, who will spend how many years in branch offices, and who will climb the pay scale at what rate.[7] The question is whether LDP leaders use this potential political influence.

Note three additional features of the courts. First, Japanese courts do not use juries. All trials are bench trials, with the judge deciding questions of fact as well as law. Second, most trials are conducted by three-judge panels, routine nonserious criminal trials being the exception. Third, lower court opinions are signed by the entire panel. Even if a judge dissents, that fact is not publicly disclosed.

SAMPLES

The Project

A systematic examination of how public law opinions affect judicial careers requires a study of disputes that involve large numbers of judges. A dispute in which only one or two lower court panels are engaged provides anecdotal evidence of political influence, and scholars have detailed these anecdotes in both Japanese and English (e.g., Kashimura 1991; Miyazawa 1991; Ramseyer and Rosenbluth 1997, chaps. 8–9; Sakaguchi 1988; Tsukahara 1991). Although the anecdotes suggest that judges who flout the political preferences of the LDP receive worse assignments, such evidence is inconclusive. Too many judicial transfers have nothing to do with politics. Many are simply random. The Secretariat worries about corruption of judges by organized crime, and the easiest way to reduce the likelihood is to move judges regularly. Other transfers are incentives for effort. The Secretariat cares whether judges work or shirk, and assignments can be used to reward or punish. Even the most pro-LDP judges may spend time in branch offices and provincial cities if they shirk.

To determine whether the Secretariat uses job assignments to punish and reward judges for the opinions they write, we need a systematic multivariate approach. Accordingly, we focus on disputes for which a relatively large number of opinions were written. We then code these by a political metric and whether they were

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[5] For more detail, see Ramseyer and Rosenbluth 1997, chaps. 8–9.
[6] The Secretariat is staffed by career judges. It is headed by the secretary general (also a career judge, but one on a fast track; unless he makes serious mistakes he has high odds of being named to the Supreme Court soon), who answers only to the Supreme Court. The LDP has potential indirect control over the courts, through its control over the cabinet, which names Supreme Court justices. The LDP has no direct control.

[7] No legislative body in Japan plays the oversight role of the U.S. Senate Judiciary Committee. Because of the LDP's hold over the Diet, oversight is primarily played out within the party and executed indirectly through the appointment of party loyalists to the Supreme Court.

reversed, and we ask whether judges' decisions help explain the appointments they later received. More precisely, we estimate the quality of a judge's post-public-law opinion job postings, denoted *Job*, through a regression equation:

$$Job = a + B_1 Opinion + B_2 Controls + e.$$

Opinion is a vector of variables that describe the judge's public law opinion—for example, whether it followed LDP policy or was reversed on appeal. *Controls* is a vector of control variables that proxy for the judge's seniority, ideology, effort, and intelligence.

Why would opinion ever take an antigovernment value if judges know that job would be affected? If we are correct that such behavior hurts careers, then it should not happen in equilibrium. Opinion—our independent variable—should not be totally independent but should depend on the size of expected punishment. This is not a serious concern here for two reasons. First, because most of the heterodox opinions we study date from the 1960s and 1970s, a period when the LDP was still institutionalizing its career structure, judges could not yet be certain whether they would be punished. Although judicial decisions today may indeed be endogenous to the expected punishment, that is less a problem the earlier the decision. ·

Second, some judges simply will not sacrifice principle for career in even the small number of politically sensitive cases that come before them. Their behavior is effectively independent of career concerns (those observations in our data sets provide the variance for our statistical tests).

We will investigate three sets of politically charged disputes for our opinion variables: the constitutionality of the military, malapportionment, and injunctions against the government. If we find that opinions in only one category of dispute affect job quality, then we might conclude either that it involves a particularly sensitive area for politicians or that the result is an accident of the data. If we find that opinions in each set of disputes consistently affect job quality, then we can safely conclude that judges face politically biased incentives in politically charged cases. As a supplementary test, we will investigate the effect of a judge's membership in a leftist bar organization on the speed of promotion, which indirectly tests the effect of political affiliation on pay.

The Data Sets

Sources. We assemble data from several sources. For judicial opinions, we rely on the *Hanrei taikei* (Dai-ichi various years), which resembles the American Westlaw and Lexis and includes virtually all post–World War II published opinions on CD-ROMs. For judicial careers, we use the *Zen saibankan keireki soran* (Nihon minshu 1998), a book that details all job postings for judges educated after World War II. For membership in the communist-leaning Young Jurists League (YJL) as of 1969, we use *Osorubeki saiban*, which copied the list

from the league's own newsletter (Shiso 1969). We present selected summary statistics in Appendix A.[8]

Constitutionality of the Military. Our first set of cases involves the constitutionality of the Japanese military. Article 9 of the Constitution proclaims that "land, sea, and air forces, as well as other war potential, will never be maintained." By any but the most tortured interpretation, this bans the SDF. Consistently, the LDP has claimed it does not. By no stretch of the imagination could Douglas MacArthur, godfather of the clause, have thought it banned U.S. bases. Occasionally, the opposition has said it does.

Each time the Supreme Court faced a challenge to the SDF or American bases it refused to hold either unconstitutional (Beer 1996). From time to time, however, lower court judges did. We found 25 district court opinions that addressed Article 9, three of which held either the SDF or the bases unconstitutional (the source of our key independent variable). As we cannot code opinions and careers about which we have insufficient data, here and elsewhere we drop (1) unsigned opinions, (2) judges who do not appear in Nihon minshu (1998) (generally judges educated before the war), and (3) judges with less than eighteen months of experience before the opinion or less than 2.5 years' experience afterward. Through this process, we obtained a set of 47 judges who wrote opinions on Article 9.[9]

Malapportionment. Our second data set concerns electoral apportionment, which is a chronic issue in Japanese courts. Through the 1960s and into the 1970s, the LDP relied heavily on the rural vote, but farm families were steadily migrating to metropolitan centers. As a result, the LDP gained by stalling reapportionment. By keeping the old rules, it maximized the number of representatives from heavily LDP rural districts.

Increasingly, LDP leaders recognized that delaying reapportionment was a bad strategy for the long term. Sooner or later, the LDP would have to create a new identity as a party for urban consumers. Many in the rank-and-file, particularly Diet members from the rural districts, fought this change. Into the 1980s there was internal turmoil between the leaders, who eventually would lose their power if the party did not reposition itself, and the rank-and-filers who would immediately lose their jobs if it did (Ramseyer and Rosenbluth 1997, chap. 3).

Faced with challenges to the existing apportionment schemes, the Supreme Court wrote opinions that generally tracked the positions of LDP leaders. During the first period, it rejected challenges to the rural overrepresentation. In the 1979 case of *Kurokawa* v. *Chiba*,[10] however, it switched sides. By this point, the LDP

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[8] The data and STATA programs for the regressions are available at Ramseyer and Rasmusen 2001a and at the *APSR* web site, linked to our abstract.

[9] Three of them wrote two opinions each. To avoid improperly weighting judge-specific effects for these judges in our regressions, we include dummy right-hand-side variables for each.

[10] *Kurokawa* v. *Chiba ken senkyo kanri iinkai*, 808 Hanrei jiho 24

leaders were pushing the party to jettison the agricultural vote. The plaintiffs in *Kurokawa* claimed that some votes counted five times as heavily as others, and the Court held the apportionment unconstitutional. In the process, it helped the LDP leaders who otherwise would have found it harder to force LDP Diet members to redistrict themselves potentially out of a job. In 1985, in *Kanao v. Hiroshima*, the Court reiterated the point: Rural overrepresentation was unconstitutional.[11] By then, LDP leaders were solidifying the party's position as an urban party and abandoning the farmers to the socialists and communists. Again, the Supreme Court strengthened their hand.

Given this shift in the position of the LDP leadership and the Supreme Court, one would not expect the Secretariat always to punish judges for holding apportionment rules unconstitutional.[12] Instead, one would expect judges to be punished only if they either (1) held an apportionment scheme unconstitutional before the lower court opinions in *Kurokawa* (1974) and *Kanao* (1984) or (2) held an apportionment scheme unconstitutional and found that opinion reversed on appeal.

To test these hypotheses, we begin with the 69 lower court opinions that raised the propriety of electoral apportionment schemes, whether on constitutional or statutory grounds.[13] By law, most electoral challenges begin at the intermediate appellate level, so the judges in this data set were already in somewhat prestigious positions at the time of their decisions. Among the cases, 54 involved challenges to national elections and 15 to local elections. We coded the cases according to whether they invalidated the apportionment scheme, antedated *Kurokawa* (1974) or *Kanao* (1984), involved the local or national government, and were reversed on appeal.[14]

Injunctions against the Government. In administrative litigation, a plaintiff who can show the potential for irreparable harm can obtain a preliminary injunction

that stops the government from doing the harmful action at least until the underlying lawsuit is finally decided.[15] Do judges risk their careers in granting such injunctions?

One would not expect careers to be jeopardized simply for deciding routine administrative cases against the government. As noted earlier, to stay in office the LDP not only must enact the policies voters want but also must deliver them, and for that it needs dependable bureaucrats. Yet, bureaucrats can shirk their job and ignore instructions. Although LDP politicians maintain staffs that provide some bureaucratic interventionist services directly, they do not necessarily want to intervene in every tax audit and taxi license revocation. To discipline bureaucrats in these more mundane—or politically delicate—disputes, they allow citizens to sue them (McCubbins and Schwartz 1984). For that mechanism to work, in turn, they need a cohort of relatively unbiased judges.

In several crucial ways, the LDP facilitates legal challenges in mundane administrative cases. First, Japanese "standing" rules generally permit challenges to bureaucratic decisions that are too minor to warrant direct intervention by the · LDP or the local Diet representative. Second, to ensure an impartial forum in these ordinary administrative disputes, the Secretariat does not punish judges simply for favoring plaintiffs who challenge the government (Ramseyer and Rasmusen 1999b). Third, because accuracy matters if the courts are to monitor the bureaucracy, the Secretariat punishes judges whose decisions are reversed on appeal by the higher courts (Ramseyer and Rasmusen 1999b).

Injunctions against the government, however, can be decidedly nonroutine. It is one thing to hold that a taxpayer owes only X in back taxes rather than the $2X$ dishonestly claimed by a bureaucrat trying to fill a quota. It is quite another to block government policy. Because national bureaucrats answer to the cabinet, if LDP leaders want a national agency to stop doing action Y, they can simply tell it to stop and fire the agency head if it does not. In cases important enough to prompt politicians to intervene, a court that orders an agency to desist from doing Y thus directly jeopardizes LDP-mandated policy, since the judge cannot be fired. Therefore, one might plausibly suspect that judges who readily enjoin the national government jeopardize their career.

By the same logic, a judge would not face this threat for enjoining local governments. During the 1960s, the LDP increasingly lost control at this level. By 1975, only 12.5% of mayors ran on an exclusively LDP ticket.[16] As a result, even if the Secretariat punished judges for enjoining LDP policy, we should not observe the punishment among judges who enjoined local governments.

To test these hypotheses, we coded all published

(Sup. Ct. Apr. 14, 1979) (en banc), *rev'g* 30 Saihan minshu 288 (Tokyo High Ct. April 30, 1974); see Haley 1998, 179–80.

[11] *Kanao v. Hiroshima ken senkyo kanri iinkai*, 1163 Hanrei jiho 3 (Sup. Ct. July 17, 1985 (en banc), *aff'g* 1134 Hanrei jiho 27 (Hiroshima High Ct. September 28, 1984). The Court needed more than one opinion to make the point forcefully because of the fact-specific nature of the problem. The Court did not require that every vote have exactly the same effect, so several opinions were needed to clarify just how much variation in electoral power it would allow.

[12] And it did not. When we created a general variable equaling 1 if a judge held any apportionment scheme improper and ran a regression with the same control variables as in Table 4, we obtained coefficients and standard errors of −.040 (0.17) (for the bad jobs afterward regression) and .018 (0.09) (for the good jobs afterward regression); the effect on careers of striking down rural overrepresentation, averaged over the entire period, is nil.

[13] This data set includes 89 judges. Eight of them wrote two opinions. As we do elsewhere for judges who wrote multiple controversial opinions, we use judge-specific dummy variables.

[14] Among the observations in our sample, 2% are invalidations before 1974 and 31% after, with 67% being opinions that uphold the apportionment. Seven percent are invalidations before 1984 and 26% after. Four percent are invalidations that were reversed, and 1% are validations that were reversed. This sample is composed of more senior judges than those in the Article 9 sample (a mean of 23.01 years compared to 13.12 years of seniority), who have commensurately better initial jobs.

[15] *Gyosei jiken sosho ho*, Law No. 139 of 1962, § 25.

[16] Ramseyer and Rosenbluth 1997, 48, Table 3.3. This was a factor that led LDP leaders to reposition the party away from the overrepresented rural districts.

administrative cases from 1961 to 1970 in which a petitioner demanded a preliminary injunction. We restricted observations to one decade to limit the potential length of time between multiple injunctions issued by any one judge (the career effect of two injunctions over thirty years should be quite different from two over ten years) and because injunctions were common enough to provide a good-sized sample even over one decade. We used cases from the 1960s because judges issued fewer preliminary injunctions thereafter, which was a reasonable response to what we will show below.

This process generated a data set of 130 judges.[17] If a judge handled several injunctive petitions, we coded his career by the most recent year in which he granted an injunction. We located only five opinions in which a higher court reversed the grant of an injunction, so we did not include a reversals variable. For each judge, we counted the number of national and local injunctions granted and the number denied (our key independent variables).

Political Affiliation and Pay. Does the punishment against political nonconformists extend to salaries? During their first ten years on the bench, Japanese judges climb through the twelve steps of the assistant judge pay scale, which ranged as of 1989 from 190,600 to 405,600 yen per month. During the rest of their career, they move through another nine steps—from 494,000 yen (step 8), to 912,000 (step 3), to 1,115,000 (step 0). Although the Constitution protects them from explicit pay cuts, the Secretariat need not promote all judges at the same rate. If it is unhappy with a judge's work, it need not grant any promotion whatsoever.

Although a judge's salary is confidential, it correlates with certain observable indices. Most important, according to some observers a judge can serve as *sokatsu* (an administrative post with some personnel responsibilities) only after reaching step 3 (Netto 1995, 204). If so, then the time from initial appointment to this assignment will reflect, however imperfectly, the amount of time taken to reach step 3 in pay.

In our data set are all judges hired between 1959 and 1968. To control for unobservable differences among the cohorts, we added dummy variables indicating the year in which a judge finished education at the LRTI. As the dependent variable, we used the time it takes a judge to reach first sokatsu appointment. See Appendix B.

THE VARIABLES

Dependent Variables

Good Jobs Afterward is the percentage of the decade after a potentially controversial opinion that a judge spends in prestigious appointments (as chief judge, with sokatsu responsibilities, or in another administrative post). *Bad Jobs Afterward* is the percentage of the

decade after the opinion that a judge spends in a branch office (other than the relatively desirable Hachioji office in suburban Tokyo).

Because these dependent variables are censored, only taking values between 0 and 100, we use tobit rather than ordinary least squares (OLS) in our regressions (using the program Stata 5). We use one-tailed tests throughout, since we hypothesize that antigovernment behavior hurts careers. We use linear specifications, but as diagnostics for robustness we report for each regression whether the significance of any of the opinion variables changes if (1) the observations with the three largest residuals in the reported regression are dropped, (2) a log-linear specification is used, or (3) a log-log specification is used (where log $(1 + x)$ rather than log (x) is used because of the many zeroes in our data).

Note that the variables good jobs afterward plus bad jobs afterward will not sum to 100 for an individual judge, because not all jobs are good or bad. Most are mediocre. Our interest is in carrots and sticks, not benign neglect.

Time to Sokatsu is the number of years after a judge's graduation from the LRTI to first appointment as sokatsu. Because this dependent variable is uncensored, we use OLS for the relevant regression.

Opinion Variables

The Constitutionality of the Military. *SDF Unconstitutional* is coded 1 if a judge held either the SDF or U.S. bases unconstitutional, 0 otherwise.

Malapportionment. *Invalidation before 1974* is coded 1 if a judge held a national apportionment scheme illegal before the 1974 trial court opinion in *Kurokawa,* 0 otherwise.[18] *Invalidation after 1974* is coded 1 if a judge held a national apportionment scheme illegal in or after the 1974 trial court opinion in *Kurokawa,* 0 otherwise. *Invalidation before 1984* is coded 1 if a judge held a national apportionment scheme illegal before the 1984 trial court opinion in *Kanao,* 0 otherwise. *Invalidation after 1984* is coded 1 if a judge held a national apportionment scheme illegal in or after the 1984 trial court opinion in *Kanao,* 0 otherwise. *Invalidation Reversed* is coded 1 if the Supreme Court reversed a judge's opinion that held a national apportionment scheme illegal, 0 otherwise. *Validation Reversed* is coded 1 if the Supreme Court reversed a judge's opinion that held a national apportionment scheme legal, 0 otherwise.

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[17] By contrast, from 1971 through 1980, there were only 20 reported district court opinions that granted preliminary injunctions; from 1981 through 1997, there were 17.

[18] We also ran the first four regressions in Table 2 using a variable that combined judges involved in all apportionment challenges, whether local or national. Because many local electoral schemes benefited parties other than the LDP, one would expect the punishment effect to be less pronounced. The coefficients and standard errors on a variable equal to 1 if the judge held improper any (national or local) pre-*Kurokawa* apportionment scheme were 1.32 (0.47) (bad jobs afterward) and −.689 (0.33) (good jobs afterward); for pre-*Kanao* apportionment schemes, they were .453 (0.24) (bad jobs afterward) and −.046 (0.15) (good jobs afterward).

Injunctions against the Government. National Injunctions Granted is the number of injunctions against the national government granted by a judge during 1961–70. *National Injunctions Denied* is the number of injunctions against the national government denied by a judge during 1961–70. *Local Injunctions Granted* is the number of injunctions against a local government granted by a judge during 1961–70. *Local Injunctions Denied* is the number of injunctions against a local government granted by a judge during 1961–70.

Control Variables

Good Jobs Before is like the variable good jobs afterward, but for the decade before the opinion (this captures various otherwise unobserved information about the judge). *Bad Jobs Before* is like the variable bad jobs afterward, but for the decade before the opinion. *Seniority* is the number of years between the opinion and the year a judge graduated from the LRTI. *Flunks* is the number of times a judge failed the LRTI entrance exam (the pass rate varied between 1% and 4%), which is an inverse proxy for intelligence and work habits. *Elite College* is coded 1 if a judge graduated from either of the two most prestigious universities (Tokyo or Kyoto), 0 otherwise. This is a proxy for intelligence and work habits that also captures any old-school ties. *Opinions per Year* is a judge's average productivity for the decade before the opinion, as measured in published opinions per year on the bench. This also is a proxy for intelligence and work habits. *YJL* is coded 1 if a judge was a member of the YJL as of 1969, 0 otherwise. *Tokyo Start* is coded 1 if a judge began his career at the Tokyo District Court (a mark of fast-track status), 0 otherwise.

RESULTS

Control Variables

In all regressions below, when the control variables are significant, they have the predicted signs. Consistently, they reflect both the meritocratic organization of the Japanese courts, and the use of career incentives to reduce judicial shirking. The regressions are presented in tables 1–4. The variable good jobs before (which captures otherwise unobserved information about a judge's status) has a positive effect on the variable good jobs afterward. The more administrative responsibilities a judge had before deciding a controversial case, the more he had afterward (Table 1, regression 1.2, and Table 3, regression 3.2).

Seniority has a positive effect on the variable good jobs afterward and a negative effect on the variable bad jobs afterward. Administrative responsibilities tend to go to the more senior judges, and branch office assignments go to the younger judges (Table 1, regression 1.2; Table 2, regressions 2.2, 2.4, 2.6; Table 3, regressions 3.1, 3.2).

The variable flunks is negatively correlated with the variable good jobs afterward. Judges who fail the LRTI exam the fewest times (the smartest and the hardest

TABLE 1. Job Quality by Article 9 (Military) Opinions and Control Variables (Tobit Regressions)

Independent Variable	Dependent Variable	
	(1.1) Bad Jobs Afterward	(1.2) Good Jobs Afterward
Unconstitutional	−.028 (.208)	−.397 (.223)*
Good Jobs Before		.729 (.344)*
Bad Jobs Before	−.073 (.299)	
Seniority	.011 (.009)	.019 (.011)*
Flunks	.026 (.018)	−.018 (.020)
Elite College	−.256 (.119)*	.201 (.113)*
Tokyo Start	.071 (.157)	.231 (.143)
Opinions per Year	−.100 (.042)*	−.023 (.015)
YJL	.140 (.253)	.099 (.289)
Intercept	.177 (.179)	.061 (.196)
Pseudo R^2	0.46	0.55
Standard error	0.29	0.31
Diagnostics (outliers, log-lin, log-log)	(c,c,c)	(c,s,c)
Censoring ($y < 0$, unc., $y > 1$)	(26,24,0)	(11,34,5)

Note: $N = 50$. Coefficients are followed by standard errors in parentheses. *$p < .05$; one-tailed tests. These regressions include dummies for the three judges with multiple opinions, but the coefficients are not reported. Diagnostics show whether the significance of unconstitutional is confirmed (c) or switched (s). See page 337 for details.

working) have the most administrative responsibilities, even beyond the effect this has on their career before the controversial case (Table 3, regression 3.2).

The variable elite college has a positive effect on the variable good jobs afterward and a negative effect on the variable bad jobs afterward. Judges from the prestigious universities of Tokyo and Kyoto spend the most time in administrative roles and the least time in branch offices—again, beyond the effect their education has on their career before the decision (Table 1, regressions 1.1, 1.2; Table 2, regressions 2.1, 2.3, 2.5; Table 3, regressions 3.1, 3.2).

The variable Tokyo start has a negative effect on the variable time to sokatsu. Judges identified as the most promising at the outset spend the most time at administrative jobs and climb the pay scale the most rapidly, as shown in Table 4.

The variable opinions per year has a positive effect on the variable good jobs afterward and a negative effect on the variable bad jobs afterward. Judges who publish the most opinions spend the most time in administrative jobs

TABLE 2. Job Quality by Malapportionment Opinions and Control Variables (Tobit Regressions)

Independent Variable	(2.1) Bad Jobs Afterward	(2.2) Good Jobs Afterward	(2.3) Bad Jobs Afterward	(2.4) Good Jobs Afterward	(2.5) Bad Jobs Afterward	(2.6) Good Jobs Afterward
Invalidation before 1974	1.36 (.470)**	−.710 (.330)*				
Invalidation after 1974	−.018 (.163)	.018 (.094)				
Invalidation before 1984			.607 (.271)*	−.243 (.173)		
Invalidation after 1984			−.173 (.204)	.036 (.104)		
Invalidation Reversed					.980 (.368)**	−.380 (.224)*
Validation Reversed					.448 (.505)	−.199 (.389)
Good Jobs Before		−.053 (.212)		−.016 (.214)		−.026 (.212)
Bad Jobs Before	−.129 (.363)		−.183 (.392)		−.258 (.386)	
Seniority	.010 (.011)	.020 (.010)*	.015 (.012)	.020 (.010)*	.013 (.012)	.020 (.010)*
Flunks	.001 (.029)	−.017 (.020)	−.010 (.033)	−.016 (.020)	−.014 (.033)	−.016 (.020)
Elite College	−.389 (.176)*	.135 (.100)	−.276 (.179)	.111 (.102)	−.339 (.178)*	.120 (.101)
Tokyo Start	.191 (.207)	−.107 (.127)	.218 (.222)	−.111 (.129)	.241 (.212)	−.108 (.014)
Opinions per Year	−.084 (.033)**	.031 (.014)*	−.096 (.040)*	.027 (.014)*	−.100 (.040)*	.029 (.014)*
YJL	−.174 (.272)	.030 (.142)	−.116 (.296)	.029 (.146)	−.131 (.285)	.031 (.144)
Intercept	.003 (.282)	−.036 (.207)	−.069 (.302)	−.043 (.211)	.006 (.296)	.047 (.204)
Pseudo R^2	0.33	0.30	0.31	0.29	0.33	0.29
Standard error	0.45	0.36	0.48	0.37	0.47	0.37
Diagnostics (outliers, log-lin, log-log)	(c,c,c)	(c,c,c)	(c,c,c)	(c,c,c)	(c,c,c)	(c,s,s)
Censoring ($y < 0$, unc., $y > 1$)	(71,24,1)	(13,64,19)	(71,24,1)	(13,64,19)	(71,24,1)	(13,64,19)

Note: $N = 97$. Coefficients are followed by standard errors in parentheses. *$p < .05$, **$p < .01$; one-tailed tests. These regressions include dummies for the 8 judges with multiple opinions, but the coefficients are not reported. Diagnostics show whether the significance of the opinion variables is confirmed (c) or at least one switched (s). See page 337 for details.

and the least time in branch offices (Table 1, regression 1.1; Table 2, regressions 2.1–2.6).[19]

Article 9: The SDF

The best-known constitutional dispute in Japan is the argument over Article 9 of the Constitution. The regressions in Table 1 measure the career effects of a judge's decision on that issue. Crucially, the coefficient on the variable unconstitutional in the good jobs afterward regression (1.2) is negative and significant at −.397. Compared to judges who ruled the SDF or U.S. bases constitutional (or who ducked the issue), those who held either of them unconstitutional received fewer prestigious administrative duties in the decade after the opinion.

The effect of the variable unconstitutional coefficient on the variable bad jobs afterward is not statistically significant. To punish a judge, the Secretariat can use

either longer assignments to branch offices or shorter administrative appointments, and we have no theory about when it uses one or the other. Here, it seems to have cared more about keeping erring judges out of administrative positions.

Malapportionment

Table 2 confirms our hypothesis about the malapportionment cases. Before the switch in position of the LDP leadership (and the Supreme Court) on the issue, judges who upheld the constitutionality of national apportionment rules did better than those who struck them down; after that switch, the effect disappeared. Judges who held a national apportionment scheme improper during the years when the LDP depended on the rural vote were punished: The variable invalidation before 1984 in regression 2.3 is significant and has a positive effect on the variable bad jobs afterward.[20]

[19] Ramseyer and Rasmusen (1997, 272) explain why a low opinions-per-year score is more properly a cause of inferior assignments than a result.

[20] The effect is even stronger for the very earliest opinions (invalidation before 1974), but we do not base our analysis on that because of the small number of observations.

TABLE 3. Job Quality by Injunctions and Control Variables (Tobit Regressions)

Independent Variable	Dependent Variable	
	(3.1) Bad Jobs Afterward	(3.2) Good Jobs Afterward
National Injunctions Granted	−.038 (.073)	−.119 (.070)*
National Injunctions Denied	−.132 (.065)*	.076 (.040)*
Local Injunctions Granted	.051 (.057)	−.003 (.050)
Local Injunctions Denied	.075 (.073)	.046 (.067)
Good Jobs Before		1.074 (.257)**
Bad Jobs Before	.207 (.220)	
Seniority	−.035 (.008)**	.045 (.008)**
Flunks	.009 (.013)	−.039 (.013)**
Elite College	−.158 (.081)*	.127 (.071)*
Tokyo Start	−.114 (.145)	.056 (.127)
Opinions per Year	.021 (.027)	−.026 (.025)
YJL	.031 (.112)	−.116 (.127)
Intercept	.371 (.115)**	−.223 (.110)*
Pseudo R^2	0.22	0.44
Standard error	0.37	0.34
Diagnostics (outliers, log-lin, log-log)	(c,c,c)	(c,s,s)
Censoring ($y < 0$, unc., $y > 1$)	(64,66,0)	(61,65,4)

Note: N = 130. Coefficients are followed by standard errors in parentheses. *$p < .05$, **$p < .01$; one-tailed tests. Diagnostics show whether the significance of the opinion variables is confirmed (c) or at least one switched (s). See page 337 for details.

Unlike the Article 9 cases, here the punishment seems to have operated mainly through more branch office time rather than less time in prestigious postings. As expected, for the years after 1984, when the LDP leadership decided to jettison its rural base, we find no evidence of any punishment against judges who held the schemes improper: The variable invalidation after 1984 is insignificant.

Table 2 also illustrates a related phenomenon. If the Supreme Court reversed an opinion that invalidated an apportionment scheme, the judge spent more time in branch offices and less time with administrative responsibilities.[21] The variable invalidation reversed in regressions 2.5 and 2.6 has a significant positive effect on the variable bad jobs afterward and a negative effect on the variable good jobs afterward. Note that a judge suf-

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[21] On why the punishment is not just a bureaucratic response to those who went against the Supreme Court, see the discussion in Ramseyer and Rasmusen 1999a.

fered no significant penalty for having the validation of an apportionment scheme reversed.

These regressions help resolve an issue that the Article 9 regressions could not address. A possible explanation for the Article 9 regressions is that the Secretariat was simply run by judges who were conservative both in politics and judicial temperament. As a result, they hesitated to use Article 9 to weaken national defense, even though they were completely independent from LDP pressure. That disposition, however, would not explain a Secretariat indifferent to rural overrepresentation until the mid-1970s or 1980s but bothered by it thereafter. The timing suggests that the courts were following the LDP leadership.

Preliminary Injunctions

Tale 3 confirms that judges who enjoin the national government jeopardize their career. The coefficient on the variable national injunctions granted in regression 3.2 is negative and significant: Judges who enjoined the national government received fewer administrative responsibilities over the ensuing decade. This contrasts sharply with the fate of judges who denied injunctions. The coefficient on the variable national injunctions denied is positive in regression 3.2 and negative in regression 3.1; judges who refused to issue injunctions against the national government received additional administrative assignments and spent less time in branch offices. The results also confirm the predicted distinction between national and local bureaucrats. In both regressions 3.1 and 3.2, the granting or denial of injunctions against local governments had no significant effect on a judge's career.

These regressions also help distinguish among different explanations for judicial behavior. First, the Secretariat is not judicially conservative, unwilling to thwart the actions of elected officials or to intervene in political issues. Rather, it seems to be conservative with respect to the national government and neutral with

TABLE 4. 1959–68 Judges: Time to Sokatsu Promotion by YJL Membership and Control Variables (OLS)

Independent Variable	Dependent Variable: Time to Sokatsu
YJL	.919 (.538)*
Tokyo Start	−1.383 (.804)*
Flunks	.014 (.071)
Elite College	.086 (.516)
Intercept	22.905 (.785)**
Diagnostics (outliers, log-lin, log-log)	(c,s,s)
R^2	.11

Note: N = 501. Coefficients are followed by standard errors in parentheses. *$p < .05$, **$p < .01$; one-tailed tests. Dummies for the year the judges were hired were included but are not reported. Time to Sokatsu is the number of years to the judge's first sokatsu appointment from the time he graduated from the LRTI. Diagnostics show whether the significance of YJL is confirmed (c) or switched (s). See page 337 for details.

respect to the local government—just as the LDP would have wanted. Second, the explanation for punishment cannot lie exclusively in whether judges' decisions were reversed. Some of those punished in the Article 9 and malapportionment samples were indeed reversed on appeal, and we know from other studies that reversals can hurt careers. That is not, however, what occurred in these injunction cases. Instead, the explanation again lies in politics.

Political Affiliation and Pay

Table 4 reports the results of our investigation into the effect of political affiliation on pay. Notably, the coefficient on the variable YJL is positive and significant. Judges who joined the league in the 1960s received their first sokatsu assignment a year later than their peers. If, as observers claim, the appointment signals promotion to step 3 on the pay scale, then YJL members did indeed climb the pay scale more slowly than their peers.

One caution is that the variable YJL becomes insignificant if logarithmic specifications are used. This suggests that perhaps the significance in the basic regression (which is confirmed in the diagnostic regression with outliers dropped) is due to a long delay in sokatsu for a relatively small number of judges in this large sample, an effect that is larger in a linear specification than a logarithmic one.

CONCLUSION

To test whether Japanese politicians indirectly influence judges, we used newly assembled data and a multivariate approach. We asked whether judges who flout the ruling party in politically volatile cases pay a career penalty.[22] They do. In the case of SDF constitutionality, judges who sided with the LDP received better assignments than those who did not. In the

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[22] It goes without saying that the vast majority of cases in any legal system have nothing to do with electoral politics. The indirect political manipulation involved in these politically charged cases says nothing about potential bias in private law cases or even (see Ramseyer and Rasmusen 1999b) routine cases involving the government.

apportionment debate, those who held existing schemes valid during the time when the LDP relied on overrepresented rural districts did better than those who did not. Judges who granted injunctions against a national agency did worse than those who denied them. The politically biased penalties even extended to money: Judges who joined the leftist YJL climbed the pay scale more slowly than their more conservative peers.

Granted, very few judges flouted the LDP. Of the 47 judges who ruled on Article 9, only five held the military unconstitutional. Among the 89 judges who addressed electoral districting, only 7 struck down the rules before the mid-1980s. The small number of judges who flouted LDP policies raises the question of whether those who did shared some characteristic correlated with unsuccessful careers. In fact, however, this problem is less severe than it might appear. In our controls, we use variables (good jobs before and bad jobs before) that largely capture the effect of any omitted variables correlated with career success. Because we look not for bad careers but for careers that become worse after a decision, omitted variables would explain our results only if they were correlated with events that occurred the same year as the decision in question.

Moreover, precisely because of the small number of heterodox judges in some of our samples, we repeated the tests on a variety of independent data sets. In all politically sensitive sets of cases, we obtained the results we predict. In related studies, we find significant punishment of judges who held unconstitutional the ban on door-to-door canvassing, who acquitted criminal defendants on formalistic grounds, or whose tax opinions were reversed on appeal (Ramseyer and Rasmusen 1997, 1999b, 2001b). Indeed, not all our tests involve small numbers of judges anyway. Among the 130 who ruled on petitions for preliminary injunctions, 42 enjoined the national government. Among the 500 judges hired between 1959 and 1968, 140 had joined the leftist bar association. To us, this uniformity across a wide variety of data sets suggests that we capture far more than a statistical anomaly. We capture indirect political manipulation.

APPENDIX A: SUMMARY STATISTICS

Variable	Minimum	Median	Mean	Maximum
A. Article 9 (Military) Data Set ($N = 50$)				
Good Jobs Afterward	0	0.39	0.44	1
Bad Jobs Afterward	0	0	0.17	0.92
Unconstitutional	0	.0	0.12	1
Good Jobs Before	0	0	0.22	0.93
Bad Jobs Before	0	0	0.09	0.8
Seniority	2	11	13.12	29
Flunks	0	3	3.84	12
Elite College	0	1	0.56	1
Tokyo Start	0	0	0.24	1
Opinions per Year	0	2.23	3.36	29.75
YJL	0	0	0.12	1
B. Malapportionment Data Set ($N = 97$)				
Good Jobs Afterward	0	0.64	0.60	1
Bad Jobs Afterward	0	0	0.10	1
Invalidation before 1974	0	0	0.02	1
Invalidation after 1974	0	0	0.31	1
Invalidation before 1984	0	0	0.07	1
Invalidation after 1984	0	0	0.26	1
Invalidation Reversed	0	0	0.04	1
Validation Reversed	0	0	0.01	1
Good Jobs Before	0	0.31	0.39	1
Bad Jobs Before	0	0	0.12	0.73
Seniority	5	24	23.01	39
Flunks	0	2	3.00	9
Elite College	0	0	0.46	1
Tokyo Start	0	0	0.20	1
Opinions per Year	0	4.85	5.88	22.22
YJL	0	0	0.12	1
C. Preliminary Injunctions Data Set ($N = 130$)				
Good Jobs Afterward	0	0.07	0.26	1
Bad Jobs Afterward	0	0.09	0.21	0.9
National Injunctions Granted	0	0	0.37	3
National Injunctions Denied	0	0	0.50	7
Local Injunctions Granted	0	0	0.40	4
Local Injunctions Denied	0	0	0.40	2
Good Jobs Before	0	0	0.06	0.75
Bad Jobs Before	0	0	0.11	0.81
Seniority	2	8	9.42	22
Flunks	0	4	4.32	16
Elite College	0	0	0.46	1
Tokyo Start	0	0	0.09	1
Opinions per Year	0	1.49	2.07	10.67
YJL	0	0	0.12	1
D. Sokatsu Data Set ($N = 501$)				
Time to Sokatsu	11	21	21.71	37
YJL	0	0	0.28	1
Tokyo Start	0	0	0.10	1
Flunks	0	4	4.83	18
Elite College	0	0	0.32	1

APPENDIX B

For our Table 4 regression, we made several further adjustments. First, in those rare cases in which a judge served as chief judge before serving as sokatsu, we treated appointment to the chief judgeship as the sokatsu posting. Such an appointment is unambiguously higher than a sokatsu post. Second, we dropped those judges who held nonjudicial postings (generally regarded as prestigious) within two years before their first sokatsu posting. A judge in such a post could not have held sokatsu duties but might well have been at step 3 already, and including the time to first sokatsu in the database would exaggerate the time required for his promotion.

Of course, by dropping such judges entirely we bias our data. Since the judges were among the more successful in their cohort, removing them from the sample exaggerates our estimate of the group's true time to sokatsu. Crucially, however, Ramseyer and Rasmusen (1997) find that YJL members did not receive these prestigious administrative responsibilities as often as their peers. As a result, dropping these star judges disproportionately drops non-YJL judges. In the process, we understate the success of the non-YJL judges and concomitantly bias our data against finding anti-YJL discrimination.

Third, we dropped judges who never obtained a sokatsu appointment but who quit or died before the mean time to first sokatsu for the rest of the group (20.41 years). This omitted another 164 judges. Of these, 23.8% were YJL members.

Fourth, judges who quit or died after 20 years without a sokatsu appointment were kept in the database, but the death or resignation was treated as the first sokatsu appointment. We applied this procedure to 83 judges, 33.7% of whom were YJL members.

Although this last adjustment also biases the data, it again biases it against finding anti-YJL discrimination. The fact of quitting during the first 20 years of a career tells us little about how well a judge was doing. By contrast, those who quit at a time when many of their peers are serving as sokatsu but they are not can be considered relatively unsuccessful. Yet, by treating their resignation as equivalent to a sokatsu appointment, we overstate their professional success. Because they never received a sokatsu appointment, the length of their career will necessarily be shorter (or equal to) the length of time it would have taken them to reach sokatsu had they not quit. Crucially, this group of unsuccessful judges included a disproportionately high fraction of YJL members (33.7%). By making this adjustment, we therefore underestimate the true time to first sokatsu for YJL members.

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Cabinet Decision Rules and Political Uncertainty in Parliamentary Bargaining

JOHN D. HUBER and NOLAN McCARTY   Columbia University

We investigate how cabinet decision-making rules interact with political uncertainty to affect the outcomes of bargaining processes in parliamentary systems. Our formal models compare two types of decisions rules: (1) those that give prime ministers unilateral authority to demand a vote of confidence and (2) those that require prime ministers to obtain collective cabinet approval for confidence motions. We examine these models under assumptions of complete information and of political uncertainty, that is, party leaders lack information about the precise policies that others in the governing coalition will support. Our analysis suggests that the nature of the cabinet decision rules should influence the distribution of bargaining power, the ability to exploit political uncertainty, the likelihood of inefficient government terminations, the circumstances surrounding such failures, and, indirectly, the political considerations that parties face when choosing prime ministers during government formation. Simple empirical tests support some of these insights.

In parliamentary democracies, if members of the governing coalition have diverse policy interests, they must bargain and compromise 'to achieve policy change after government formation is complete. The outcomes from these bargaining processes are quite diverse. There is variation, for example, in whether they result in successful policy change or in bargaining failure and government termination. If successful policy change occurs, there is variation in the nature of the policy agreements themselves, and some coalition partners seem to do better than others in their efforts to influence policy outcomes. If bargaining failure occurs, there is variation in the actual mode of failure. Sometimes, for instance, the government voluntarily resigns before even attempting to shepherd a new policy initiative through parliament. At other times members of the governing coalition proceed with parliamentary debate on a bill, only to suffer a dramatic defeat and be forced to step down.

What explains the various outcomes that can occur from bargaining among members of a divided coalition? What determines, for example, whether bargaining will end in policy change or government termination? Why do some members of a governing coalition seem to have an upper hand over others in the bargaining processes that determine policy outcomes? Why do some prime ministers seem weaker than others? And why do some governments, when faced with conflict, give up without a fight, whereas others take the battle to the floor of parliament, only to lose and be forced out of office?

We address these questions by focusing on a distinction that exists in the institutional arrangements for cabinet decision making, on the one hand, and on the strategic incentives created by political uncertainty, on

the other. The institutional arrangements we examine affect how decisions are made in the cabinet and parliament after government formation is complete. Specifically, we examine limits that can exist on the authority of prime ministers to demand confidence votes on the policies they espouse. In some countries, they can unilaterally demand a vote of confidence on any policy they wish. In other countries, they can only demand a vote of confidence on a policy after obtaining the collective approval of the cabinet. We argue that this distinction between unilateral authority and collective approval has a significant effect on parliamentary bargaining processes and ultimately on the policies enacted and the stability of governing coalitions.

Political uncertainty refers to the lack of information that party leaders often have about the precise policies that other participants in the governing coalition will support on the floor of parliament. Whenever a new policy issue arises, each member of the governing coalition must decide which policy changes are acceptable, that is, which policy changes they would prefer to accept rather than end the government. Since the precise nature of these acceptable policies is private information, participants in the governing coalition may at times have incentives and opportunities to make exaggerated claims about the specific policy concessions of other partners necessary to preserve the coalition. If a coalition participant goes too far, however, and rigidly insists on concessions that other members of the government have no intention of making, then inefficient bargaining failures can occur, that is, a government termination may happen even though an identifiable majority collectively prefers that it does not.

We explore how cabinet decision rules and political uncertainty together interact to affect bargaining outcomes. To this end, we examine three aspects. First, we describe variation in the institutional structures of the confidence relationship in parliamentary systems, and we use two brief examples of bargaining failure from Norway and the Netherlands to discuss how the procedures can influence the outcomes of bargaining

processes.[1] Second, we analyze two game theoretic models that allow us to compare the effects of unilateral confidence procedures to the effects of confidence procedures that require collective cabinet approval. We examine these models under the assumptions of incomplete information to determine the effect of political uncertainty on bargaining outcomes. Third, we discuss the substantive implications of the results and provide some empirical evidence in support of our arguments.

CABINET DECISION RULES, POLITICAL UNCERTAINTY, AND BARGAINING FAILURES

Models of bargaining in parliamentary systems typically focus on the government formation process. Their objective is to understand either which parties enter government coalitions (e.g., Austen-Smith and Banks 1988; Baron 1991, 1993) or which parties receive specific portfolios within the government (e.g., Austen-Smith and Banks 1990; Laver and Shepsle 1990, 1996). These models implicitly assume that policy outcomes are determined at the time of government formation, and therefore it is unnecessary to examine the dynamics of bargaining processes that occur after formation is complete.

More recently, scholars have begun to analyze post-election bargaining processes. Lupia and Strøm (1995) examine a model of coalition termination in which an exogenous event initiates bargaining among parties that can lead to maintenance of the status quo, to the formation of a new government, or to an election. Tsebelis and Money (1995, 1997) explore how the institutions of bicameralism affect strategic bargaining processes between an upper and lower house. Tsebelis (1999) and Bawn (1999) investigate how diversity in the preferences of coalition partners affects the adoption of significant policy changes. Strøm (1994) examines bargaining processes among opposition parties that are attempting to bring down the government. Baron (1998), Diermeier and Feddersen (1998), and Huber (1996b) develop models that analyze the effect of confidence procedures on various aspects of parliamentary bargaining processes.

We address two limitations of this literature. First, no model examines institutional variation in cabinet decision-making processes, and a central objective of our analysis is to compare explicitly the effects of unilateral autonomy for the prime minister on confidence votes with collective cabinet decision rules. Second, each model except that of Strøm (1994) assumes bargaining agents have complete information about the preferences of other actors.[2] Thus, if governments fall during the course of parliamentary bargain-

ing, such terminations are always "efficient" in that they are preferred by a majority to any alternative that keeps the government in power. We relax this assumption of complete information in our analysis of the two different types of cabinet decision rules.

To see how institutional variations may effect coalition bargaining, we consider countries in which the prime minister can act unilaterally to make a vote on a particular policy a vote on the continued existence of the government.[3] In such systems, if members of parliament adopt or threaten to adopt a bill that the prime minister does not like, s/he can make his or her preferred policy a question of confidence. This forces the parliament either to accept the prime minister's policy or to bring the government down.

As the following example from Norway illustrates, the unilateral confidence procedure is a powerful weapon, but its use carries with it considerable risk for the prime minister. In early March 2000, Norway's minority government, led by Prime Minister Kjell Magne Bondevik, a Christian Democrat, was attempting to push through parliament a policy that would permit the development of a major technology park. The two major opposition parties, Labour and Progress, refused to support it. On March 7 Bondevik threatened to make the issue a question of confidence in the government. The opposition parties backed down, which allowed development of the technology park to proceed.

The next day, the *Financial Times* argued that Bondevik's position was strengthened by this victory. The newspaper predicted that his government was now unlikely to lose on the next conflictual agenda item, which involved the construction of several new gas-fired power plants. Bondevik wanted to delay the projects until new antipollution technology could be evaluated, whereas the major opposition parties wanted to begin construction immediately. Bolstered by his victory on the technology park, Bondevik made his policy on the power plants an explicit question of confidence. The motion was defeated on the floor, which ushered in a leftist government that excluded the Christian Democrats.

As this example illustrates, the unilateral confidence procedure presents a strategic dilemma to prime ministers. Its use allows them significant influence over policy outcomes, but a defeat may cost them their job.

It is interesting to contrast the bargaining dynamics in this example with bargaining processes in systems that require collective cabinet approval on confidence motions.[4] In such countries, if the partners in government withhold approval, the prime minister cannot make the final policy proposal a confidence issue. Instead, either s/he must resign (if s/he does not support the policy) or the bill proposed in parliament is voted against the status quo, and a defeat results in

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[1] For a more general model of the distributive consequences of the allocation of veto and proposal rights, see McCarty 2000.

[2] Although Strøm (1994) considers the effect of incomplete preference information on bargaining outcomes, he does not examine the cabinet decision rules that are central to this article, and he does not model the process by which political uncertainty may be resolved, which is a primary purpose of this work.

[3] These countries include Australia, Belgium, Canada, Denmark, France, Germany, Ireland, New Zealand, Norway, Portugal, and the United Kingdom (Huber 1996b, Table 1).

[4] These countries include Finland, Italy, Luxembourg, Netherlands, Spain, and Sweden (Huber 1996b).

maintenance of the status quo (but not government failure).

As the following example from the Netherlands illustrates, collective cabinet decision rules create a strategic dilemma not so much for the prime minister as for the governing partners. In 1982, a majority coalition was formed between the Christian Democratic Appeal (CDA) and the People's Party for Freedom and Democracy (VVD), with the CDA's Ruud Lubbers as prime minister. This coalition governed Dutch politics for most of the 1980s, but the collaboration broke down in May 1989, when the two parties clashed over the financing arrangements for a major plan to reduce environmental pollution. The CDA wanted to increase gas taxes and eliminate tax deductions for car commuters, whereas the VVD strongly opposed the tax increases and preferred to use general government revenues to pay for the plan. The VVD threatened to submit a no-confidence motion if its policy demands on financing were not met. Rather than cede to these demands, Lubbers resigned on May 2.

At the request of the queen, Lubbers stayed on until new elections could be held in September. Under this caretaker government, parliament adopted the CDA's policy on the environment without VVD support. In the September elections, the VVD suffered its worst showing since 1973, whereas the CDA maintained its seat share in parliament and formed a majority coalition with the Labour Party (PvdA), which made it possible for Lubbers to remain as prime minister.

In both of these examples, it seems that political uncertainty (as defined above) played a role in the bargaining failures that occurred, but it worked quite differently under the two cabinet decision rules. In Norway, the prime minister's apparent miscalculation about what policies could be adopted using the confidence procedure led to his downfall. In the Netherlands, the VVD apparently miscalculated by pushing for its preferred energy policy, which led to resignation by the prime minister. In both cases, the consequences were severe for the agents that miscalculated—Bondevik lost his job, and the Christian Democrats (in Norway) and VVD (in the Netherlands) lost their membership in the governing coalition.

Although these cases illustrate how political uncertainty can influence bargaining under the two types of cabinet decision rules, the examples are incomplete because we can never know precisely what was going on in the minds of the participants. Did Bondevik prefer to lose his job rather than compromise on the power plants issue? Did the VVD prefer to leave the government and suffer an electoral defeat rather than compromise on energy taxes? It seems unlikely that we can get reliable answers to such questions, and the participants may have thought their actions would lead to different consequences. We should also note that both examples focus on bargaining failures. Although such failures are interesting, they are only one type of outcome. As a purely empirical matter, they are much more rare than successes, as government coalitions typically are able to adopt new policies rather than break up.

Instead of examples, we need a clear and testable theory of the dynamics of strategic bargaining under the two cabinet decision rules when political uncertainty exists. We will develop formal models of each procedure and then offer two simple tests, one of a direct implication of our theory, and one of an indirect (and more speculative) implication about the choice of prime minister during government formation.

MODELS OF UNILATERAL AND COLLECTIVE CABINET DECISION-MAKING PROCEDURES

Both of our models analyze interactions between two players, a *Prime Minister*, P, and the *Coalition Partner*, C. The latter may be a cohesive group of deputies (such as the Euro-Rebels in Britain's Conservative Party under John Major) or a party (such as the VVD in the Dutch example). In our model, C is the pivotal member of the governmental majority with whom the prime minister must bargain. The partner can ensure that a bill passes or that the government falls in a vote of confidence. The two players bargain to determine a policy outcome in a unidimensional policy space, although their roles differ subtly according to cabinet decision rules. To enhance readability, we assign a female gender to the prime minister and a male gender to the coalition partner.

The unilateral model (the prime minister can invoke a confidence motion without cabinet approval) is summarized in Figure 1A. In the initial stage, the coalition partner proposes a bill, b, which is any policy on the real line. This is the policy that takes effect if the prime minister does not invoke a confidence vote. The prime minister reacts to the partner's bill in one of three ways: (1) acceptance (ending the game, with b as the outcome); (2) resignation (ending the game, and retaining the status quo policy), or (3) unilateral invocation of a confidence vote procedure to propose any other policy, z. If the prime minister uses a confidence vote, then the coalition partner decides whether to accept or reject z. If the partner accepts z, then z is the outcome. If z is rejected, the government falls, and the status quo, x_0, is retained.

The collective cabinet model (the prime minister must obtain cabinet approval for a confidence motion) is depicted in Figure 1B. The game begins with the coalition partner proposing b. The prime minister can (1) accept b or (2) make a motion in the cabinet that policy z be treated as a question of confidence. At this stage, the major difference with the unilateral model occurs. If the prime minister makes a motion of confidence, then the partner must decide whether to approve or reject it in the cabinet. Thus, we assume that the coalition partner is in the cabinet, an assumption that is not necessary in the unilateral model.[5] If the

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[5] Under collective rules during coalition minority government, we assume that the pivotal coalition partner is in the opposition (see discussion below). In this case, we treat the unilateral model as appropriate because the prime minister need not obtain the opposition party's approval in the cabinet in order to proceed with a

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FIGURE 1. Sequence of Interactions in the Two Models

A. THE UNILATERAL MODEL

| *C* proposes a bill, *b* | *P* can:
• accept *b* (ending the game);
• resign (ending the game); or
• propose *z* using a confidence motion. | If *P* proposes *z*, then *C* can:
• accept *z* (ending the game);
• reject *z* (ending the game with a government termination). | |

| *C* proposes a bill, *b* | *P* can:
• accept *b* (ending the game); or
• propose *z* to the cabinet using a confidence motion. | If *P* proposes *ż*, *C* can:
• accept *z* (ending the game);
• reject *z*. | If *C* rejects *z* in the cabinet, then *P* can:
• resign (ending the game with a government termination); or
• allow *b* to be adopted. |

B. THE COLLECTIVE CABINET MODEL

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partner accepts the motion, then the outcome is the prime minister's motion, z.[6] If the partner rejects the motion, the prime minister can either allow b to be adopted on the floor or resign, preserving the status quo policy.

The utility functions for the prime minister and her coalition partner have several components. First, each player's policy preferences are represented by strictly quasi-concave utility functions, $u_P(\cdot)$ and $u_C(\cdot)$. The prime minister has an ideal point of x_P, and the coalition partner has an ideal point of x_C. Without loss of generality, let $x_P < x_C$.

Second, in the unilateral game only, we assume that if the prime minister uses a confidence vote procedure she may pay some exogenous "electoral cost" or "audience cost," $e \geq 0$. This cost, the motivation for which

confidence motion. An extension of the model would involve three players to cover this situation: the prime minister, a partner in the cabinet, and a partner in parliament.

[6] We show in the Appendix that a formal voting stage on the floor is superfluous once the motion passes in the cabinet.

is described in detail by Huber (1996a), is due to the fact that the confidence vote procedure is a dramatic event that attracts a great deal of attention, and it often leads to the perception that the prime minister has no true majority for her policy and therefore must use "procedural force" against members of her own majority. All else equal, then, a prime minister would prefer to obtain her policy goals without resorting to this dramatic procedure. We assume $e = 0$ under collective cabinet decision rules for the obvious reason that a partner cannot claim the prime minister is using procedural force against it because the partner has to approve the confidence vote in the cabinet in the first place.

Third, we assume in both models that the prime minister and partner may value the continued existence of the government, which implies that if the game ends in a government termination, both players may pay some positive cost (which for P and C is due to the time and risk associated with facing an election or forming a new government, and which for P is also due to the

FIGURE 2. Strategic Interactions under the Unilateral Procedure
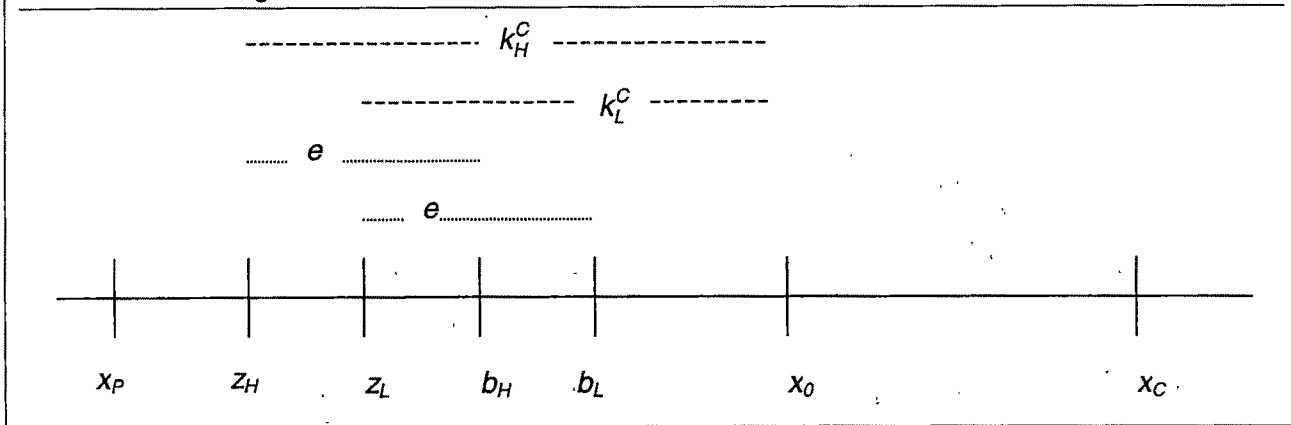
costs of being fired). Let k_t^P be the prime minister's termination cost, which is a function of P's type. These costs are known to the prime minister but not her partner and are either high ($t = H$) or low ($t = L$). Thus, $0 < k_L^P < k_H^P$. "Nature" determines these costs, and the partner believes that P has high costs with probability q_0. Upon observing a confidence vote strategy by the prime minister, the partner updates his beliefs by Bayes's rule, with q_1 representing the updated belief that the prime minister has high termination costs.

Similarly, the partner's government termination costs, k_t^C, are private information, known only by the partner. These costs can be either low ($t = L$) or high ($t = H$), with $0 < k_L^C < k_H^C$. The prime minister's ex ante belief that C has high costs is p_0, and (upon observing the partner's initial policy proposal) her updated belief is p_1.

We assume that these termination costs are nonnegative because, if this were not true for either player, then that player should simply resign, bringing down the government and recouping the benefits of so doing. We also assume that the prime minister's termination costs are large relative to the audience costs of using a confidence motion, so that $e < k_L^P$. This assumption seems appropriate substantively (we might expect the electoral cost of simply invoking a specific procedure to be smaller than the costs of losing one's job), and it eliminates the trivial case in which the prime minister always prefers to accept her partner's ideal point rather than propose the best policy that she can obtain using a confidence vote procedure.

These assumptions about costs of government termination allow us to analyze the effect of political uncertainty on bargaining processes. In particular, both players have private information about which specific policy outcomes are preferable to government termination. Consequently, each player will attempt to exploit this information in order to obtain the largest possible policy concessions from the other. Yet, in doing so, both players risk a government termination that both would prefer to avoid.

We use the concept of perfect Bayesian equilibrium in these signaling models, focusing on pure strategies

where they exist. Although the technical details of the analysis are somewhat complex, the central substantive insights that we have discovered from analyzing the models typically are not. Therefore, the formal analysis of the models is relegated to the Appendix, and we will concentrate on conveying the substance and logic of our main results. These concern the distributive consequences of the two rules as well as the role of uncertainty in influencing bargaining strategies and bargaining failures.

THE UNILATERAL CONFIDENCE VOTE PROCEDURE

If the prime minister decides to use a confidence motion, she will propose the policy z she most prefers from those policies that her partner will accept. To see how this policy is determined, consider Figure 2, in which we assume that the players have linear policy utility functions. If the partner votes against any z, he obtains the policy utility associated with the status quo, x_0, and incurs termination costs. Assume that the costs are k_L^C. In this case, the partner will accept any confidence motion that is closer to his ideal point than z_L. Since the prime minister knows the partner's termination costs (by the assumption of complete information), z_L is the best policy that the prime minister can propose using a confidence motion. Using the confidence procedure, however, requires the prime minister to incur a (nonnegative) audience cost, e. In Figure 2, then, the prime minister will accept the partner's initial policy, b, if it yields a higher utility than would adopting z_L and incurring this audience cost. Thus, in a subgame perfect equilibrium, the partner will adopt his preferred policy from those the prime minister will accept. This is b_L in Figure 2.

The prime minister uses her "last move advantage" to extract the coalition partner's government termination costs in policy concessions. The partner can stem these losses to a certain extent by using his "first move advantage" to extract concessions equivalent to the prime minister's audience costs. Unless these costs are unusually large ($e > k_t^C$), the threat of a confidence motion always gives the prime minister the upper hand

in the bargaining process. That is, she always can exploit the partner's termination costs to make herself better off and even to make the partner worse off in policy terms.

Next, consider the effect of political uncertainty. If the prime minister does not know her partner's termination costs, she cannot be sure which confidence proposals will be accepted. By trying to extract too many policy concessions (i.e., to extract k_H^C when the partner's true costs are k_L^C), she will trigger an unwanted government termination (because her confidence motion will be rejected on the floor). We can see the logic of the prime minister's strategic dilemma in Figure 2. As noted, if the partner has low termination costs, the best policy the prime minister can achieve using a confidence vote is z_L. By a similar logic, the best policy she can obtain if the partner's termination costs are high is z_H in the figure. If the prime minister demands a confidence motion on the safe policy, z_L, she knows she cannot be defeated. But if she tries to extract more concessions by demanding a confidence vote on z_H, she will be defeated, and the government will fall if in fact the partner's true costs are low.

Because the prime minister can always make a "safe" proposal by using a confidence motion to extract small policy concessions (k_L^C), the model yields the substantively interesting result that, under unilateral cabinet rules, resignation occurs with zero probability (so long as the reasonable assumption that $k_L^P > e$ is maintained). This is true because the prime minister always prefers to adopt the safe confidence motion rather than resign. We prove this result formally as lemma 1 in the Appendix.

The prime minister, of course, does not simply choose between adopting the risky confidence motion and adopting the safe one—she can also choose to accept the coalition partner's policy proposal, invoking no confidence procedure at all. Which of these three strategies she prefers depends on the partner's proposal and on the prime minister's updated beliefs, p_1, about the coalition partner's termination costs. In perfect Bayesian equilibria, these beliefs depend on whether the partner adopts a separating strategy (whereby each type adopts a different policy, fully resolving uncertainty), a pooling strategy (whereby each type adopts the same strategy, making it impossible to update beliefs), or a semipooling strategy (whereby the types partially pool and partially separate, allowing some learning by the prime minister to occur).

In the Appendix, we show that pooling, separating, and semipooling equilibria can exist. The properties of these different equilibria, and the circumstances under which they exist, depend on a set of conditions that involve initial beliefs, termination and audience costs, and the location of the status quo. There are a large number of cases to consider, but several substantive points emerge from the analysis of the model.[7] These

are summarized below for perfect Bayesian equilibria under unilateral cabinet decision rules.

Distributive consequences: The prime minister never makes policy concessions to the partner (relative to the status quo), but the partner often makes policy concessions to the prime minister. Consequently, the prime minister's policy utility from the final (equilibrium) outcome is never lower (and is often higher) than the prime minister's utility from the status quo.

Mode of government termination: The prime minister never resigns in equilibrium. Instead, if inefficient bargaining failures occur, it is because a confidence motion by the prime minister is defeated on the floor.

Factors leading to termination: If e is very large, terminations never occur. In other cases, terminations can occur in equilibrium only when each player places a sufficiently high probability that the opponent has high costs (i.e., p_0 and q_0 are large enough). The threshold on p_0 (above which inefficient terminations can occur) increases with k_L^C, k_L^P, k_H, and e, and it decreases with k_H^C. The threshold on q_0 (above which inefficient terminations can occur) increases with e and k_L^C and decreases with k_H^C.

The intuition for the distributive consequence is the same with uncertainty as it is when complete information exists. In the remainder of this section, we describe how political uncertainty can lead to unwanted government terminations.

We begin with an examination of the only equilibria in which no inefficiencies are generated either by incurring termination costs or audience costs. In this pooling equilibrium, the partner, regardless of his type, makes an initial proposal that is acceptable to both types of prime minister. Thus, confidence motions are never invoked, and governments never fall. For this equilibrium to exist, however, the prime minister's initial belief that the partner has high costs (p_0) must be sufficiently low. If p_0 becomes too large (relative to the prime minister's termination and audience costs), then the "strong" prime minister (whose type has low termination costs) will prefer to reject the pooled proposal and make a risky confidence motion.[8]

In the efficient equilibria just described, government terminations and confidence motions are avoided not because the partner conveys information to the prime minister through his initial proposal, but because the partner makes a proposal that is always acceptable to the prime minister, given the prime minister's prior beliefs. Another way that inefficient government terminations are avoided is for the partner to use his initial policy proposal to reveal his type in a separating equilibrium. In this equilibrium, a partner with low termination costs makes an initial proposal that is closer to his preferred policy than is the initial proposal of a partner with high termination costs. Returning to Figure 2, for example, the initial proposal could be b_L

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[7] For formal statements and proofs, see propositions 1–5 in the Appendix.

[8] Another factor in P's decision to defect to the risky proposal would be the difference between k_H^C and k_L^C, as this difference determines the policy gains available from the risky proposal.

FIGURE 3.   Strategic Interactions under the Collective Procedure

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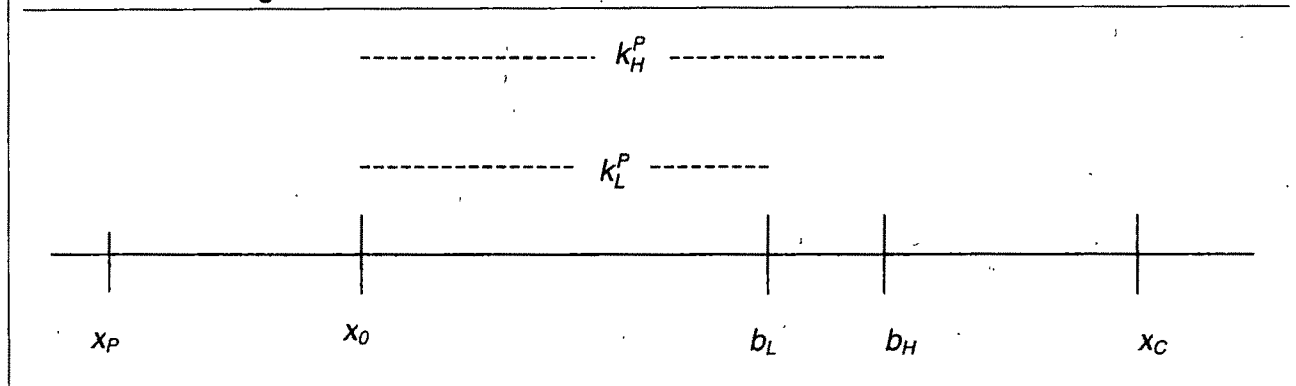
if the partner has low termination costs, b_H otherwise. In this case, the prime minister cannot always accept the proposal by the partner whose type has low termination costs. If she did, the high-cost partner type would have an incentive to mimic the low-cost type (i.e., if the prime minister accepted b_L in Figure 2, then the high-cost type of partner would never want to adopt b_H). Thus, confidence procedures are invoked with positive probability in order to give the high-cost type of partner an incentive to retain the informative, separating strategy.

Although the confidence procedure is used in equilibrium, it never results in an inefficient termination because all incomplete information about the partner's termination costs is resolved in equilibrium. The only inefficiency is due to the audience cost, e, that is paid for using the confidence procedure. This inefficiency arises because, as noted above, the prime minister must use the procedure to provide the partner with the proper incentives to reveal information.

Finally, we consider cases in which political uncertainty leads to inefficient government terminations with positive probability. One pattern leading to this outcome occurs when both types of partner pool on a proposal acceptable to the high-cost prime minister but provokes the low-cost prime minister to propose a risky confidence motion. No information about costs is revealed by the coalition partner's initial proposal, and a risky confidence proposal fails if the coalition partner has a low termination cost. Thus, whenever both the prime minister and the partner have low termination costs, the government always fails in this equilibrium. For such a pooling equilibrium to exist, several conditions must be met. First, the partner's prior belief that the prime minister has high termination costs must be reasonably high, so that the partner's risk of provoking a confidence vote is not undue.[9] Second, the prime minister's prior belief that the partner has low costs must not be large enough to entice the prime minister (with low termination costs) to make the safe proposal.[10] At the same time, this belief cannot be too small, or the prime minister whose type has high costs will

[9] Alternatively, C's termination costs must be low.
[10] Alternatively, P's audience costs must be low, or $k_H^C \gg k_L^C$, so that the policy gains from the risky confidence motion are large.

have an incentive to use the confidence vote as well. We can see, then, that prior beliefs of both the prime minister and the partner are relevant to determining the conditions under which inefficient bargaining failures can occur. Substantively similar equilibria are generated by semipooling equilibria in which both types of P make a confidence motion with a positive probability. We will return to issues of cabinet instability after we analyze the collective approval model.

COLLECTIVE CABINET DECISION MAKING

Intuitions about the distributive consequences of collective cabinet decision rules are nicely conveyed by first describing what occurs with complete information. Consider the partner's decision to accept or reject a confidence motion by the prime minister in the cabinet. Acceptance yields the policy utility associated with the motion. The consequences of rejection depend on the initial bill adopted by the partner and on the prime minister's termination costs. The prime minister will resign following rejection in the cabinet only if the partner's original bill yields a worse outcome than government termination (status quo policy and the costs of termination). Thus, if the original bill is preferred to termination by the prime minister, the partner will accept only a confidence motion that provides additional policy concessions beyond his original proposal. Alternatively, if this initial proposal is not one that the prime minister prefers to termination, the partner will accept any z that he prefers to termination.

In Figure 3, for example, if it is common knowledge that the prime minister has low termination costs, then the utility to her of resigning is a function of the distance to the status quo, x_0, and the termination costs, k_L^P. If the prime minister's confidence motion is rejected and the partner's initial proposal is at least as close to x_P as b_L, then the prime minister will not resign following a vote against this motion in the cabinet by the partner. In this case, the partner will only accept confidence motions that make him better off than his initial proposal (i.e., will only accept confidence motions that the prime minister never has an incentive to propose). If the initial proposal by the

partner is not as close as b_L, then the prime minister will resign if the partner rejects a confidence motion in the cabinet. In this case, the partner will accept any confidence motion that he prefers to government termination.

Therefore, the prime minister can only use a confidence motion to make herself better off if the partner's original bill gives her a lower utility than termination. The partner can ensure that the prime minister never has an opportunity to change policy using a confidence motion by proposing the policy he most prefers from the set of policies that the prime minister prefers to accept over resigning. This policy corresponds to b_L in Figure 3. In a subgame perfect equilibrium, the partner will propose this policy, and the prime minister will accept it.

The collective cabinet decision rule, then, effectively reverses the nature of proposal and veto opportunities in cabinet decision making, with rather dramatic consequences for policy outcomes. Under the unilateral procedure, the unencumbered last move advantage permits the prime minister to extract policy concessions from her coalition partner, even though she need not invoke the procedure to do so. Under the collective cabinet decision rule, coalition partners, aware that they can veto confidence motions in the cabinet, can extract policy concessions from prime ministers who pay a cost of losing their job.

Now consider the effect of political uncertainty. Like the prime minister in the unilateral model, who must choose between a risky and safe confidence strategy, the coalition partner faces a strategic dilemma under collective decision rules. We can see the logic in Figure 3. When the prime minister has low termination costs, we have described why the best policy the partner can adopt is b_L. By a similar logic, the best policy he can propose if the prime minister has high termination costs is b_H. Thus, with political uncertainty, the risky proposal in this game is obviously b_H: If the partner adopts b_H and rejects confidence motions in the cabinet, then the government will fall when the prime minister has low termination costs.

As in the unilateral model, the effect of this uncertainty depends on a number of parameters in the collective model. In the Appendix, we describe the perfect Bayesian equilibria to this game, which fall into three straightforward cases. We summarize the main substantive results as follows.[11]

Distributive consequences: The partner never makes policy concessions to the prime minister, but the prime minister often makes policy concessions to the partner. Consequently, the partner's policy utility from the final (equilibrium) outcome is never lower (and is often higher) than the partner's utility from the status quo.

Mode of government termination: A confidence motion by the prime minister is never defeated on the

floor. Instead, inefficient bargaining failures occur only by resignation.

Factors leading to termination: Resignation occurs in equilibrium only when the partner has a sufficiently high prior belief that the prime minister has high termination costs (i.e., q_0 is sufficiently large). The critical values of q_0 are increasing functions of k_L^C, k_H^C, and k_L^P and are decreasing functions of k_H^P. The prime minister's beliefs about the partner's termination costs never influence equilibrium outcomes.

Again, the distributive implications can be well understood from the intuition of the complete information case, so we turn to a discussion of incomplete information. When the partner's belief that the prime minister has high termination costs is sufficiently low (i.e., q_0 is sufficiently low), there are several different equilibrium paths. Some entail use of confidence motions that are approved in the cabinet, some do not, and none results in a government termination. The final policy outcome in each is equivalent to the partner's "safe" proposal (b_L in Figure 3). These equilibria are sustained by the fact that q_0 is sufficiently low, that is, the probability that the prime minister has low termination costs is so high that the partner either must adopt the most accommodating policy or trigger a confidence motion on this policy (which he must accept).[12]

When the partner's prior belief that the prime minister has high termination costs is sufficiently high, the partner is willing to take more risks by proposing a less accommodating bill, rejecting confidence motions in the cabinet, yet knowing that such rejections will lead to government resignation only in the (relatively unlikely event) that the prime minister actually has low termination costs. There are two cases to consider. The first is when q_0 has an intermediate value. Here, its value is sufficiently high that the partner with high termination costs will not adopt the risky proposal, which leads to resignation if the prime minister has low termination costs. At the same time, q_0 has a sufficiently low value that if the partner has low termination costs, he will make a risky proposal that leads to a government termination if the prime minister has low termination costs. In this equilibrium, information about the prime minister's type is revealed in equilibrium (because only the low-cost type makes a confidence motion when a risky proposal is adopted by the partner). Yet, the cost of inducing the separation is that the partner must provoke the low-cost prime minister to resign when she is faced with a risky initial proposal. This is true because if the partner accepts a confidence motion in the cabinet by the low-cost prime minister, then the high-cost prime minister will have no incentive to maintain her separating strategy, which destroys the equilibrium.

In the final case, the partner's prior belief that the prime minister has high termination costs is relatively low. In this equilibrium, the partner (regardless of his type) has an incentive to adopt the risky initial pro-

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[11] See proposition 6 in the Appendix for formal statements and proofs.

[12] Alternatively, this outcome occurs if the termination costs are high enough to deter the partner from provoking a resignation.

TABLE 1. Policy Differences between Prime Minister and Government		
Cabinet Decision Rule	Mean Difference between Prime Minister and Government	
	All Governments	Coalition Governments
Unilateral	.47 (.05) N = 236	.86 (.09) N = 106
Collective	.71 (.07) N = 126	1.04 (.08) N = 82
p-value	.003	.08

Note: The cells give the mean of the absolute difference between the Left-Right location of the prime minister and the weighted Left-Right location of the government (where parties are weighted by the number of seats they hold). The N is for the number of governments. The p-value is for the null hypothesis that the unilateral mean < collective mean. Standard errors are in parentheses. The government location and prime minister location variables are measured using the Left-Right positions of political parties from expert surveys. We use the most (temporally) proximate measure available from among Castles and Mair 1984; Huber and Inglehart 1995; and Morgan 1976. The government location of coalitions is weighted by the percentage of seats controlled by each party. The governments are from Woldendorp, Keman, and Budge 1993. The countries include Australia, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, the Netherlands, New Zealand, Norway, Sweden, and the United Kingdom. We omit all caretaker governments.

posal. The prime minister (regardless of her type) responds by making a confidence motion that is always rejected by the partner in the cabinet, but resignation occurs only if she has low termination costs.

A COMPARISON OF CABINET DECISION RULES

A comparison of the two models suggests that strategic behavior and bargaining outcomes should be quite different under confidence procedures that allow unilateral action by the prime minister compared to those that require collective cabinet approval. In this section, we discuss these differences and some empirically testable hypotheses they suggest. We also present two very simple tests.

Our analysis indicates that the nature of cabinet decision rules often has a significant effect on policy outcomes, in particular on which member of a governing coalition has the upper hand in determining final policies. When unilateral action on confidence motions is allowed, prime ministers have the opportunity to make the final proposal, and they can use this power to extract policy concessions from their partners in the governing majority. When collective cabinet approval of confidence motions is required, final proposal power belongs to the pivotal member of the governing coalition, who can use it to extract policy concessions from the prime minister.

Although testing this claim about the policy biases of different procedures is beyond the scope of this article, we can provide a simple test of an indirect implication concerning the selection of prime ministers. The test is indirect because our model obviously does not include a government formation stage. Yet, our analysis suggests that if political parties anticipate the effect of the cabinet decision-making rules during government formation, then these rules could well influence the type of prime minister selected. In particular, if prime ministers have significantly more power to influence policy outcomes under unilateral cabinet decision rules, then the costs to the governing coalition of selecting a prime minister with extreme preferences will be significantly greater in unilateral than collective systems. We might expect the preferences of prime ministers and the governing majority to be more closely aligned in systems with unilateral cabinet rules.

Table 1 shows that this is indeed the case. The table gives the average absolute distance between the Left-Right position of prime ministers and the weighted Left-Right position of the government in 14 parliamentary democracies. We analyze these averages when all governments are included (column 1) and when only coalitions are included (column 2). The argument against including all governments is that single-party majorities (1) always have a difference (between the prime minister and government) of zero and (2) form most often in countries with unilateral rules. Thus, including these governments may create a bias. The argument for including all governments is that if unilateral rules exist, then so should the incentives to form single-party majorities. Thus, excluding these countries should also lead to bias.[13] Both arguments have some merit, but the inferences we draw do not hinge significantly on our position on this issue. In both columns of Table 1, prime ministers are closer to their government in the unilateral systems than in the collective systems. The differences exist under both assumptions, although, as expected, the p-values are somewhat larger when we exclude single-party majorities. There is evidence, then, that the nature of cabinet decision rules influences the choices that parties make about prime ministers during government formation.

A second implication of our analysis concerns the effect of political uncertainty on bargaining among coalition partners. One consequence of uncertainty is at times to mitigate the ability of the dominant bargaining player (the prime minister under unilateral rules, the partner under collective rules) to extract policy concessions from the other side. One reason this occurs is because uncertainty encourages "safe" bargaining strategies, so that the dominant player does not fully extract the available policy concessions in order to avoid government failures. In general, under collective rules, as the coalition partner's belief that the prime minister is the strong type increases (i.e., q_0 decreases), so does the ability of the partner to extract concessions from the prime minister. Under unilateral rules, as each player's belief that the other is strong increases (i.e., q_0 and p_0 decrease), so does the ability of the prime minister to extract concessions.

Another reason uncertainty mitigates the dominant bargaining player's power is less desirable—uncer-

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[13] Indeed, this is borne out by a simple probit model that shows the formation of a single-party majority government is much more likely in countries with unilateral rules.

tainty under some conditions leads to bargaining failure (and thus to the extraction of no concessions). Comparing the likelihood of bargaining failure under the two procedures is not always straightforward, however. Under collective decision rules, termination is always the result of a resignation by the low-cost prime minister following a gamble by the coalition partner that she has high costs. The prime minister's uncertainty about the partner never affects the final outcome, or the possibility of inefficient terminations. This is in sharp contrast to the unilateral confidence procedure: Uncertainty by the partner about the prime minister's termination costs can lead him to propose policies that provoke confidence votes, and uncertainty by the prime minister about the partner's termination costs guarantees that some confidence votes will fail.

Despite the fact that two-sided uncertainty matters in the unilateral case, it is not true in general that the unilateral confidence procedure produces more government instability. The outcome depends on the prior beliefs of the prime minister and the coalition partner. Generally speaking, when the partner's belief that the prime minister is weak is high relative to the prime minister's belief that the partner is weak (i.e., p_0 is high relative to q_0), the unilateral confidence motion produces more instability. Under these conditions, the prime minister is more willing to risk termination by extracting concessions under the unilateral rule than is the partner under the collective rule. When the opposite is true (p_0 is low relative to q_0), the collective rule produces more inefficient terminations. When these prior beliefs are approximately equal, either the systems are identical or the comparison depends in a complicated way on other parameters.

Our analysis does suggest, however, an additional implication of political uncertainty that is testable. In particular, if political uncertainty is operating as our model suggests, then the mode of government termination should depend on the nature of cabinet decision rules. In the unilateral case, inefficient terminations should occur on the floor following the defeat of a specific policy; in the collective case, governments should resign before taking the fight to the floor. As noted above, however, this difference should only exist when the pivotal members of the governing coalition are all in the government. During minority governments, this will not be the case. If the prime minister under collective rules can achieve cabinet approval for a particular policy but then loses on the floor for lack of necessary support from a party outside the government, then the collective case operates identically to the unilateral one.

Therefore, we offer a conditional hypothesis about the effect of cabinet decision rules and political uncertainty on the mode of termination. If uncertainty has the effect our model suggests, then compared to minority governments or systems with unilateral cabinet rules, a government with majority status and collective rules should be more likely to terminate by resigning than by failing on the floor. We test this with a simple probit regression. An observation is a government termination caused by policy conflict among members

of the majority, and the dependent variable *Resign* takes the value 1 if the government resigns without taking the fight to the floor, 0 if the government takes the fight to the floor and loses.[14] We include one independent variable, a dummy with the value 1 if there are collective cabinet decision rules and majority government, 0 otherwise. The results of the logit model are as predicted by the model (standard errors in parentheses):

Resign = .15 (.13) + .58 (.26) Collective Rules

$\times$ Majority Government.

We find, then, rather direct (albeit preliminary) evidence that the strategic dynamics created by private information in our model indeed affect the nature of bargaining processes differently in parliamentary systems with collective rules as compared to those with unilateral rules.

CONCLUSION

Abstract game theoretic models in political science frequently suggest that the outcomes of bargaining processes in democratic systems should be significantly influenced by the precise character of proposal and veto opportunities, on the one hand, and by informational asymmetries that exist among participants, on the other. In parliamentary systems, although the cabinet is widely recognized as the central arena for strategic bargaining, scholars have examined neither variation in cabinet decision rules nor the effect of private information on bargaining processes in the cabinet. Our analysis attempts to fill this gap by arguing for the central importance of distinguishing between collective cabinet decision institutions and institutions that permit unilateral action by the prime minister on confidence motions.

Our models suggest that this institutional distinction profoundly affects the distribution of bargaining power within the government. Under collective rules, pivotal parties within the prime minister's majority have the advantage of being able to make the final policy proposal; under unilateral rules, the prime minister has this advantage. The distribution of proposal power, we argue, affects not only the nature of policy outcomes but also the types of political considerations that parties face when choosing a prime minister. These institutions also influence the ability of parties to exploit political uncertainty. They affect, for example, the capacity of the privileged actor within a coalition to extract policy concessions, the propensity for inefficient bargaining failures to occur, and the circumstances surrounding such failures.

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[14] We used Keesings Contemporary Archive to code government terminations (from the Woldendorp data) according to whether (1) they occurred because of policy conflict, and (2) they ended with resignations before floor debate as opposed to defeats on the floor. We excluded governments that ended for reasons other than policy conflict. For the remaining cases, our dependent variable takes the value 1 if resignation occurred, 0 otherwise. There are 130 observations, 81 of which take the value 1 (for resignation).

The institutional distinction central to this analysis, then, can play a valuable role in the broader comparative research agenda that seeks to understand important institutional differences within parliamentary democracies (e.g., Lijphart 1999; Powell 2000). It also serves to underscore shortcomings in previous comparative research that focuses on broad institutional categories, such as parliamentarism, presidentialism, federalism, and bicameralism. As our model and evidence illustrate, there are significant limitations to such an aggregated approach. Even somewhat subtle distinctions in cabinet decision-making rules can lead to substantial differences across parliamentary systems in both performance and policy outputs. Without understanding these differences within parliamentary democracy, we cannot hope to understand its fundamental differences with alternative systems.

APPENDIX

We begin by formally defining the strategies and beliefs of the two players.[15]

- $b_i(p_0) \in R$ is the coalition partner's initial policy proposal.
- $a_t(b, p_1)$ is the prime minister's response to the partner's bill, b. P can either accept b ($a_t = accept$) or propose some new policy, $z_t(b, p_1) \in R$.
- $v_t(z, q_1) \in \{0, 1\}$ is C's voting strategy on the floor following P's use of the confidence vote motion (CVM) under unilateral cabinet decision rules, where $v_t = 1$ denotes acceptance of the CVM, and $v_t = 0$ denotes rejection.
- $y_t(b, z, q_1)$ is C's voting strategy in the cabinet following P's proposed CVM under collective cabinet decision rules, where $y_t = 1$ denotes acceptance, and $y_t = 0$ denotes rejection.
- $r_t(b) \in \{0, 1\}$ is P's resignation strategy following the rejection of a CVM in the cabinet under collective rules, where $r_t = 1$ denotes resignation, and $r_t = 0$ denotes acceptance of C's initial policy, b_t.
- p_1 is P's updated beliefs that C has high termination costs.
- q_1 is C's updated beliefs that P has high termination costs.

Next, we define the sets of policy that C and P would prefer to adopt in lieu of terminating the government. For type $t \in \{H, L\}$ and player $i \in \{P, C\}$, this set is given by $A_t^i = \{x | u_i(x) \geq u_i(x_0) - k_t^i\}$. In order to focus on cases in which private information has strategic relevance, we make two assumptions about A_L^C and A_L^P.

ASSUMPTION 1. $x_P \notin A_L^C$.

ASSUMPTION 2. $x_C \notin A_L^P$.

If assumption 1 is violated, then P does not care about C's termination costs in the unilateral game because she can always use a CVM to obtain her most preferred policy. Similarly, if assumption 2 is violated, in the collective case, C can adopt his ideal point and reject any confidence motion by P in the cabinet, yielding an outcome equal to C's ideal point.

We now define specific policies that play an important role in the analysis below:

$$x_t^P = \arg\max_{x \in A_t^P} u_C(x) \text{ and } x_t^C = \arg\max_{x \in A_t^C} u_P(x).$$

Thus, x_t^P (alternatively, x_t^C) represents the best policy for C (P) from the set of policies that P (C) prefers to government termination. Using these definitions and assumptions 1 and 2, it follows that $x_P \leq x_H^C < x_L^C$ and $x_L^P < x_H^P \leq x_C$.

The Unilateral Case

As noted in the text, in the unilateral model with complete information, P can always invoke a confidence motion on $z = x_t^C$. The problem P faces with incomplete information is that she does not know k_t^C, C's termination costs. If P invokes a confidence procedure on x_H^C and C has low costs, then the government falls. Thus, P of type t's expected utility from $z = x_H^C$ is $p_1 u_P(x_H^C) + (1 - p_1)[u_P(x_0) - k_t^P] - e$, and her utility from $z = x_L^C$ is $u_P(x_L^C) - e$. The CVM that P most prefers depends on updated beliefs, p_1, so that these updated beliefs also influence the optimal b that C proposes. If P knows C's type, then the best proposal C can make that deters P from use of a CVM is $\tilde{x}_t^C = \arg\max_{\{x | u_P(x) \geq u_P(x_t^C) - e\}} u_C(x)$, where $\tilde{x}_H^C \leq \tilde{x}_L^C$, given that $k_H^C > k_L^C$. If P is uncertain about C's type, then C's best proposal depends on p_1. Define $m_t = \arg\max u_C(x)$ such that $u_P(x) \geq \max \{u_P(x_L^C), p_1 u_P(x_H^C) + (1 - p_1)[u_P(x_0) - k_t^P]\} - e$.

Thus, m_t is the policy that if accepted by P of type t gives P the same utility that she would receive from adopting her optimal CVM. If C adopts any $b > m_t$, then P of type t will reject this policy and use a confidence motion to propose a worse one for C. Thus, m_t is C's best proposal that forestalls a CVM by P of type t.

Before proving several propositions, it is useful to make some observations about m_t and $\tilde{x}_t^C$. First, note that $\tilde{x}_H^C \leq m_L \leq m_H \leq \tilde{x}_L^C$. Second, if

$$p_1 u_P(x_H^C) + (1 - p_1)[u_P(x_0) - k_t^P] - e < u_P(x_L^C) - e$$

$$\text{or } p_1 \leq \frac{u_P(x_L^C) - u_P(x_0) + k_t^P}{u_P(x_H^C) - u_P(x_0) + k_t^P}, \quad \text{then } m_t = \tilde{x}_L^C.$$

Finally, $x_L^C > x_P$ implies that $x_P \notin A_L^C$ and thus that $u_C(x_L^C) = u_C(x_0) - k_L^C$.

We now establish lemma 1, which suggests a lower bound on P's equilibrium utility and the fact that P will not resign in equilibrium.

LEMMA 1. If $k_L^P > e$ and $k_L^C > 0$, P resigns with probability zero under the unilateral confidence vote procedure. Furthermore, P's expected utility must exceed $u_P(x_L^C) - e$ in equilibrium.

Proof: Either type of P can always assure herself of at least $u_P(x_L^C) - e$ through the use of a CVM; since both C types will accept x_L^C. Thus, P will resign only if $u_P(x_0) - k_L^P > u_P(x_L^C) - e$, which can never be satisfied because $u_P(x_L^C) \geq u_P(x_0)$, since $k_L^C > 0$ and $k_L > e$. Q.E.D.

Lemma 2 is a technical result that proves useful in several of the propositions below.

LEMMA 2. $u_C(m_t) \geq u_C(x_L^C)$ whenever

$$\frac{u_P(x_L^C) - u_P(x_0) + k_t^P + e}{u_P(x_H^C) - u_P(x_0) + k_t^P} \geq p_0.$$

Proof: Note that $u_C(x_L^C) = \max u_C(x)$ such that $u_P(x) \geq u_P(x_L^C)$, and $u_C(m_t) = \max u_C(x)$ such that $u_P(x) \geq \max \{u_P(x_L^C), p_1 u_P(x_H^C) + (1 - p_1)[u_P(x_0) - k_t^P]\} - e$. By principles of constrained maximization, $u_C(m_t) \geq u_C(x_L^C)$ if and only if

$$u_P(x_L^C) \geq p_1 u_P(x_H^C) + (1 - p_1)[u_P(x_0) - k_t^P] - e \text{ or}$$

$$\frac{u_P(x_L^C) - u_P(x_0) + k_t^P + e}{u_P(x_H^C) - u_P(x_0) + k_t^P} \geq p_0 . \qquad Q.E.D.$$

The equilibrium strategies and beliefs in the unilateral confidence motion game depend on a set of conditions that involve initial beliefs, termination and audience costs, and the location of the status quo. Depending on these parameter values, pooling, separating, and semipooling equilibria can exist.

PROPOSITION 1. *Let* $m \in (max \{x_L^C, \tilde{x}_H^C\}, m_L]$. *If*

$$\frac{u_P(x_L^C) - u_P(x_0) + k_L^P + e}{u_P(x_H^C) - u_P(x_0) + k_L^P} \geq p_0 ,$$

then the following strategies and beliefs constitute a perfect Bayesian equilibrium to the unilateral CVM game:

$$b_t^* = m,$$

$$a_t^* = \begin{cases} accept \text{ if } b = m \text{ or } b \leq \tilde{x}_H^C \\ propose \ z = x_H^C \text{ otherwise} \end{cases},$$

$$v_t^* = \begin{cases} 1 & if \ z \geq x_t^C \\ 0 & otherwise \end{cases}, \text{ and}$$

$$p_1^* = \begin{cases} p_0 & if \ b = m \\ 1 & otherwise \end{cases}.$$

Proof: C's floor voting strategies are straightforward, as he accepts a CVM if and only if its policy utility exceeds that of the status quo plus the termination costs. Thus, we begin by ensuring that C will not defect from proposing $m \in (max \{x_L^C, \tilde{x}_H^C\}, m_L]$. By definition, $u_C(m_L) \geq u_C(\tilde{x}_H^C)$, and lemma 2 implies $u_C(m_L) \geq u_C(x_L^C)$ whenever

$$\frac{u_P(x_L^C) - u_P(x_0) + k_L^P + e}{u_P(x_H^C) - u_P(x_0) + k_L^P} \geq p_0 .$$

Thus, the interval $(max \{x_L^C, \tilde{x}_H^C\}, m_L]$ is nonempty. Both C types receive $u_C(m)$ in equilibrium because this proposal is accepted independent of P's type. Because C prefers m to any other policy that is always accepted, it only remains to consider defections that induce a CVM. Given assumption 1, P's equilibrium confidence motion, and C's voting strategies, defections that trigger a CVM yield $u_C(x_t^C)$ for each type. Such a defection must lower C's utility, since $u_C(m) \geq u_C(x_L^C) > u_C(x_H^C)$.

We now turn to P's best response. Recall that m_t is the policy which, if accepted by P of type t, gives this type of prime minister the same utility she would obtain from adopting her optimal CVM when $p_1^* = p_0$. Given $k_L^P < k_H^P$, we know that $m_L < m_H$. Thus, since $u_P(m) \geq u_P(m_L) > u_P(m_H)$, both types of P prefer accepting C's bill to proposing either x_L^C or x_H^C under the CVM. Following out-of-equilibrium proposals by C, the beliefs $p_1^* = 1$ make a CVM of x_H^C a best response.

Beliefs on the equilibrium path $p_1^* = p_0$ are determined by Bayes's rule. $\qquad Q.E.D.$
Comment: To establish the comparative statics on the critical value

$$\frac{u_P(x_L^C) - u_P(x_0) + k_L^P + e}{u_P(x_H^C) - u_P(x_0) + k_L^P} ,$$

note that $u_P(x_t^C)$ is increasing in k_t^C. Therefore, the critical value is increasing in k_L^C, k_L^P, and e and is decreasing in k_H^C.

PROPOSITION 2. *Let* $m \in (max \{m_L, x_L^C\}, m_H]$. *If*

$$q_0 \geq \frac{u_C(\tilde{x}_H^C) - u_C(x_H^C)}{u_C(m) - u_C(x_H^C)} \text{ and}$$

$$\frac{u_P(x_L^C) - u_P(x_0) + k_H^P + e}{u_P(x_H^C) - u_P(x_0) + k_H^P} \geq p_0 \geq \frac{u_P(x_L^C) - u_P(x_0) + k_L^P}{u_P(x_H^C) - u_P(x_0) + k_L^P} ,$$

then the following strategies and beliefs constitute a perfect Bayesian equilibrium to the unilateral CVM game:

$$b_t^* = m,$$

$$a_H^* = \begin{cases} accept \text{ if } b = m \text{ or } b \leq \tilde{x}_H^C \\ propose \ z = x_H^C \text{ otherwise} \end{cases},$$

$$a_L^* = \begin{cases} accept \text{ if } b \leq \tilde{x}_H^C \\ propose \ z = x_H^C \text{ otherwise} \end{cases},$$

$$v_t^* = \begin{cases} 1 & if \ z \geq x_t^C \\ 0 & otherwise \end{cases}, \text{ and}$$

$$p_1^* = \begin{cases} p_0 & if \ b = m \\ 1 & otherwise \end{cases}.$$

Proof: Again, C's voting strategies are straightforward. Consider C's choices in the proposal-making stage. Lemma 2 implies $u_C(m_H) \geq u_C(x_L^C)$ if

$$\frac{u_P(x_L^C) - u_P(x_0) + k_H^P + e}{u_P(x_H^C) - u_P(x_0) + k_H^P} \geq p_0 .$$

Thus, the interval $(max \{m_L, x_L^C\}, m_H]$ is nonempty.
Given that a low-cost P always uses a CVM to propose $z = x_H^C$ in equilibrium, the equilibrium utilities of C of types L and H are $q_0 u_C(m) + (1 - q_0)u_C(x_L^C)$ (by assumption 1) and $q_0 u_C(m) + (1 - q_0)u_C(x_H^C)$, respectively. There are two defections that we must rule out: C must neither wish to induce a CVM by the high type of P nor propose $b \leq \tilde{x}_H^C$, which is always accepted. Consider an induced CVM such as $b \neq m > \tilde{x}_H^C$. Given P's best response of proposing $z = x_H^C$ and the voting strategies of C, this generates a payoff of $u_C(x_t^C)$ for each type. Neither type can benefit from invoking a CVM as long as $q_0 u_C(m) + (1 - q_0)u_C(x_t^C) \geq u_C(x_t^C)$ or $u_C(m) \geq u_C(x_t^C)$, which is true since $m \in (max \{m_L, x_L^C\}, m_H]$.
Next, consider whether C will defect by proposing $b \leq \tilde{x}_H^C$. For both types, this defection generates $u_C(b) \leq u_C(\tilde{x}_H^C)$ because P always accepts this bill. Yet, none of the proposals are profitable for type t so long as

$$q_0 u_C(m) + (1 - q_0)u_C(x_t^C) \geq u_C(\tilde{x}_H^C) \text{ or}$$

$$q_0 \geq \frac{u_C(\tilde{x}_H^C) - u_C(x_t^C)}{u_C(m) - u_C(x_t^C)} ,$$

which is guaranteed by the assumption that

$$q_0 \geq \frac{u_C(\tilde{x}_H^C) - u_C(x_H^C)}{u_C(m) - u_C(x_H^C)}$$

(which is possible only if $m > \tilde{x}_H^C$).
Now consider P's strategies. Given $p_1^* = 1$ following an out-of-equilibrium proposal by C, x_H^C must be a best response, and we can concentrate on her response to m. Because $m \leq m_H$, P with high costs prefers m to making either possible $z = x_t^C$. Because $m_L < m$, P with low costs prefers a confidence motion to m, and she prefers proposing x_H^C over x_L^C if

$$p_0 u_C(x_H^C) + (1 - p_0)[u_C(x_0) - k_L^P] \geq u_C(x_L^C) \,,$$

which implies that

$$p_0 \geq \frac{u_P(x_L^C) - u_P(x_0) + k_L^P}{u_P(x_H^C) - u_P(x_0) + k_L^P} \,,$$

which is assumed by the proposition.

Beliefs on the equilibrium path $p_1^* = p_0$ are determined by Bayes's rule. $\qquad$ Q.E.D.

Comment: Similar to the above, the upper critical value of p_0 increases in k_L^C, k_H^P, and e, and it decreases in k_H^C. The lower critical value increases in k_L^C and k_L^P and decreases in k_H^C. The properties of the critical value of q_0 can be established from noting that $u_C(x_t^C)$ is increasing in k_t^P, and $u_C(\bar{x}_t^C)$ is increasing in e and decreasing in k_t^C. Thus, for a given m, the critical value increases in e and decreases in k_H^P and k_H^C.

PROPOSITION 3. *If $u_C(\bar{x}_H^C) > u_C(x_L^C)$, and $u_P(\bar{x}_L^C) = u_P(x_L^C) - e$, then the following strategies and beliefs constitute a perfect Bayesian equilibrium to the unilateral CVM game:*

$$b_t^* = \bar{x}_t^C,$$

$$a_t^* = \begin{cases} accept \ if \ b \leq \bar{x}_H^C \\ accept \ with \ prob \ \eta \ if \ b = \bar{x}_L^C \\ propose \ z = x_L^C \ with \ prob \ 1 - \eta \ if \ b = \bar{x}_L^C \\ propose \ z = x_H^C \ otherwise \end{cases}$$

$$v_t^* = \begin{cases} 1 & if \ z \geq x_t^C \\ 0 & otherwise \end{cases}, \ and$$

$$p_1^* = \begin{cases} 0 & if \ b = \bar{x}_L^C \\ 1 & otherwise \end{cases},$$

$$where \ \eta = \frac{u_C(\bar{x}_H^C) - u_C(x_L^C)}{u_C(\bar{x}_L^C) - u_C(x_L^C)}.$$

Proof: P learns C's type from C's proposal, and $u_P(\bar{x}_L^C) = u_P(x_L^C) - e$ implies $u_P(\bar{x}_H^C) = u_P(x_H^C) - e$. So P is indifferent between accepting $\bar{x}_L^C$ and using a confidence procedure to adopt x_L^C. Thus, a_t^* is a best response on the equilibrium path. For any bill off the equilibrium path, $p_1^* = 1$ implies that adopting $z_t = x_H^C$ for any such $b > \bar{x}_H^C$ is a best response, as is accepting any $b \leq \bar{x}_H^C$.

For C, any proposal $b \neq \bar{x}_t^C$ will result in $u_C(x_t^C)$, which cannot be a best response because $u_C(\bar{x}_H^C) \geq u_C(x_H^C)$, and η is defined so that $\eta u_C(\bar{x}_L^C) + (1 - \eta)u_C(x_L^C) \geq u_C(x_L^C)$. So that H will not mimic L it must be true that $u_C(\bar{x}_H^C) \geq \eta u_C(\bar{x}_L^C) + (1 - \eta)u_C(x_L^C)$, and for L to not mimic H it must be true that $u_C(\bar{x}_H^C) \leq \eta u_C(\bar{x}_L^C) + (1 - \eta)u_C(x_L^C)$. The only value of η that solves both of these inequalities is

$$\eta = \frac{u_C(\bar{x}_H^C) - u_C(x_L^C)}{u_C(\bar{x}_L^C) - u_C(x_L^C)}.$$

So that η is a feasible probability, we require that $u_C(\bar{x}_H^C) > u_C(x_L^C)$. The necessary condition $u_P(\bar{x}_L^C) = u_P(x_L^C) - e$ rules out any other separating equilibria. $\qquad$ Q.E.D.

Comment: This equilibrium, in which no terminations occur, exists only if e is sufficiently large (so that $u_C(\bar{x}_H^C) > u_C(x_L^C)$). Whenever this condition is satisfied, so are the conditions necessary for the existence of the equilibrium described in proposition 1.

PROPOSITION 4. *If*

$$p_0 \geq \frac{u_P(x_L^C) - u_P(x_0) + k_H^P + e}{u_P(x_H^C) - u_P(x_0) + k_H^P} \ and$$

$$q_0 \geq \frac{u_C(\bar{x}_H^C) - u_C(x_H^C)}{u_C(x_L^C) - u_C(\bar{x}_H^C)} \,,$$

then the following strategies and beliefs constitute a perfect Bayesian equilibrium to the unilateral CVM game:

$$b_L^* = x_L^C \,,$$

$$b_H^* = \begin{cases} x_L^C \ with \ prob \ \lambda \\ \bar{x}_H^C \ otherwise \end{cases},$$

$$a_L^* = \begin{cases} accept \ b \leq \bar{x}_H^C \\ propose \ z = x_H^C \ otherwise \end{cases},$$

$$a_H^* = \begin{cases} accept \ b \leq \bar{x}_H^C \\ accept \ b = x_L^C \ with \ prob \ \omega, \\ propose \ z = x_H^C \ otherwise \end{cases}$$

$$v_t^* = \begin{cases} 1 & if \ z \geq x_t^C \\ 0 & otherwise \end{cases}, \ and$$

$$p_1^* = \begin{cases} \dfrac{p_0 \lambda}{p_0 \lambda + 1 - p_0} & if \ b = x_L^C, \\ 1 & otherwise \end{cases}$$

$$where \ \lambda = \frac{(1 - p_0)}{p_0} \cdot \frac{u_P(x_L^C) - u_P(x_0) + k_H^P + e}{u_P(x_H^C) - u_P(x_L^C) - e}$$

$$and \quad \omega = \frac{1}{q_0} \cdot \frac{u_C(\bar{x}_H^C) - u_C(x_H^C)}{u_C(x_L^C) - u_C(\bar{x}_H^C)}.$$

Proof: We first consider P's optimal response to b. If $b = \bar{x}_H^C$, then P is sure that C is a high-cost type. Both types of P are willing to accept $\bar{x}_H^C$, since doing so avoids audience costs and generates as high a utility as the best acceptable CVM, given the beliefs $p_1^* = 1$, $z = x_H^C$.

If $b = x_L^C$, then Bayes's rule implies

$$p_1 = \frac{p_0 \lambda}{p_0 \lambda + 1 - p_0}.$$

Obviously, P prefers accepting $u_P(x_L^C)$ to adopting $z = x_L^C$. Because the only CVMs P might adopt in equilibrium are x_L^C or x_H^C (because any other confidence motion involves policy concessions that do not lower the probability of rejection), P must choose between accepting $b = x_L^C$ and proposing $z = x_H^C$. Proposing $z = x_H^C$ has an expected payoff of

$$\frac{p_0 \lambda[u_P(x_H^C) - e] + (1 - p_0)[u_P(x_0) - k_t^P - e]}{p_0 \lambda + 1 - p_0}.$$

P prefers this CVM if

$$\lambda \geq \frac{1 - p_0}{p_0} \cdot \frac{u_P(x_L^C) - u_P(x_0) + k_t^P + e}{u_P(x_H^C) - u_P(x_L^C) - e}.$$

Given the definition of λ, P with low costs strictly prefers to make the CVM, whereas the high-cost type is exactly indifferent between accepting $b = x_L^C$ and adopting $z = x_H^C$. It is therefore a best response to accept $b = x_L^C$ with probability ω.

We now turn to C's strategies. For low-cost C, the utility of $b = x_L^C$ is $u_C(x_L^C)$. Although $b = x_L^C$ is sometimes rejected, rejection leads to a CVM that C of type L rejects. Because $x_L^C > \bar{x}_H^C$, low-cost C prefers $b = x_L^C$ to any other bill that is accepted by P. Any other b that triggers a CVM results in termination, so C of type L weakly prefers $b = x_L^C$.

For C of type H, proposing $\bar{x}_H^C$ generates $u_C(\bar{x}_H^C)$ since P always accepts. This is obviously preferred to any acceptable $b < \bar{x}_H^C$. Proposing any other $b > \bar{x}_H^C$ other than x_L^C triggers a CVM, yielding $u_C(x_H^C) < u_C(\bar{x}_H^C)$. Finally, if $b = x_L^C$, this

policy is rejected by the low-cost P and by the high-cost P with probability $1 - \omega$. Therefore, the payoff to this bill is $\omega q_0 u_C(x_L^C) + (1 - \omega q_0) u_C(x_H^C)$ so that, if

$$\omega = \frac{1}{q_0} \cdot \frac{u_C(\tilde{x}_H^C) - u_C(x_H^C)}{u_C(x_L^C) - u_C(x_H^C)},$$

the high-cost C is indifferent between proposing $b = x_L^C$ and proposing $b = \tilde{x}_H^C$. The high-cost C can therefore play the mixed strategy specified in the proposition. The conditions

$$p_0 \geq \frac{u_P(x_L^C) - u_P(x_0) + k_H^P + e}{u_P(x_H^C) - u_P(x_0) + k_H^P} \quad \text{and}$$

$$q_0 \geq \frac{u_C(\tilde{x}_H^C) - u_C(x_H^C)}{u_C(x_L^C) - u_C(x_H^C)}$$

are required so that $\lambda \leq 1$ and $\omega \leq 1$.   Q.E.D.
Comment: The properties of the critical value of p_0 are established above. The critical value of q_0 is similar, but it also increases in k_L^C.

PROPOSITION 5. *If*

$$p_0 \geq \frac{u_P(x_L^C) - u_P(x_0) + k_L^P + e}{u_P(x_H^C) - u_P(x_0) + k_L^P} \quad and$$

$$q_0 \leq \frac{u_C(\tilde{x}_H^C) - u_C(x_H^C)}{u_C(x_L^C) - u_C(x_H^C)},$$

then the following strategies and beliefs constitute perfect Bayesian equilibrium to the unilateral CVM game:

$$b_L^* = x_L^C,$$

$$b_H^* = \begin{cases} x_L^C \text{ with prob } \lambda' \\ \tilde{x}_H^C \text{ otherwise} \end{cases},$$

$$a_L^* = \begin{cases} accept \ b \leq \tilde{x}_H^C \\ accept \ b = x_L^C \text{ with prob } \omega' , \\ propose \ z = x_H^C \text{ otherwise} \end{cases}$$

$$a_H^* = \begin{cases} accept \ b \leq \tilde{x}_H^C \text{ and } b = x_L^C \\ propose \ z = x_H^C \text{ otherwise} \end{cases},$$

$$v_t^* = \begin{cases} 1 & if \ z \geq x_t^C \\ 0 & otherwise \end{cases}, \ and$$

$$p_1^* = \begin{cases} \dfrac{p_0\lambda'}{p_0\lambda' + 1 - p_0} & if \ b = x_L^C \\ 1 & otherwise \end{cases},$$

where $\lambda' = \dfrac{(1 - p_0)}{p_0} \cdot \dfrac{u_P(x_L^C) - u_P(x_0) + k_L^P + e}{u_P(x_H^C) - u_P(x_L^C) - e}$

and $\omega' = \dfrac{u_C(\tilde{x}_H^C) - [q_0 u_C(x_L^C) + (1 - q_0)u_C(x_H^C)]}{u_C(x_L^C) - [q_0 u_C(x_L^C) + (1 - q_0)u_C(x_H^C)]}.$

Comment: The logic of the proof and the properties of critical values are the same as proposition 4.

The Collective Case

We begin by analyzing P's optimal resignation strategy following a CVM that fails in the cabinet. Because P's only options are to accept the partner's bill or resign, her optimal strategy must be

$$r_t^*(b) = \begin{cases} 1 \ \forall \ b \in A_t^P \\ 0 \ \forall \ b \notin A_t^P \end{cases}.$$

These strategies follow from the definition of A_t^P, which define the sets of policies that the low- and high-cost P's would (respectively) prefer to accept rather than resign.

Therefore, if P has costs k_t^P, $x_t^P = \arg\max_{x \in A_t^P} u_C(x)$ is the best bill C can enact without the resignation of P. Note that $x_L^P \leq x_H^P \leq x_C$. Recall that by assumption 2 we focus on cases in which $x_L^P < x_C$ so that $x_L^P \neq x_H^P$.

Before characterizing equilibrium behavior, we prove two general properties that must be found in any equilibrium for the collective cabinet case.

LEMMA 3: *In equilibrium, the expected utility to C (of either type) can be no less than $u_C(x_L^P)$.*

Proof: Because both types of P are willing to accept x_L^P, C can do no worse than propose it and block all confidence proposals.   Q.E.D.
Comment: This lemma demonstrates that the collective decision rule produces expected outcomes more favorable to C than does the unilateral case. In the unilateral case, P is guaranteed at least utility $u_P(x_L^C) - e$, so the best achievable expected utility for C is $u_C(\tilde{x}_L^C)$. It is easy to show that the worst expected policy in the collective case, $u_C(x_L^P)$, is preferred by C to the best policy in the unilateral case so long as $e < k_L^P$.

LEMMA 4. *Under collective cabinet decision making, resignation is the only equilibrium mode of government termination because confidence motions cannot be defeated on the floor.*

Proof: If C approves a confidence motion in the cabinet that it expects to vote against on the floor, its utility of $u_C(x_0) - k_t$ is strictly less than the utility guaranteed by lemma 3.

We characterize the equilibria under collective cabinet decision-making rules in proposition 6. There are three important cases to consider, defined by C's initial beliefs about the termination costs of P. To simplify the statement of the proposition, we define $\hat{y}$ as the realization of $y_t^*(b, z)$, C's decision to allow a confidence vote on z.   Q.E.D.

PROPOSITION 6.

Case 1: If

$$q_0 \leq \frac{u_C(x_L^P) - u_C(x_0) + k_L^C}{u_C(x_H^P) - u_C(x_0) + k_L^C},$$

then the following strategies and beliefs constitute a perfect Bayesian equilibrium:

$$b_t^* \in [x_L^P, x_H^P],$$

$$a_t^* = \begin{cases} accept \ if \ b \leq x_L^P \\ propose \ z = x_L^P \ if \ x_L^P < b \leq x_H^P, \\ propose \ z = x_H^C \ if \ x_H^P < b \end{cases}$$

$$y_t^* = \begin{cases} 1 \ if \ z = x_L^P \ or \ z \geq b \\ 1 \ if \ b > x_H^P \ and \ z \geq x_t^C, \\ 0 \ otherwise \end{cases}$$

$$r_t^* = \begin{cases} 1 \ if \ \hat{y}z + (1 - \hat{y})b \leq x_t^P \\ 0 \ otherwise \end{cases}, \ and$$

$$v_t^* = \begin{cases} 1 \ if \ \hat{y}z + (1 - \hat{y})b \geq x_t^C \\ 0 \ otherwise \end{cases}.$$

Beliefs are determined by Bayes's rule on the equilibrium path $p_1^* = 1$ following out-of-equilibrium actions by C, and $q_1^* = 1$ following deviations by P.

Proof: C with costs k_t^C makes a proposal from the interval $[x_L^P, x_H^P]$. Thus, there are both separating and pooling equilibria, but they all have the same equilibrium payoffs.

C's voting decision on the floor is trivial, so we begin with checking the optimality of y_t^*. Note that C's optimal strategy in the cabinet depends on b, since it determines the probability P will resign following a rejected confidence motion. Thus, we begin by checking y_t^* following equilibrium proposals b_t^*. Clearly, C with costs k_t^C does better by approving $z > b_t^*$, since it results in a better policy than if the CVM were rejected and b_t^* stood. Yet, each type of C will reject any CVM such that $b_t^* > z > x_L^P$, since this produces a worse policy and no greater likelihood of avoiding a resignation, because the low-cost P would still resign even if the motion were successful. C will also reject $z < x_L^P$, since his belief in this case is $q_1^* = 1$; C thus believes that P will accept b_t^* rather than resign. Finally, we need to show that C will accept $z = x_L^P < b_t^*$. Both P types make the same proposal, so the updated beliefs are $q_1^* = q_0$. Because the low-cost P will resign if the proposal is rejected, the utility of rejection is $q_0 u_C(b_t^*) + (1 - q_0)[u_C(x_0) - k_t^C]$, whereas the utility of acceptance is $u_C(x_L^P)$. Thus, C will accept so long as

$$q_0 \leq \frac{u_C(x_L^P) - u_C(x_0) + k_t^C}{u_C(b_t^*) - u_C(x_0) + k_t^C},$$

which is guaranteed by

$$q_0 \leq \frac{u_C(x_L^P) - u_C(x_0) + k_L^C}{u_C(x_H^P) - u_C(x_0) + k_L^C}.$$

This condition of q_0 is not only sufficient but also necessary to guarantee that low-cost C will not defect to proposing $b = x_H^P$ and rejecting all $z < x_H^P$.

Now consider y_t^* on paths of play following $b \neq b_t^*$. If $b < x_L^P$, then neither type of P will resign following a rejected CVM, and any motion will be rejected unless $z \geq b$. The best response to z following $b \neq b_t^* \in [x_L^P, x_H^P]$ is identical to that on the equilibrium path. Finally, if $b > x_H^P$, rejecting z leads to a resignation with certainty. Thus, C with cost k_t^C is willing to accept so long as $z \geq x_t^C$.

Turning to P's choice of CVM, it is easy to see that $z \geq b$ makes P worse off, since C will always accept this policy concession. If $b > x_H^P$, then $p_1^* = 1$ and $z = x_H^C$ is optimal. For $b \in (x_L^P, x_H^P]$, any $z \neq x_L^P$ will be rejected, yielding $u_P(x_t^P)$. Thus, P of type L weakly prefers $z = x_L^P$, which the high-cost P strictly prefers. By an analogous argument, accepting $b = x_L^P$ is optimal for both types of P. Finally, if $b < x_L^P$, any $z < b$ will be rejected in the cabinet, yielding an outcome of b. Thus, either type of P can do no better than accept b.

It only remains to consider C's optimal bill. Given the preceding strategies and beliefs, for any $b > x_H^P$, each C receives $u_C(x_t^C)$, and for any $b < x_L^P$, C receives $u_C(b)$. Because all $b \in [x_L^P, x_H^P]$ always yield $u_C(x_L^P)$, C of either type prefers $b \in [x_L^P, x_H^P]$ to any other bill and may choose any of these bills in equilibrium.   *Q.E.D.*

Case 2: If

$$\frac{u_C(x_L^P) - u_C(x_0) + k_L^C}{u_C(x_H^P) - u_C(x_0) + k_L^C} \leq q_0 \leq \frac{u_C(x_L^P) - u_C(x_0) + k_H^C}{u_C(x_H^P) - u_C(x_0) + k_H^C},$$

the following strategies and beliefs constitute a perfect Bayesian equilibrium:

$$b_L^* = x_H^P,$$

$$b_H^* = x_L^P,$$

$$a_L^* = \begin{cases} \text{accept if } b \leq x_L^P \\ \text{propose } z = x_H^C \text{ otherwise}, \end{cases}$$

$$a_H^* = \begin{cases} \text{accept if } b \leq x_H^P \\ \text{propose } z = x_H^C \text{ otherwise}, \end{cases}$$

$$y_t^* = \begin{cases} 1 & \text{if } z \geq b \\ 1 & \text{if } b > x_H^P \text{ and } z \geq x_t^C, \text{ and} \\ 0 & \text{otherwise} \end{cases}$$

$$r_t^* = \begin{cases} 1 & \text{if } \hat{y}z + (1 - \hat{y})b \leq x_t^P \\ 0 & \text{otherwise}. \end{cases}$$

Beliefs are determined by Bayes's rule on the equilibrium path $p_1^* = 1$ following out-of-equilibrium actions by C, and $q_1^* = 1$ following deviations by P.

Proof: Because C plays a separating strategy, high-cost C has his bill, x_L^P, accepted, whereas following $b_L^* = x_H^P$, low-cost P responds with a CVM that is rejected in the cabinet, leading to resignation by the low-cost P.

We begin by checking the optimality of y_t^* following equilibrium proposals by C. As above, C of either type does better by approving any $z \geq b$. Consider C with high costs, who proposes $b_H^* = x_L^P$. Because P will not resign given this proposal regardless of her costs, the C with high costs will reject any $z < b_H^* = x_L^P$. If $z = x_H^C$ is proposed following an equilibrium proposal by high-cost C, then it is off the equilibrium path, $q_1^* = 1$, and the high-cost C will likewise reject it. Now consider low-cost C, who proposes $b_L^* = x_H^P$. If $x_H^P > z > x_L^P$, it must be rejected because $b_L^* = x_H^P$ results in a better policy, and P is no more likely to resign under z than under b_L^*. Consider $z \leq x_L^P$. To see that these proposals must be rejected by the low-cost C, note that there are two cases: $x_L^P \geq z \neq x_H^C$ and $z = x_H^C$. If $z \neq x_H^C$, $q_1^*(z) = 1$, rejecting z leads to $u_C(b_L^*)$, whereas accepting z leads to a utility of $u_C(z)$. Thus, C will reject. If $z = x_H^C$, low-cost C must obviously reject because, even if P resigns, this yields a higher utility than adopting z.

Now consider y_t^* on paths following out-of-equilibrium proposals by C. Consider $b > x_H^P$. Rejecting any z leads to a certain resignation, so C with costs k_t^C will be willing to accept if $z \geq x_t^C$. Now consider $b < x_L^P$. In this case, neither type of P will resign following a rejected CVM, and thus any such motion will be rejected unless $z \geq b$. Finally, consider $b \in (x_L^P, x_H^P)$. If $z(b) = x_H^C$, then for any beliefs, both C types obviously prefer to reject z. If $z \neq x_H^C$, then $q_1^* = 1$ and rejection is optimal because the expected outcome will be b.

We now turn to P's CVM strategy. As above, $z > b$ makes P worse off. Given y^*, upon observing b_t^*, P can make no confidence motion that will be accepted. Thus, if $b_t^* = x_L^P$, any strategy P adopts will yield $u_P(x_L^P)$ (because neither type will resign following a rejected confidence motion). If $b_t^* = x_H^P$, then P with high costs will not resign following a rejected CVM, which makes the utility of accepting b_t^* the same as the utility of proposing any z. Similarly, if $b_t^* = x_H^P$ and P has low costs, a rejected CVM will lead to resignation. Because the utility of resigning is greater than the utility of x_H^P, the low-cost P must reject this bill and propose any alternative that will be rejected by C (including $z = x_H^C$). Finally, following out-of-equilibrium proposal $b > x_H^P$, P believes that C is the high-cost type and will accept $z = x_H^C$ to avoid a resignation.

It only remains to consider the optimality of b_t^*. As in case 1, it is never optimal for C to adopt $z < x_L^P$ or $z > x_H^P$. For any $b \in (x_L^P, x_H^P]$, C's expected utility is $q_0 u_C(b) + (1 - q_0)[u_C(x_0) - k_t^C]$, which is maximized by $b = x_H^P$. From $b = x_L^P$, C receives $u_C(x_L^P)$. Therefore, since

$$\frac{u_C(x_L^P) - u_C(x_0) + k_L^C}{u_C(x_H^P) - u_C(x_0) + k_L^C} \leq q_0 \leq \frac{u_C(x_L^P) - u_C(x_0) + k_H^C}{u_C(x_H^P) - u_C(x_0) + k_H^C},$$

$b_H^* = x_L^P$ and $b_L^* = x_H^P$ are optimal. The upper bound on q ensures that high-cost C prefers x_L^P to x_H^P, and the lower bound ensures that low-cost C prefers x_H^P to x_L^P. *Q.E.D.*

Case 3: If

$$\frac{u_C(x_L^P) - u_C(x_0) + k_H^C}{u_C(x_H^P) - u_C(x_0) + k_H^C} \leq q_0,$$

the following strategies and beliefs constitute a perfect Bayesian equilibrium:

$$b_t^* = x_H^P,$$

$$a_L^* = \begin{cases} accept\ if\ b \leq x_L^P \\ propose\ z = x_H^C\ otherwise \end{cases}$$

$$a_H^* = \begin{cases} accept\ if\ b \leq x_H^P \\ propose\ z = x_H^C\ otherwise \end{cases}$$

$$y_t^* = \begin{cases} 1 & if\ z \geq b \\ 1 & if\ b > x_H^P\ and\ z \geq x_t^C \quad ,\ and \\ 0 & otherwise \end{cases}$$

$$r_s^* = \begin{cases} 1 & if\ \hat{y}z + (1 - \hat{y})b \leq x_s^P \\ 0 & otherwise \end{cases}.$$

Beliefs are determined by Bayes's rule on the equilibrium path $p_1^* = 1$ following out-of-equilibrium actions by C, and $q_1^* = 1$ following deviations by P.

Proof: The proof is identical to case 2, except that

$$\frac{u_C(x_L^P) - u_C(x_0) + k_H^C}{u_C(x_H^P) - u_C(x_0) + k_H^C} \leq q_0$$

makes $b_H^* = x_H^P$ optimal. *Q.E.D.*
Comment: To establish the properties of the two critical values

$$\frac{u_C(x_L^P) - u_C(x_0) + k_L^C}{u_C(x_H^P) - u_C(x_0) + k_L^C} \text{ and } \frac{u_C(x_L^P) - u_C(x_0) + k_H^C}{u_C(x_H^P) - u_C(x_0) + k_H^C},$$

note that $u_C(x_t^P)$ is increasing in k_t^P. Thus, the first critical value is increasing in k_L^P and k_L^C, decreasing in k_H^P. The second has the same properties except that it increases in k_H^C rather than k_L^C.

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Congressional Decision-Making and the Separation of Powers

ANDREW D. MARTIN *Washington University in St. Louis*

To what extent does the separation of powers affect congressional roll call voting behavior? To answer this question, I offer a strategic model of congressional decision making that asserts members of Congress pursue public policy goals when casting roll call votes. From the equilibrium predictions of a formal model, I generate testable hypotheses by computing the expected net amount of sophisticated (nonsincere) congressional behavior given changes in decision context. I test the predictions of the theoretical model with data from all civil rights roll call votes from the 83d to the 102d Congress. The results demonstrate that both the other legislative chamber and the Supreme Court profoundly constrain House members and senators when casting roll call votes. This is strong evidence of the importance of policy outcomes to members of Congress when voting on the floor.

When building (and later trumpeting) their legislative accomplishments, members of Congress refer both to the votes they cast and the public policy enacted during their watch. If Congress acted alone in the policymaking process, these actions would be one and the same. Yet, because of the separation of powers, taking a position by casting the right vote may come in conflict with pursuing the right policy outcome. This is because policy is not formed by Congress alone in a system characterized by the separation of powers, but in conjunction with the president and the Supreme Court. Indeed, voting for a specific bill does not guarantee the outcome prescribed in the legislation because the president and the courts can review the established policy.

A foundational assumption of congressional research is that members of Congress pursue reelection (Mayhew 1974). To do so, Mayhew argues that members of Congress advertise (create a favorable image through behavior devoid of issue content); claim responsibility "for causing the government, or some unit thereof, to do something [desirable]" (p. 52); and take positions on various issues of the day. Clearly, members of Congress use all these behaviors to pursue reelection. Yet, in an institutional context, there may be an inconsistency between the credit-claiming and position-taking strategies.

When casting a roll call vote, a member of Congress may simply be seeking to go on the record for or against a particular policy, or she may be interested in claiming credit for a tangible policy outcome. On the

one hand, a member of Congress interested in taking a position can always vote sincerely for her most preferred policy. On the other hand, if policy outcomes are important, she cannot always vote her preference because Congress, the president, and the Supreme Court work together to form public policy (Eskridge 1991; Marks 1989). Thus, if members want to claim credit for policy change, they must be mindful of the separation of powers. As I argue below, adopting a credit-claiming strategy rather than simply taking a position can lead to profoundly different congressional behavior under certain circumstances. For example, had supporters of the Violence Against Women Act of 1994 voted for a more moderate bill, it is possible the Supreme Court would not have struck down the civil damages provision. In this case, the final policy was significantly different from the bill passed by Congress and signed by the president.

My purpose is to examine the effect of the separation of powers on congressional behavior. I address two questions that are inextricably related. First, does the separation of powers affect congressional roll call voting behavior? Second, when members of Congress cast roll call votes are they motivated by a desire solely to take a position, or both take a position and claim credit? I begin with a brief review of the literature, highlighting the predominant assumption of sincere behavior. I then posit a formal model of congressional decision making and the separation of powers, from which I generate hypotheses about congressional behavior. Next, I focus on research design, in particular case selection, measurement, and statistical issues. This section is followed by an analysis of roll calls on civil rights bills from the 83d to the 102d Congress. The results demonstrate that both House members and senators are systematically and significantly constrained by the separation of powers in their roll call behavior; that is, they take positions and strategically claim credit with their roll call votes. I conclude with a discussion of the implications of this research.

CONGRESSIONAL GOALS: VOTES OR POLICY?

The literature is rich with discussions of congressional goals. Some authors posit a strict reelection goal

(Arnold 1990; Fiorina 1974; Mayhew 1974), and others argue for multiple goals (Fenno 1973; Kingdon 1989), such as reelection, intra-Washington influence, and obtaining good public policy. Members of Congress pursue these goals in many different ways, one of which is through their roll call votes.

The Policy Dimension Theory

The policy dimension theory of decision making is reflected in the scaling literature of the 1950s and 1960s (see, e.g., Belknap 1958; Miller and Stokes 1963). Clausen (1973, 14) provides a theoretical foundation:

> The policy dimension theory states that legislators reduce the time and energy requirements of policy decision-making by (1) sorting specific policy proposals into a limited number of general policy content categories and by (2) establishing a policy position for each general category of policy content, one that can be used to make decisions on each of the specific proposals assigned to that category.

Thus, for a single dimension, the theory predicts that individuals have a preferred position (derived from a multitude of factors), and they make choices based on that position.

The Ideological Model

In the late 1970s a separate literature emerged and asserted the existence of one (or more) ideological dimensions that structure all congressional voting. This ideological model contends that members of Congress "make their policy choices on the basis of some internalized set of political values and beliefs about the world" (McCormick and Black 1983, 45). Poole and Rosenthal (1997) demonstrate that one predominant (and occasionally a second) dimension has structured American politics for more than two centuries. In many different policy areas there is a strong relationship between ideology and congressional roll call votes (e.g., Kau and Rubin 1979; Richardson and Munger 1990; Segal, Cameron, and Cover 1992). The difference between the policy dimension theory and the ideological model concerns dimensionality, but both are consistent with the spatial voting model (Downs 1957; Enelow and Hinich 1984). The spatial model contends that members of Congress have a fixed policy preference and vote for the option closest to them in an ideological space; that is, members of Congress vote sincerely, which is consistent with a position-taking strategy.

The question remains as to whether members of Congress just take a position with their roll call votes or also strategically pursue policy outcomes to facilitate credit claiming. Credit claiming and position taking often are manifested in identical voting patterns, but in other cases strategic members of Congress may vote for a less preferred policy at the roll call stage to get better policy after review by the president and the Supreme Court. The null hypothesis for this research is that members of Congress exclusively take positions with their roll call votes and hence are not constrained

by the separation of powers. As noted earlier, if supporters of the Violence Against Women Act of 1994 had chosen an alternative remedy instead of civil damages, it is likely that the Court would not have struck down the enforcement provisions of the bill. In other words, voting for a less preferred bill could have produced a preferable policy outcome. The alternative to the null is that members of Congress take position and claim credit when voting on the floor, that is, behave strategically.

THEORY AND HYPOTHESES

To isolate the circumstances when position-taking and credit-claiming behavior differ, I construct an account of congressional decision making under the separation of powers. My explanation departs from the literature in that I assume members of Congress strategically pursue public policy in the separation of powers system.

Assumptions

The first assumption is that members of Congress pursue policy goals when voting on the floor. A number of factors may shape policy preference, including personal goals, ideological goals, or instrumental goals of satisfying constituents or a particular interest group. Yet, as argued by Clausen (1973), in whatever manner these competing interests are weighed in the congressional mind, members have a preferred policy position that they hope to pursue with their votes on the floor. This account is consistent with the notion of an induced preference; that is, policy preferences held by members of Congress may not be personal but are induced by external factors, such as constituents or interest groups. The assumption of goal-seeking behavior pervades models of congressional decision making (Clausen 1973; Kingdon 1989; Mayhew 1974; Poole and Rosenthal 1997).[1]

The second assumption is that policy outcomes matter to members of Congress. The strategic account asserts that they are not as concerned with the outcome of a particular vote as they are with the final policy that emerges from the separation of powers system (Arnold 1990). Citizens in our democracy are profoundly affected by public policy, not individual roll call votes; public policy, implemented by the executive branch and refined by future Congresses and the Supreme Court, is what pertains to their lives. Thus, to pursue reelection and other legislative goals, it is reasonable to assume that members of Congress are concerned with the ultimate state of public policy.[2]

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[1] In certain circumstances constituents with spatial preferences may produce a majority cycle, which yields no unique preference for the member of Congress (Bianco 1994; McKelvey 1979), who then may pursue policies to achieve other, nonconstituent goals. The argument presented below turns on whether members of Congress have single-peaked preferences, not on whether these are derived from constituencies or some other factor (Clausen 1973).

[2] To use credit claiming in a campaign, members of Congress must realize a tangible policy outcome. Yet, strategic members of Con-

The final assumption is that members of Congress are strategic. Simply put, they rationally pursue public policy in an interdependent choice setting. Nearly all legislation that passes Congress must be signed or vetoed by the president, and the Supreme Court has the power of judicial review. Thus, any policy decision made by Congress is preliminary. In the end, public policy may be quite different from the bill Congress passes. Implicit in this assumption is the notion that none of the branches has the final say on public policy. Indeed, legislation can be overturned by the Court (on statutory or constitutional grounds), and its decisions can be overturned by Congress and the president, including those decided on constitutional grounds (Eskridge 1991; Murphy 1964; Rosenberg 1992). Because legislative majorities are always changing, and because legislating is inherently costly, strategic members of Congress should be mindful of the other branches, including the Court, when casting roll call votes. I thus assume that congressional voting is consistent with the equilibrium of the model presented below.

The Separation of Powers Model

The separation of powers game is played by four actors: a member of Congress, the median member of Congress in the other chamber, the president, and the median justice of the Supreme Court. I make three assumptions. First, each political actor has well-defined and stable preferences on a policy continuum. Actors derive utility exclusively from policy; the closer is the policy outcome to their ideal point, the more utility they receive. This implies that each actor's utility function is single-peaked. Second, all players have complete and perfect information about the bliss points of the member of Congress voting on the proposal, the median member of the other chamber, the president, and the median Supreme Court justice.[3] Third, the game is played in a unidimensional policy space on the unit interval. By assumption, the policy alternative is to the left of that status quo $(A < Q)$.[4]

The game proceeds as follows. The member of Congress (whose bliss point is denoted x_c) moves first and must vote for one of two exogenously fixed choices: an alternative A or the status quo Q. If the member votes for the status quo, the game ends, and policy does not change (with policy outcome Q). If the member votes for the policy alternative, the bill proceeds to a conference, where a compromise with the median member of the other chamber (bliss point x_o) yields a

revision $(x_o + A)/2$.[5] The compromise bill proceeds to the president (bliss point x_e), who must sign or veto it. If the president vetoes the legislation, the game ends, and policy does not change (the outcome is Q).[6] If the president signs the bill, the legislation is then subject to judicial review by the median Supreme Court justice (bliss point x_j).[7] The Court can overturn the entire bill (returning policy to Q), overturn parts of it (resulting in an outcome located anywhere in the region bounded by $(x_o + A)/2$ and Q), or do nothing (making the bill signed by the president the final policy outcome $(x_o + A)/2$. Once the game is over, the policy outcome is determined, and payoffs are assigned by simple quadratic utility functions.[8]

Hypotheses

Testing predictions of formal models is a difficult task, fraught with epistemological challenges (as cautioned by Green and Shapiro [1994] and their subsequent critics). Typically, one finds the equilibria of a model and checks to see if the behavioral predictions are observed. This strategy is quite arduous because each parameter of the model must be measured on the same scale without error, due to the knife-edge cut points that often characterize the equilibria. In addition, one needs to observe multiple realizations of the game with varying parameter values to test comparative statics results. In most observational settings, neither of these is empirically feasible.

To overcome these problems and generate directly testable hypotheses, I take a different approach. I begin by making behavioral predictions for both the strategic and nonstrategic accounts, and I then exploit the observable differences between these types of behavior. These occur when members of Congress are observably nonsincere (termed by Krehbiel and Rivers [1990, 549] "empirically identifiable sophisticated behavior").[9] I average over the distribution of congressional bliss

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gress may not realize their preferred outcome because of presidential and judicial review. Even if a policy is not ultimately realized, its pursuit will yield strategic congressional behavior.
[3] "Bliss point" will be used interchangeably with preferred policy position.
[4] This assumption is purely historical. In the civil rights policy domain, most new proposals have been to the left of the status quo (although some votes were cast on conservative amendments). In the data analysis presented below, all votes are coded such that this assumption holds.

[5] Many times, such as with the Civil Rights Act of 1957 and the Civil Rights Act of 1964, bills not in agreement bypass the conference stage, as the leadership can gain passage for a compromise bill in both chambers.
[6] If the president vetoes the bill, it is certainly plausible that Congress will pass another. This occurred when President Bush vetoed the Civil Rights Act of 1990. Congress then passed a compromise bill, which became the Civil Rights Act of 1991. The second piece of legislation, however, is another realization of the separation of powers game, as is a veto override attempt (without presidential review).
[7] Only four of the nine justices are needed to grant certiorari. For a precedential decision to be reached, however, a majority is needed.
[8] The model assumes that the member of Congress believes with nonzero probability that (1) she is pivotal on the particular vote and (2) a compromise bill will always emerge from conference. These are certainly tenuous assumptions. Yet, if a member thinks she is not pivotal or that the conference committee will never report a compromise, then she is free to take a position and cast a sincere vote. This biases all empirical results in the proper direction toward the null hypothesis of sincere behavior.
[9] I will employ the term "sophisticated" to denote observably sophisticated (nonsincere) behavior. There is a formal theoretical and applied literature that documents sophisticated voting in Congress over agendas (see Calvert and Fenno 1994; Enelow and Koehler 1980; Volden 1998; c.f. Krehbiel and Rivers 1990). Rather than look

points, which yields predictions about the net amount of sophisticated congressional behavior for a particular decision context. From these I generate testable hypotheses about the net amount of sophisticated congressional behavior as a function of political decision context (see Martin 1998).

I characterize the subgame perfect Nash equilibrium of this simple game using backward induction, deriving a function $\sigma_c^*(x_c, x_o, x_j, x_e)$ that produces the equilibrium congressional strategy, given the member's bliss point and the political context (Martin 1998). To form a baseline for comparison, one can define a similar function for a nonstrategic member of Congress. Because nonstrategic actors do not take into account the decision context when casting votes, they always behave sincerely; that is, they vote for the policy option they most prefer. Given the assumption that $A < Q$, the function $\Psi(x_c)$ predicts votes for the nonstrategic account:

$$\Psi(x_c) = \begin{cases} 0 & \text{if } 0 \leq x_c \leq (A + Q)/2 \\ 1 & \text{if } (A + Q)/2 < x_c \leq 1 \end{cases} \cdot$$

This function implies that a member of Congress will vote for the option closer to her bliss point; if she falls to the left of the cutpoint, she votes for A, and if she falls to the right, she votes for Q. Note that $\sigma_c^*(\cdot)$ is a function of the policy preferences of the member of Congress and the other actors that define the decision context, whereas $\Psi(\cdot)$ is only a function of the bliss point of the member of Congress. To compare the strategic and nonstrategic models, one must compare the predictions of $\sigma_c^*(\cdot)$ and $\Psi(\cdot)$.

For many members of Congress in a particular decision context, the strategic model $\sigma_c^*(\cdot)$ and the nonstrategic model $\Psi(\cdot)$ predict precisely the same behavior. For others, however, they differ. The equilibrium predictions of the formal model show that sophisticated (nonsincere) behavior will occur in the following case. Suppose we have a moderate/conservative member of the House faced with the prospect of voting for or against a civil rights proposal. He sincerely prefers the status quo to the policy alternative, but since the players in the rest of the separation of powers system are moderate (compared to the status quo), he can vote for the liberal policy with the confidence that more moderate—and thus more preferable—policy will emerge in the end. This is illustrated in the sophisticated voting areas in Figure 1. In other words, a moderate can vote for a bill that is too liberal on its face with the expectation that, at the end of the game, the ultimate policy will be more moderate because of executive and judicial review.

To compare the behavioral predictions of the two explanations, one must compute the difference between these functions. For a particular member of Congress in a particular decision context, the absolute value of the difference between $\sigma_c^*(\cdot)$ and $\Psi(\cdot)$ equals 1 if she is acting in a sophisticated manner, 0 otherwise. Assuming a fixed proper distribution of congressional

preferences $f(x_c)$ supported on the unit interval, one can write the following composite function:

$$s(x_o, x_e, x_j) = \int_0^1 |\sigma_c^*(\cdot) - \Psi(\cdot)| f(x_c) \, dx_c.$$

The function $s(\cdot)$ averages over the distribution of bliss points and provides the net amount of sophisticated congressional behavior for a particular decision context, parameterized only by the measures of political context. To generate testable hypotheses, I perform comparative statics on this net sophisticated voting function $s(\cdot)$ (Martin 1998). By determining whether this function is increasing or decreasing in the parameters that measure political context, one can predict changes in the net amount of sophisticated congressional behavior.

The first analysis concerns the median member of the other chamber. I am interested in how a senator alters her behavior in response to a House more conservative with respect to civil rights, or how a House member responds to a more conservative Senate. In result 1, I prove that the net sophisticated voting function $s(\cdot)$ is increasing in the bliss point of the median member of the other chamber x_o (Martin 1998, 308). This proof requires a substantive assumption that the two chambers are close enough in ideological space such that the compromise in conference is less conservative than the status quo.[10] The first hypothesis follows. ·

HYPOTHESIS 1. *Holding all else constant, as Congress becomes more conservative, the net amount of sophisticated voting behavior in the other chamber will increase.*

This hypothesis is illustrated in the top two lines of Figure 1. This figure contains six realizations of the separation of powers game, and it illustrates all three hypotheses using typical examples. Both the Court and the president prefer the policy proposal $(x_o + A)/2$ to Q. Thus, members of Congress between the cutpoint $(A + Q)/2$ and $(x_o + A + 2Q)/4$ will vote in a sophisticated manner for A because they prefer the policy outcome to the status quo. As the other chamber becomes more conservative, illustrated in the second line of the figure, the width of the interval increases, which implies an increase in the net amount of sophisticated congressional voting behavior.

The second comparative static concerns the policy preference of the president x_e, based on my result 2 that net sophisticated voting function $s(\cdot)$ is decreasing in the president's bliss point x_e (Martin 1998, 309).

HYPOTHESIS 2. *Holding all else constant, as the president becomes more liberal, the net amount of sophisticated congressional voting behavior will increase.*

In Figure 1, the third line represents a conservative presidency. Here, the president vetoes all legislation,

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at agenda setting, however, I focus here on sophisticated behavior with respect to the separation of powers.

[10] This assumption is benign, as no member of Congress would agree to play the game in the pathological situation where $(x_0 + x_c)/2 > Q$.

FIGURE 1. Illustrations of Hypotheses for Particular Realizations of the Game

Hypothesis One

Hypothesis Two

Hypothesis Three
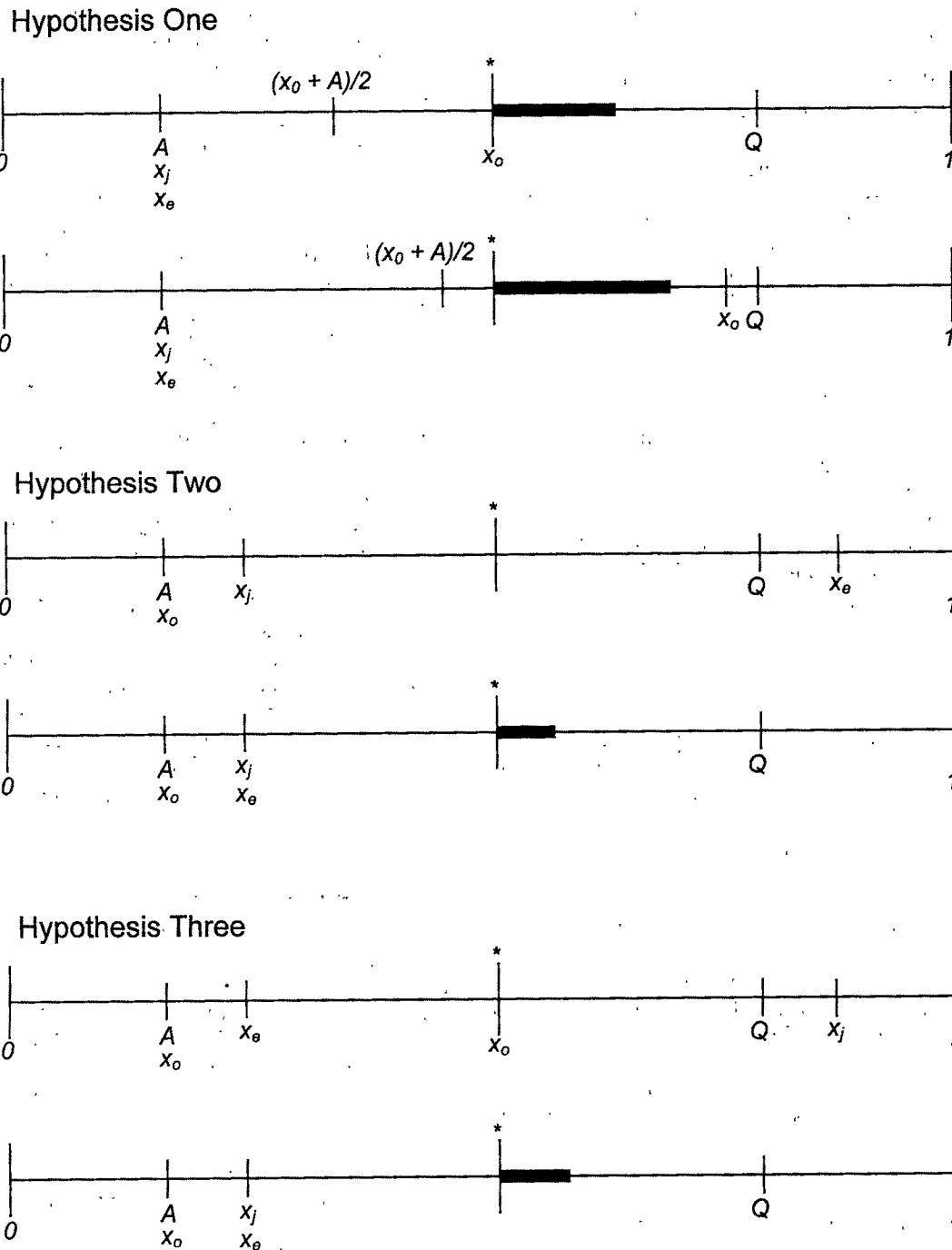
Note: $* = (A + Q)/2$, and ■ denotes sophisticated congressional behavior. All other quantities are the parameters of the game: the status quo Q, the policy alternative A, the median member of the other chamber x_o, the president x_e, and the median Supreme Court justice x_j.

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so no sophisticated congressional voting behavior is predicted. For a more liberal president, the story changes. In the fourth line of Figure 1, all members of Congress between $(A + Q)/2$ and $(x_j + Q)/2$ will behave in a sophisticated manner and vote for A instead of their sincere preference Q. This suggests that as the president becomes more liberal, on balance more members of Congress will behave contrary to their sincere preferences.

The final comparative static concerns the policy preference of the median Supreme Court justice x_j. In result 3, I prove that the net sophisticated voting function $s(\cdot)$ is decreasing in the bliss point of the median Supreme Court justice x_j, given a substantive assumption that the Court is either liberal or conservative but not moderate (near the status quo) (Martin 1998, 310). From this result, I state the final hypothesis.

HYPOTHESIS 3. *Holding all else constant, as the median Supreme Court justice becomes more liberal, the net amount of sophisticated congressional voting behavior will increase.*

This hypothesis is illustrated in the final two lines of Figure 1. In the first instance, a conservative Court overturns all legislation, which means that each member of Congress will vote sincerely. When the Court is more liberal (x_j decreases), more members of Congress have the incentive to behave in a sophisticated manner and vote for A (in fact, it will be those members between $(A + Q)/2$ and $(x_j + Q)/2$). This interval widens as the Court becomes more liberal, which comports with hypothesis 3.

RESEARCH DESIGN

To test the hypotheses, I consider all civil rights roll call votes between 1953 and 1992. This issue area (1) encompasses a substantial amount of legislation, (2) involves considerable litigation, (3) is politically contentious, which produces interinstitutional conflict, and (4) is politically important, especially throughout the last half of the century.

Case Selection

The substantive focus is all roll call votes on civil rights bills from the 83d to the 102d Congress. During this period, Congress passed many important pieces of civil rights legislation. Some were signed into law (e.g., the Civil Rights Act of 1964), some failed to pass through the legislature (e.g., the Civil Rights Act of 1966), and some were vetoed by the president (e.g., the Civil Rights Act of 1990). The issue dominated politics in the 1960s and continued to play an important role thereafter.

I am not aware of a highly reliable database of congressional roll calls from which one can sort votes into specific issue areas. Thus, selecting roll call votes that relate to civil rights is not a trivial matter. Clausen (1973) defines civil liberties very broadly, encompassing legislation that ranges from freedom from unsanctioned violence and the right to a fair trial, to the protection of constitutionally guaranteed rights. To avoid the possibility of multidimensionality, I focus on civil rights more narrowly defined. According to Black (1991, 169), civil rights legislation is "intended to implement and give further force to basic personal rights guaranteed by [the] Constitution, ... [which] prohibit discrimination in employment, education, public accommodations, etc. based on race, color, age, or religion." My goal is to select all roll calls that explicitly address the governmental guarantee of the rights provided in the Bill of Rights and subsequent amendments to the Constitution.

I begin by broadly selecting all cases that address race, gender, and sexual preference. Poole and Rosenthal (1997) provide a database of roll call votes (of unknown reliability) that classifies legislation with ninety-nine specific issue codes. From this I selected all

legislation coded as women's equality, civil rights/desegregation/busing/affirmative action, homosexuality, voting rights, and (nonblack) minorities (pp. 260–2). This selection, however, has a number of limitations. It not only puts implicit and explicit civil rights legislation into one category but also fails to identify some important legislation that affects civil rights (such as education and appropriations legislation). To add to this universe of cases, I turned to another source of roll call votes: the *Congressional Quarterly Almanac* (CQ 1953–92). I used the index to select all legislation that affects civil rights as defined above. I specifically included all pieces of legislation indexed by the following words: affirmative action, civil rights, desegregation, discrimination, equality, homosexuality, literacy test, minorities, poll tax, voting rights, and women. I combined both sets to form my universe of civil rights roll calls.

Some of these roll calls in the universe of cases explicitly affect civil rights (by outlawing discrimination in housing or employment), but others have only an implicit effect (by removing jurisdiction from federal courts, stripping funds from the Department of Justice, or procedural votes in the House and Senate). To narrow this selection to all legislation with a direct effect, I used the abstracts published with each roll call vote in the *Congressional Quarterly Almanac*. This procedure identified a set of roll calls related to explicit votes for or against protecting civil rights, without the ambiguity that arises in implicit votes. This data set covers votes on amendments, motions to table an amendment or bill, and final passage of bills and resolutions.

Data and Measurement

The dependent variable in this analysis is the dichotomous congressional vote for or against the protection of civil rights. I first built a data set of individual votes for each roll call. For the 83d to the 97th House, I used data from Poole and Rosenthal (1989). For the 98th to the 102d House and the 83d to the 102d Senate, I used data from the Inter-university Consortium for Political and Social Research and Congressional Quarterly (1997). After all votes were selected, the data were reshaped and stacked into two data sets: one for the House of Representatives and one for the Senate.[11] I coded all liberal votes (providing rights or protection to minority groups) as 0 and all conservative votes (stripping rights or protection to minority groups) as 1. Thus,

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[11] In the empirical model I assume that, conditional on knowing a member's preference, roll call votes are independent within decision contexts. Clearly, there are members of Congress who can manipulate the agenda to achieve their preferred outcomes (see Enelow and Koehler 1980; McKelvey 1979), which means that votes may not be independent in a particular Congress. Yet, Austen-Smith (1987) demonstrates that an endogenous agenda will produce sincere observed behavior. This lack of independence will bias any empirical analysis toward the null hypothesis of sincere behavior. As suggested by an anonymous reviewer, I reestimated the House model excluding cases considered under a closed rule. The results are robust given this specification (see Table 3 on the replication website).

in the data set, $A < Q$, which comports with the analytic assumption used to generate the hypotheses.[12]

To explain the variance in this dependent variable I need two sets of independent variables: one that measures policy preferences for members of Congress and one that measures the political context of the decision. In the analysis, I use Nominate Common Space first-dimension scores to measure congressional preferences (Poole and Rosenthal 1997).[13] These scores are estimated in the same space across chambers and time, and they ranged from approximately -0.6 (liberal) to 0.6 (conservative) during this period. Poole and Rosenthal use an elaborate multidimensional scaling algorithm on all roll call votes to produce these scores.[14] In contrast to D-Nominate scores, all members of Congress only get one score based on their entire voting record. These scores are also appealing because they lie on a common space across all Congresses and in both chambers. Poole and Rosenthal use these scores to illustrate stability in congressional voting and to explain voting behavior in many different policy domains.[15]

Political Context

My strategic account predicts that members of Congress behave in profoundly different ways depending upon the context in which they make their decision. The first measure I require to gauge decision context is one of legislative preferences in the other chamber (x_o).[16] To comport with the formal model, I use the

median member of the other chamber measured with the Nominate Common Space first-dimension scores.[17] The analysis also requires a measure of presidential policy preferences (x_e). One possibility is the Nominate Common Space measure based on presidential position taking (Poole and Rosenthal 1997). In its stead, I use a measure not directly based on behavior and constructed from a survey of historians and presidency scholars (Segal, Timpone, and Howard 2000). I rescale their social liberalism measure into a social conservatism measure that ranges from 0 to 1.[18] To test hypothesis 3, I require a measure of the median Supreme Court justice (x_j). Here I use another measure not directly based on behavior (Segal and Spaeth 1993). It has been shown highly reliable in the civil rights domain (Epstein and Mershon 1996). I rescaled these scores from 0 to 1, representing more or less conservatism.

Statistical Models

My theory dictates that strategic and nonstrategic members of Congress behave quite differently because of the separation of powers. In the nonstrategic account, members of Congress care about position taking and always vote sincerely for their preferred policy alternative. In contrast, strategic members who care about policy outcomes sometimes cast sophisticated votes to pursue their policy goals. The amount of sophisticated voting depends on the other actors in the separation of powers game.

To model liberal and conservative roll call votes statistically, let $y_{i,k}$ represent a dichotomous congressional decision made in decision context k. The range of decision contexts is $k = 1, \ldots, K$. The total number of roll call votes cast in each decision context also varies, implying $i = 1, \ldots, n_k$. A nonstrategic explanation of congressional behavior predicts that votes are functions of preferences alone. Thus, decisions made by every member of Congress in every decision context can be pooled. Because decisions are dichotomous, the nonstrategic account can be tested with a standard probit model. Using a latent utility specification, where $z_{i,k}$ represents an unobserved utility function, yields

$$y_{i,k} = \begin{cases} 1 & \text{if } z_{i,k} > 0 \\ 0 & \text{if } z_{i,k} \leq 0 \end{cases},$$

$$z_{i,k} = x'_{i,k}\beta + \varepsilon_{i,k} \qquad \varepsilon_{i,k} \sim N(0, 1).$$

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[12] This coding convention was used to maximize the size of the sample, particularly the number of decision contexts. It is possible, however, that including votes on conservative proposals may bias the empirical results. Please refer to Appendix Table A-1.

[13] The second-dimension scores have been shown useful in explaining additional variance in voting patterns in some policy areas, including civil rights in certain (but not all) Congresses. I employ the first dimension because of its continued importance throughout this period (Poole and Rosenthal 1997). To test the robustness of my results, I reestimated all models using only the Nominate Common Space second-dimension scores (appropriately recoded). The substantive conclusions reached are robust given this specification (see tables 1 and 2 on the replication website).

[14] There is an inherent endogeneity problem with this measurement strategy because Nominate scores are calculated from congressional votes. Because they are computed for all members of Congress across time, the effect of these particular civil rights votes may be minimal. An additional criticism is that the scores may come from strategic congressional behavior. Assume that members sometimes vote in a sophisticated manner and that we have a preference measure based on strategic votes with some random error. Within a decision context k, a probit model will predict votes equally well as for the case in which only sincere votes enter the measure. Sophisticated behavior is built into the measure, which makes some conservatives look more liberal than they sincerely are, and some liberals more conservative than they sincerely are. This behavioral equivalence suggests that the biases introduced by strategic congressional voting in the preference measure will bias the analysis toward the null of sincere behavior.

[15] Many factors can explain what members of Congress do: constituency concerns, interest group influence, the desire to move up in the party hierarchy, or the desire to become nationally prominent. Each member of Congress is motivated by one or many of these factors. I assume that these factors can be weighted by the member of Congress into a preferred policy position.

[16] Intrachamber strategic voting may bias the analysis toward finding

an influence from the other legislative chamber. An anonymous reviewer suggests an alternative specification that includes the median member of the chamber under consideration as a control variable at the second level of the hierarchical model. All results reported below are robust given this specification (see tables 4 and 5 at the replication website).

[17] In the Senate, one could use the cloture member, which Rule XXII stipulated before 1975 was the 2/3 member, the 3/5 member thereafter. Such a measure is highly correlated with the median measure employed in the analysis, and the results do not differ using either measure.

[18] This measure correlates at 0.926 ($n = 8$) with the inferential Nominate measure. The empirical results are the same for both measures.

For this formulation, $x'_{i,k}$ is a $(1 \times p)$ row vector of covariates, and β is a $(p \times 1)$ column vector of parameters (p denotes the number of explanatory variables). I adopt a Bayesian approach (see Jackman 2000) and estimate the model with the Gibbs sampling algorithm of Albert and Chib (1993), using conjugate noninformative priors. The row vector of covariates $x'_{i,k}$ contains two elements: a constant and my measure of the member's preference. Note that for this nonstrategic model, one estimates a single vector of parameters. The estimated β_2 coefficient gauges the strength of the preference/behavior relationship. If it is positive and differs from zero with high probability, then one can conclude that preferences systematically affect congressional behavior.

The theoretical argument offered above suggests behavioral heterogeneity (Western 1998); that is, members of Congress behave in profoundly different ways depending on the context of their decision. Within a particular decision context k, the formulation of the model is nearly the same:

$$y_{i,k} = \begin{cases} 1 & \text{if } z_{i,k} > 0 \\ 0 & \text{if } z_{i,k} \leq 0 \end{cases},$$

$$z_{i,k} = x'_{i,k}\beta_k + \varepsilon_{i,k} \qquad \varepsilon_{i,k} \sim N(0, 1). \qquad (1)$$

The only difference is that the preference/behavior relationship β_k is measured for each decision context. As hypothesized above, the preference/behavior relationship should systematically covary with measures of political context. Indeed, as the net amount of sophisticated congressional voting behavior increases, $\beta_{k,2}$ should decrease, and vice versa.

To test these hypotheses, it is necessary to model the changes in the preference/behavior relationship across decision contexts. This can be done with a seemingly unrelated regression (SUR) model. Recall that β_k is a $(p \times 1)$ column vector of parameters. Thus:

$$\beta_k = W_k\alpha + v_k, \qquad v_k \sim N_p(0, \Omega). \qquad (2)$$

W_k is a $(p \times q)$ matrix of covariates, α is a $(q \times 1)$ vector of parameters, and Ω is a $(p \times p)$ variance-covariance matrix. Note that this formulation is hierarchical: The first level relates preferences to decisions, and the second level incorporates context by explaining variation in the β_k parameters. Since the strategic explanation indicates that the net amount of sophisticated congressional voting behavior should covary with three measures of context defined by the separation of powers, for the strategic probit models I present below, the measure of political context is:

$$W_k = \begin{bmatrix} 1 & 0 & 0 & 0 & 0 \\ 0 & 1 & x_o & x_e & x_j \end{bmatrix}.$$

The second-level parameters α are used to explain the variance in the preference/behavior relationship across decision contexts. If, for example, that relationship is constant across contexts (formally, $\beta_k = c$ for all k), then the α coefficients for the other chamber, the president, and the judiciary will be zero. If, as predicted in hypothesis 2, the preference/behavior relationship

strengthens as a function of presidential conservatism, then the α coefficient on the presidency measure will be positive. Thus, if an element of the α vector is positive, it means that sophisticated behavior is decreasing in that covariate, and vice versa. For example, hypothesis 1 predicts that the net amount of sophisticated congressional voting behavior will increase as the other chamber (x_o) becomes more conservative. Thus, the α coefficient on the other chamber should be negative.

If we knew with certainty the contextual effects (i.e., $v_k = 0$ for all k), then we could directly substitute equation 2 into equation 1 and would produce a standard probit model with a handful of interaction terms. After this substitution, our explanatory variables would be a constant, the preference measure, and the preference measure interacted with each of the contextual variables: x_o, x_e, and x_j (which vary across decision contexts). We could estimate a vector of α coefficients. In the tables of results that follow, I call this the interaction probit model, which can be estimated using standard techniques. Note that for this model we do not directly estimate each β_k in each context. In practice, however, it is unlikely that one can model these contextual effects with certainty. To relax this assumption, I employ Markov chain Monte Carlo (MCMC) estimation methods to estimate simultaneously the first-level parameter vectors β_k and the hyperparameters α and Ω. I refer to this as the hierarchical probit model. As it turns out, the latter outperforms the interaction probit model in all cases and gives a more reliable picture of the effect of the separation of powers on congressional voting. For a detailed discussion of estimation issues, please refer to the Appendix.

Testing the Hypotheses

To compare the strategic and nonstrategic explanations, I proceed as follows. First, I estimate a nonstrategic model of congressional decision making using the standard probit model. Then, to test hypotheses 1–3, I estimate an interaction probit model and a hierarchical probit model.[19] For both these models, the α parameters explain changes in the preference/behavior relationship and can be interpreted just like SUR coefficients. To conserve space, for the hierarchical model I summarize the posterior densities of the preference/behavior relationship $\beta_{k,2}$ for each decision context using boxplots, and I only report the α parameters and the variance-covariance matrix Ω. Finally, for all models, I report the log-marginal likelihood, which is useful for model comparison using Bayes factors (Kass and Raftery 1995).

As noted earlier, the α hyperparameters explain heterogeneity in congressional voting behavior. Thus, the α coefficients on the legislative, presidential, and

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[19] To check robustness of the results, I reestimated all models with various informative prior specifications, and the results remain robust (see tables 6 and 7 on the replication website as examples). I also performed posterior sample analysis to assure convergence of the samples.

FIGURE 2.   House Pooled Data, Local Regression Line
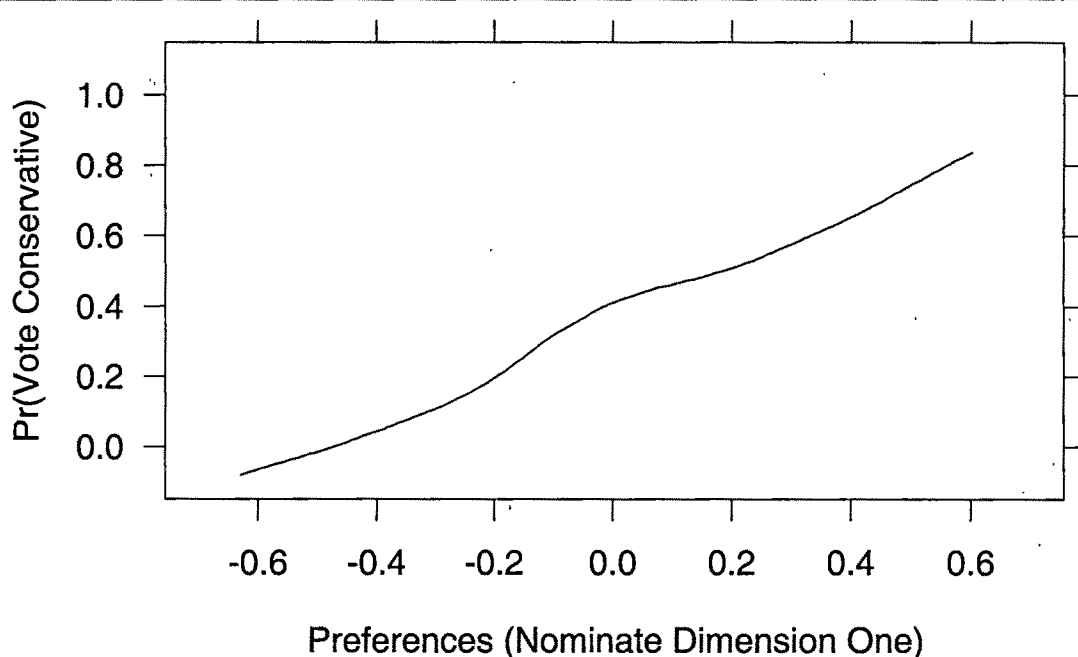
Note: The points are suppressed in the figure because the vote variable is dichotomous.

judiciary measures tell us how the other institutions affect the preference/behavior relationship for members of Congress and serve as the test of hypotheses 1–3. Hypothesis 1 indicates that, as the other chamber becomes more conservative, more sophisticated voting behavior is to be expected. This implies that the strength of the preference/behavior relationship decreases as the other chamber becomes more conservative. Thus, the α-Congress measure should be negative. Hypotheses 2 and 3 indicate that as the president or the median Supreme Court justice become more liberal, more sophisticated voting behavior should be observed. This implies that β_k increases as x_e and x_j increase. Thus, we expect the α-president and the α-Supreme Court coefficients to be positive. If these expectations are borne out, then one can reject the null that members of Congress do not respond strategically to the separation of powers and are solely motivated by position taking. As an additional comparison of the strategic and nonstrategic models, I also compute the Bayes factor between them.

THE HOUSE OF REPRESENTATIVES

I first examine decision making in the House of Representatives, where votes were cast on many pieces of civil rights legislation from the 84th to the 102d Congress (no votes were cast in the 83d). These votes were cast in $K = 34$ decision contexts.

The first result I present is from a nonstrategic account of congressional decision making. I pool all data across all decision contexts from 1953 to 1992 and investigate the relationship between preferences and behavior. Figure 2 contains a local regression line that relates House pref-

erences to votes. Because the vote variable is dichotomous, I suppress the data points in the graph. The line shows the probability of casting a conservative vote for a given preference. As expected, House preferences are directly related to the vote; as members become more conservative, they vote more often for conservative civil rights policy. This is far from an astonishing finding, but it serves as a baseline for comparison.

In the first column of Table 1, I summarize the posterior density from the probit model. Note that one can interpret the posterior mean or median just as one would interpret a point estimate in classical models, and the posterior standard deviation as the standard error. These results demonstrate that preferences are strongly related to the vote. Indeed, 100% of the posterior density sample for β_2 is positive. The posterior sample has a minute standard deviation.

Before I present statistical results from the strategic House model, I investigate how the preference/behavior relationship changes in different decision contexts using a graphical device called the conditioning plot. A conditioning plot is similar to the scatterplot in Figure 2, but the data are conditioned by a third variable. For these graphs, I divide the data based on the 25th, 50th, and 75th percentiles of the conditioning variable. Thus, in Figure 3, the bottom cell contains the data from the minimum to the 25th percentile of the Senate median measure, the next cell up contains data from the 25th to the 50th percentile of the Senate median measure, and so forth. By my strategic account, the slopes of the local regression lines should vary as the measures of decision context change. With these plots, one can easily see how the relationship changes as a function of another variable.

The coplot in Figure 3 contains the preference/

TABLE 1. Posterior Density Summaries for House Models

Variable	Pooled Probit Model			Interaction Probit Model			Hierarchical Probit Model		
	Post Mean	Post Median	Post StD	Post Mean	Post Median	Post StD	Post Mean	Post Median	Post StD
β_1 – Constant	−0.405	−0.405	0.008						
β_2 – Preference	2.309	2.309	0.032						
α_1 – Constant				−0.405	−0.405	0.008	−0.589	−0.590	0.253
α_2 – Preference constant				0.071	0.071	0.122	0.665	0.656	0.351
α_3 – Preference × Senate				−5.435	−5.428	0.472	−2.056	−2.044	0.935
α_4 – Preference × president				−0.379	−0.380	0.210	0.666	0.655	0.562
α_5 – Preference × judiciary				4.624	4.626	0.193	4.081	4.096	0.577
Ω_{11} – Error							0.481	0.452	0.156
Ω_{12} – Error							−0.008	−0.010	0.187
Ω_{22} – Error							2.187	2.101	0.574
Ln(marginal likelihood)	−17,041.62			−16,524.91			−14,675.55		
Burn-in iterations	500			500			500		
Gibbs iterations	5,000			5,000			5,000		
Contexts	34			34			34		
n	31,429			31,429			31,429		

Note: Uninformative prior distributions are used for all parameters. *Post Mean* denotes the posterior mean, *Post Median* the posterior median, and *Post StD* the posterior standard deviation. The models are estimated using Markov chain Monte Carlo (MCMC). See the Appendix for a discussion of estimation issues.

behavior relationship conditioned on the Senate median. Hypothesis 1 predicts that as the Senate gets more conservative, we expect more sophisticated behavior. The bottom three cells of the coplot show a strong relationship that resembles the pattern in Figure 2 (with a slight deviation in the moderate liberal cell). As the Senate median grows more conservative, the relationship lessens not only in level but also in curvature, which is apparent in the top cell. The nonstrategic baseline suggests that the probability of voting conservatively is strictly increasing in preferences. The pronounced bump in the top cell is suggestive of the behavior predicted in hypothesis 1.

Hypothesis 2 predicts that as the president becomes more conservative, the slope of the local regression line should increase. In the bottom cell of Figure 4 we see the relationship between preference and behavior during the Johnson years. Compare this with the top cell, when Richard Nixon and Ronald Reagan were in the White House. We see a strengthening relationship when the president is more conservative, which implies more sincere voting behavior. Again, we see departures from sincere behavior at the middle of the policy space in the bottom cell. For this bivariate analysis, this finding comports with hypothesis 2.

In Figure 5, I condition House behavior on the median Supreme Court justice. Again, the expectations are borne out. Indeed, in both of the lower cells we observe many moderate House members voting for conservative policy, which is consistent with a desire to prevent extreme liberal outcomes. This is why there are bumps around the middle of each graph. As the judiciary grows more conservative, we see a much stronger relationship. Indeed, during the early Burger

Court in the second cell, and the late Burger and Rehnquist Courts in the top cell, we see a much stronger preference/behavior relationship, which is consistent with hypothesis 3.

The exploratory data analysis just presented is persuasive on its face, the next step is to see whether the hypothesized relationships hold in a multivariate setting. The statistical results are given in the final two columns of Table 1. For the two strategic models, it is important to determine whether the simpler interaction model suffices or whether the hierarchical model is necessary. To make this determination, I rely on a model comparison tool called the Bayes factor. With an equal prior probability that each model is the true data-generating mechanism, the Bayes factor is simply the ratio of marginal likelihoods (or the difference between the log-marginal likelihood). The Bayes factor $B_{j,k}$ can be interpreted on the scale of probability that model j is the true data-generating mechanism compared to model k. If the Bayes factor is greater than five, it is very strong evidence that j is the better model (Kass and Raftery 1995, 777).

The Bayes factor between the hierarchical probit model and the interactive probit model is $B_{j,k} = 1849.36$, which suggests that the hierarchical model is superior. In addition, the statistically significant estimates of the error parameters suggest that the simplifying assumption needed to employ the interaction model does not hold. As we would expect, the interaction model has larger posterior means and smaller standard deviations than the hierarchical model, which overstate the confidence we have in the results. Although the interaction model serves to illustrate the modeling strategy for the strategic case, I will rely on

FIGURE 3. House Local Regression Conditioned on Senate Median
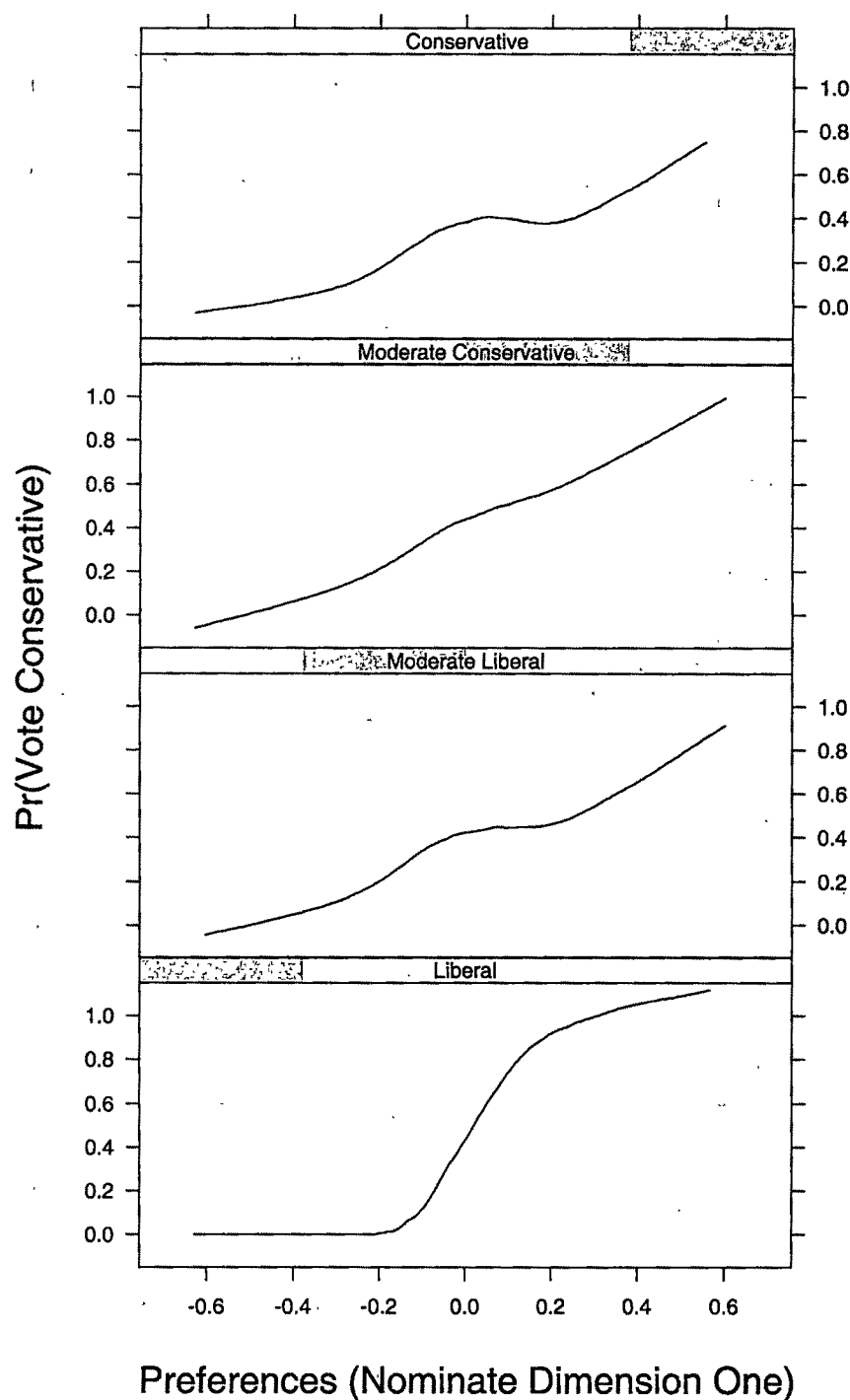
Note: The 25th, 50th, and 75th percentiles for the Senate median are −0.17, −0.12, and −0.08, respectively.

the superior hierarchical model for inference. As hypothesized, the posterior mean of α-Senate is negative (−2.06), with a standard deviation of 0.93. Because 98.4% of the posterior density sample is less than zero, there is a 98.4% probability that α-Senate is negative. This demonstrates; that the Senate significantly and systematically constrains House behavior, as predicted in hypothesis 1.

The results for the presidency are not as compelling.

Although the posterior mean is above zero, the posterior standard deviation is large, and only 88.9% of the posterior density is positive. This means that there is an 88.9% chance that this coefficient is positive, which many would not regard as a significant result. Thus, the conclusion drawn from Figure 4 is incorrect; in the multivariate analysis, the effect of the presidency is insignificant. The α-judiciary coefficient, however, is much stronger. The posterior mean for the judicial coefficient is large (4.08),

FIGURE 4. House Local Regression Conditioned on Presidential Social Conservatism

with a small standard deviation (0.58). Indeed, 100% of the posterior density sample is positive. This demonstrates that the Supreme Court constrains House behavior in the direction hypothesized in hypothesis 3.

In Figure 6 I plot posterior density boxplots for the $\beta_{k,2}$ coefficients that measure the strength of the preference/behavior relationship. For each decision context, these boxplots summarize the posterior density. For the purposes of comparison, I include the

boxplot for the pooled probit model on the far right of the figure. This is the level we would expect all the other boxplots to share if a nonstrategic account were appropriate. If members of Congress are nonstrategic, then each of these posterior density boxplots should share a common mean. As one compares the $\beta_{k,2}$ coefficients across contexts, there is clear evidence of variance in the preference/behavior relationship. Finally, to compare the strategic and nonstrategic ac-

FIGURE 5.   House Local Regression Conditioned on Judicial Conservatism
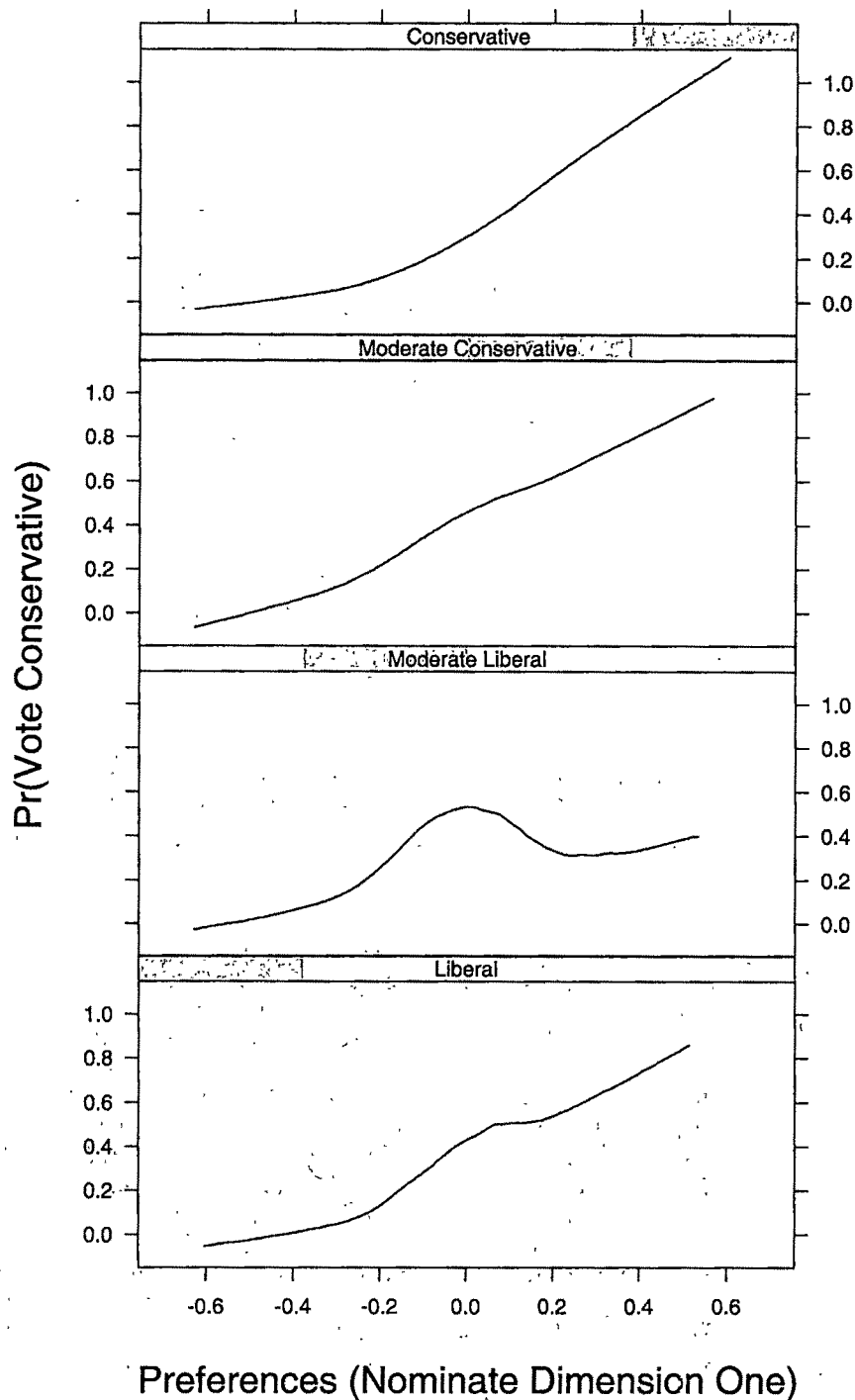
Note: The 25th, 50th, and 75th percentiles for judicial conservatism are 0.25, 0.28, and 0.63, respectively.

counts, I compute the Bayes factor between the hierarchical probit model and the pooled probit model, $B_{j,k} = 2366.07$, which is much greater than five. This is additional evidence that the hierarchical probit model fits the data better than the pooled probit model, which implies that my strategic account of House behavior not only comports with the hypotheses but also is a stronger statistical model than the non-

strategic one. For the House, I can confidently reject the null of nonstrategic congressional voting behavior.

THE SENATE

The House results are quite strong, but the question remains whether the same relationships hold in the Senate. The institutional differences between the

FIGURE 6. Posterior Density Summaries for the House Preference Measure, by Decision Context
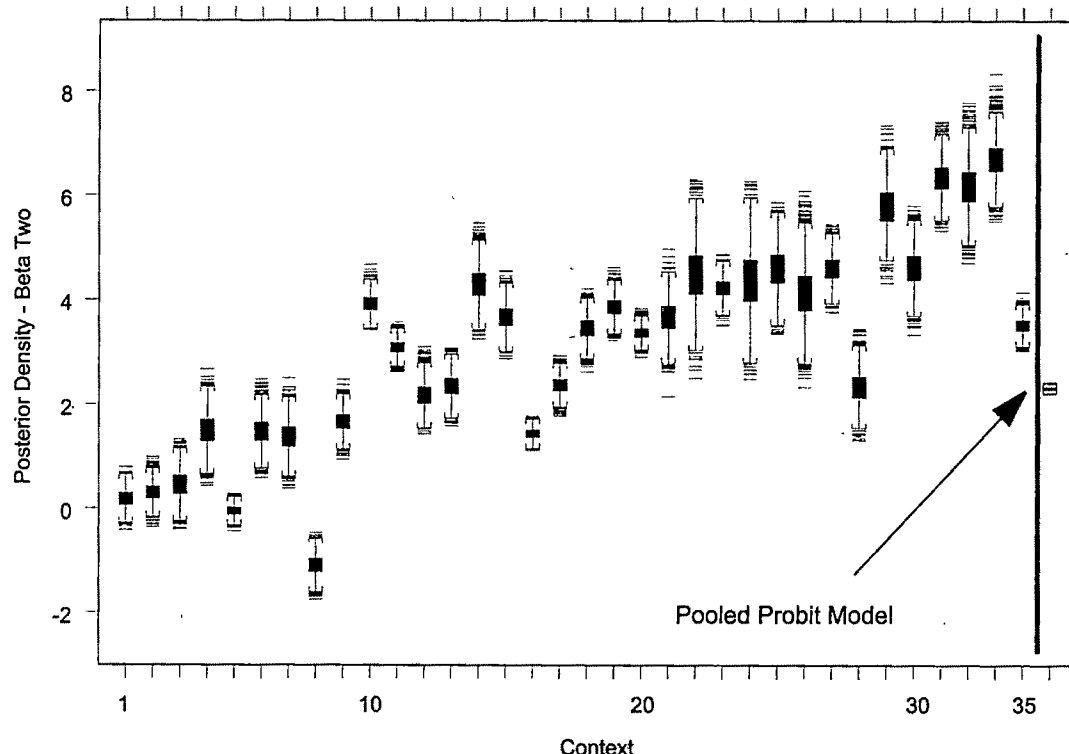
Note: The posterior density summaries are for the House hierarchical probit model. For each context, the central 50% of the distribution is summarized by the box, and the central 95% of the distribution by the upper and lower brackets. Outlying values outside the central 95% of the density are denoted by horizontal lines.

chambers might lead to a different reaction to the separation of powers. The theory, however, indicates that both should respond similarly.

The exploratory data analysis for the Senate is reminiscent of that for the House, so I only report results from the statistical analysis. I summarize the posterior density for the pooled probit model in the first column of Table 2. Just as with the House data, the posterior mean is positive (1.42), and the posterior standard deviation is small (.04). I report estimates for the interaction probit model and the hierarchical probit model in the last two columns of Table 2. Again, the Bayes factor between the two strategic models is greater than five ($B_{j,k}$ = 960.33). This, along with the statistically significant estimates of the error parameters, suggests that the hierarchical model is appropriate. In this case, relying on the interaction model would yield incorrect conclusions about the importance of the presidency. I again rely on the results from the hierarchical model for interpretation. These Senate results bear strong similarity to the House results. Hypothesis 1 is supported in this model. The posterior mean of α-House is −1.44, with a posterior standard deviation of 0.97. This is somewhat large, but 92.6% of the posterior density sample lies below zero, which is marginally significant support for the first hypothesis. Just as with the House, the presidency coefficient does not achieve significance in this multivariate setting. The

α coefficient on the Supreme Court measure, however, is strongly significant and positive (3.55), with a small standard deviation (.71). This demonstrates that the Supreme Court significantly constrains the actions of senators, as predicted in hypothesis 3.

To compare further the strategic and nonstrategic models, I construct in Figure 7 posterior density boxplots for the coefficients that measure the preference/behavior relationship in the Senate. Just as with the House data, it is clear that they are not constant across decision contexts. Decision contexts 8 and 9 provide a striking comparison. From one context to the next, there was turnover in the White House (Lyndon Johnson, the most liberal president of the period, took office), and another liberal justice was added to the Supreme Court (Byron White). When one compares the boxplots from the strategic model to the single boxplot from the nonstrategic (probit) model on the far right-hand side, it is clear that senators behave differently in various decision contexts. Not only the means vary, but also the variance of the distributions. The final piece of evidence on the Senate side is the Bayes factor between the two models. Here $B_{j,k}$ = 1091.10, which implies that for the Senate data the hierarchical probit model replicates the data-generating mechanism far better than the probit model. Senators, too, strategically respond to the separation of powers.

TABLE 2. Posterior Density Summaries for Senate Models

Variable	Pooled Probit Model			Interaction Probit Model			Hierarchical Probit Model		
	Post Mean	Post Median	Post StD	Post Mean	Post Median	Post StD	Post Mean	Post Median	Post StD
β_1 – Constant	−0.297	−0.297	0.012						
β_2 – Preference	1.424	1.425	0.038						
α_1 – Constant				−0.302	−0.302	0.012	−0.552	−0.556	0.212
α_2 – Preference constant				0.483	0.483	0.093	0.431	0.423	0.399
α_3 – Preference × Senate				−3.185	−3.181	0.738	−1.441	−1.432	0.975
α_4 – Preference × president				−0.708	−0.709	0.215	0.452	0.453	0.657
α_5 – Preference × judiciary				3.487	3.488	0.281	3.551	3.547	0.706
Ω_{11} – Error							0.835	0.796	0.247
Ω_{12} – Error							0.052	0.051	0.239
Ω_{22} – Error							1.542	1.464	0.466
Ln(marginal likelihood)	−7,243.48			−7,112.71			−6,152.38		
Burn-in iterations	500			500			500		
Gibbs iterations	5,000			5,000			5,000		
Contexts	30			30			30		
n	12,198			12,198			12,198		

Note: Uninformative prior distributions are used for all parameters. *Post Mean* denotes the posterior mean, *Post Median* the posterior median, and *Post StD* the posterior standard deviation. The models are estimated using Markov chain Monte Carlo (MCMC). See the Appendix for a discussion of estimation issues.

FIGURE 7. Posterior Density Summaries for the Senate Preference Measure, by Decision Context

Pooled Probit Model
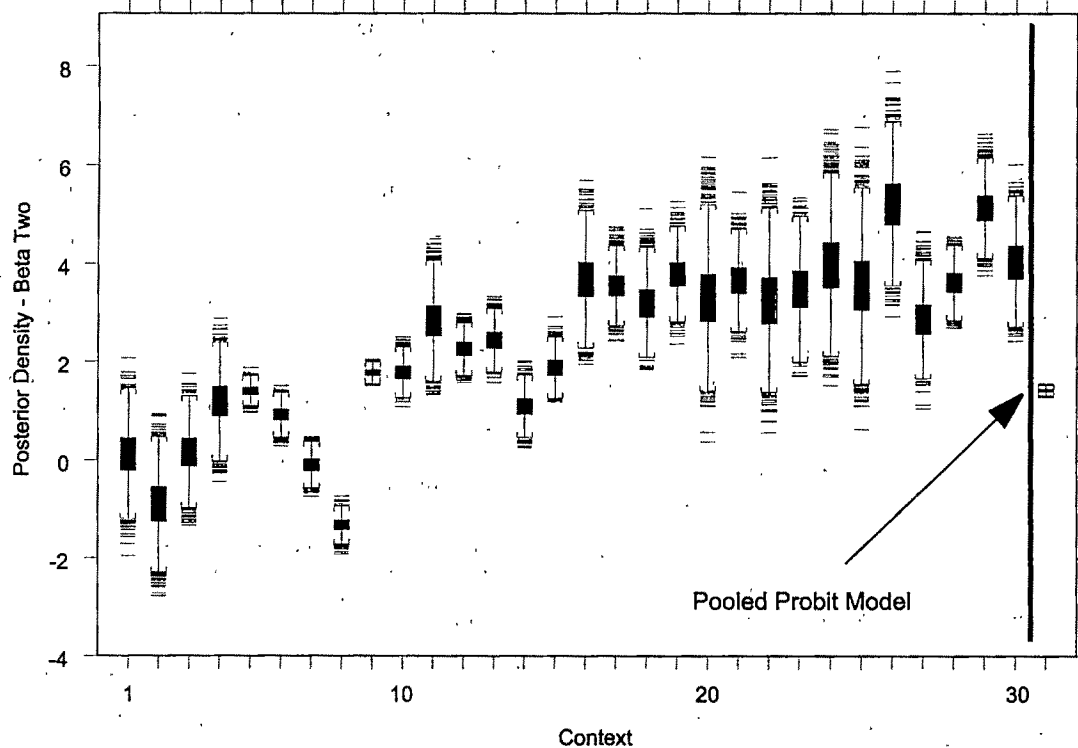
Note: The posterior density summaries are for the Senate hierarchical probit model. For each context, the central 50% of the distribution is summarized by the box, and the central 95% of the distribution by the upper and lower brackets. Outlying values outside the central 95% of the density are denoted by horizontal lines.

CONCLUSION

I began with two questions: Does the separation of powers influence congressional decision makers? Are members of Congress motivated by credit-claiming concerns when they cast roll call votes? The results of my analysis demonstrate that the separation of powers constrains the decisions that members of Congress make, and tangible policy outcomes are thus important to achieving congressional goals. The evidence suggests that some members of Congress use their roll call votes to claim credit for policies obtained in the separation of powers system.

One interesting finding, in both the House and Senate, is that the president does not seem to constrain congressional behavior. Why? It is clear that congressional agenda setters take the president into account when making proposals on the floor (Mouw and MacKuen 1992). In addition, the White House is quite involved with congressional leaders and committee staff when legislation is being crafted. Thus, the presidential effect on congressional behavior may occur earlier in the policy process, which would explain why no constraint was manifest in the data.

The major finding of this research is that members of Congress take into account the separation of powers when casting roll call votes. The evidence allows me to reject the null that members of Congress exclusively take positions when casting roll call votes. These findings are consistent with both position taking and credit claiming. Some members use the roll call to set their policy declarations in stone (by always casting sincere votes), and some pursue policy outcomes in the separation of powers system (by voting strategically, which sometimes may be observably sophisticated). The question remains as to when concerns about separation of powers are paramount, which I leave for future research.

Denzau, Riker, and Shepsle (1985) argue that it is very difficult for incumbents to justify sophisticated roll call votes to their constituents. Even so, these findings demonstrate strategic behavior that can manifest itself in sophisticated voting. Indeed, for a multitude of reasons, members of Congress are concerned about not only casting the right vote but also obtaining the best policy outcome. These results are also important when viewed in terms of the policy dimension theory and the ideological model, both of which assert that members of Congress always vote sincerely. When we recognize that policy is the result of interactions among the three branches of government, it becomes clear that the assumption of sincere congressional behavior is suspect and, as the results demonstrate, inappropriate. When we construct explanations of behavior and voting in Congress, it is important to model explicitly— theoretically and empirically—the institutional rules that structure the interactions.

APPENDIX: ESTIMATION AND SUPPLEMENTARY RESULTS

My hierarchical probit model is similar to other models used successfully in political science research. Western (1998)

makes a compelling case for hierarchical models when there is "behavioral heterogeneity." He argues that they are particularly useful in the study of comparative politics, when causal complexity makes traditional models inappropriate. This is similar to the idea of fractional pooling (Bartels 1996). Bayesian hierarchical models are the common ground between the extreme of pooling data across contexts or estimating models for each context, and they allow for inference about individual behavior as well as the causes of heterogeneity across contexts. The hierarchical model employed here is a nonlinear variant of the "general multilevel model" (see Jones and Steenberger 1997 for an introduction). Because the model is hierarchical, estimation is not straightforward. One could substitute equation 2 into equation 1, which would yield

$$z_{i,k} = x'_{i,k}W_k\alpha + x'_{i,k}\nu_k + \varepsilon_{i,k}$$

$$\nu_k \sim N_p(0, \Omega) \qquad \varepsilon_{i,k} \sim N(0, 1).$$

This is equivalent to a latent utility specification for a probit model with fixed and random effects. As noted in the text, if we could assume that $\nu_k = 0$ for all k, this would reduce to a probit model with many interactive terms.[20]

From a frequentist perspective, estimation of this model is quite difficult. For a continuous response variable (a simpler case), Jones and Steenbergen (1997) demonstrate the need to use generalized least squares (GLS) to estimate the first-level parameters, maximum likelihood estimation (MLE) to estimate the variance components, and empirical Bayes methods to estimate the second-level parameters. These problems are compounded when one moves to a dichotomous response variable. First, there are problems when estimating fixed effects with a small number of contexts (Greene 1997). In addition, Rodríguez and Goldman (1995) assess frequentist estimation techniques for multilevel models with binary outcomes (the case here). Given a set of Monte Carlo experiments, as well as the analysis of health care use in Guatemala, they demonstrate that the random effects in binary response models cannot be estimated with "acceptable levels of bias and precision" when the number of contexts is modest (p. 87). The alternative they suggest for these models is to adopt a Bayesian estimation strategy and estimate the model using the Gibbs sampling algorithm (p. 87). This is consistent with the theoretical result that hierarchical Bayes dominates fully pooled and completely separated models on a mean square error (MSE) basis (Efron and Morris 1973).

By including prior probability distributions for the hyperparameters α and Ω, one can use the Gibbs sampling algorithm to simulate directly from the posterior distribution $f(\{\beta_k\}\alpha, \Omega|y)$. In the analysis presented above, I assume Normal independent priors with mean zero and variance of 100 for the β parameters in the pooled probit model and the α parameters in the interaction and hierarchical probit models. For the variance parameters Ω, I employ a Wishart prior with large variance. As demonstrated in tables 6 and 7

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[20] The independence assumptions for the hierarchical model derive from one fundamental notion: Given a realization of the separation of powers game and a bliss point, all members of Congress behave identically. In other words, all the relevant information about congressional behavior is included in the model. This implies independence within clusters ($\varepsilon_{i,k}$ i.i.d. Normal), conditional on the hyperparameters α and Ω. At the second level of the hierarchy, the assumption is that, knowing the parameters of the separation of powers game, all members of Congress behave the same, and thus their behavior can be modeled as drawn from a common distribution (β_k i.i.d. multivariate Normal). The assumption that the errors at the first level of the hierarchy and the second level of the hierarchy are independent is a necessary modeling assumption.

TABLE A-1. Posterior Density Summaries for House and Senate Hierarchical Probit Models (*A* < *Q*)

Variable	House Hierarchical Probit Model			Senate Hierarchical Probit Model		
	Post Mean	Post Median	Post StD	Post Mean	Post Median	Post StD
α_1 − Constant	−0.711	−0.711	0.279	−0.845	−0.850	0.237
α_2 − Preference constant	0.402	0.401	0.378	0.629	0.629	0.435
α_3 − Preference × Senate	−1.872	−1.846	0.961	−1.431	−1.438	1.013
α_4 − Preference × president	0.445	0.429	0.597	0.066	0.048	0.701
α_5 − Preference × judiciary	4.797	4.806	0.663	3.641	3.642	0.732
Ω_{11} − Error	0.453	0.419	0.176	0.777	0.721	0.263
Ω_{12} − Error	−0.122	−0.110	0.189	−0.069	−0.055	0.260
Ω_{22} − Error	2.014	1.931	0.556	1.625	1.542	0.523
Ln(marginal likelihood)	−9,861.60			−1,472.51		
Burn-in iterations	500			500		
Gibbs iterations	5,000			5,000		
Contexts	31			26		
n	21,927			3,902		

Note: Uninformative prior distributions are used for all parameters. *Post Mean* denotes the posterior mean, *Post Median* the posterior median, and *Post StD* the posterior standard deviation. The models are estimated using Markov chain Monte Carlo (MCMC). See the Appendix for a discussion of estimation issues. The full data set contains all civil rights roll calls. For some votes, however, the theoretical assumption that the alternative is to the left of the status quo (*A* < *Q*) does not hold. The results presented here are for the subset of the full data set when members are voting for the liberal alternative. This eliminates three Senate and four House contexts from the analysis. The substantive results are robust to this restriction.

at the replication website, the results are robust to other prior specifications. This is expected, given the large sample size.

The specific approach employed to estimate the model is a type of Markov chain Monte Carlo (MCMC) estimation algorithm called the Gibbs sampler (see Gelman et al. 1995; Jackman 2000). This strategy allows one to draw inferences about all parameters in the model conditioned on the data, even with a small number of decision contexts. In practice, one uses diffuse priors, which in turn do not contribute substantively to the analysis (Jackman 2000; Western 1998). Given conjugate priors, the full conditional distributions take standard forms (Albert and Chib 1993; Lindley and Smith 1972): $\{\beta_k\}|y, \{z_{i,k}\}, \alpha, \Omega$ is multivariate Normal for all k, $\{z_{i,k}\}|y, \{\beta_k\}, \alpha, \Omega$ are truncated univariate Normal for all i and k, $\alpha|y, \{z_{i,k}\}, \{\beta_k\}, \Omega$ is multivariate Normal, and $\Omega^{-1}|y, \{z_{i,k}\}, \{\beta_k\}, \alpha$ is Wishart.

As suggested by an anonymous reviewer, biases may enter the analysis by including data for which the assumption that the alternative is left of the status quo $A < Q$ does not hold. This coding convention was employed to maximize the size of the sample, particularly the number of decision contexts. To test the robustness of the results, I reestimated the models after purging the cases in which this condition does not hold. The results for hierarchical probit models for both the House and Senate are reported in Table A-1. Note that the substantive conclusions reached in the article do not change under this specification.

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Political Ignorance and Collective Policy Preferences

MARTIN GILENS   *University of California, Los Angeles*

*I*n contrast with the expectations of many analysts, I find that raw policy-specific facts, such as the direction of change in the crime rate or the amount of the federal budget devoted to foreign aid, have a significant influence on the public's political judgments. Using both traditional survey methods and survey-based randomized experiments, I show that ignorance of policy-specific information leads many Americans to hold political views different from those they would hold otherwise. I also show that the effect of policy-specific information is not adequately captured by the measures of general political knowledge used in previous research. Finally, I show that the effect of policy-specific ignorance is greatest for Americans with the highest levels of political knowledge. Rather than serve to dilute the influence of new information, general knowledge (and the cognitive capacities it reflects) appears to facilitate the incorporation of new policy-specific information into political judgments.

The American public's ignorance of political matters is well documented, but the consequences for democracy are far less clear. For some, a disengaged and ill-informed citizenry signifies a failure of democracy. But for others, the practical question is whether political judgments would be any different if citizens were better informed. If a "rationally ignorant" public can nevertheless manage to approximate the "enlightened preferences" that a fully educated and engaged citizenry would hold, then perhaps democratic government and political ignorance can coexist.

A growing body of research assesses the extent to which Americans' political judgments would differ if the public were well informed about politics. The most straightforward way to address this issue is to inform a representative group of citizens about some set of political matters and see whether their judgments change as a result. This is the approach taken by James Fishkin and his colleagues in a series of "deliberative polls" that bring together a large number of randomly selected individuals and provide them with a wealth of information about specific political issues (Fishkin 1997; Luskin and Fishkin 1998). This strategy has many advantages but is limited by its great expense and by the unusual circumstances in which information is acquired. In addition, deliberative polls are explicitly designed to assess the effects of both information and deliberation on citizens' policy preferences. We cannot judge how much of the change revealed by a deliberative poll should be attributed to new information of the sort that might be acquired through the media and how much is due to the process of collective deliberation.

A second approach uses statistical models rather than experimental interventions to compare the political preferences of more and less informed Americans.

Bartels (1996), Delli Carpini and Keeter (1996), and Althaus (1998) all use this method to impute hypothetical "fully informed" preferences to less informed respondents while taking into account a range of demographic controls. This technique adjusts each respondent's political preferences to match the predicted preferences of a respondent who shares the same set of demographic characteristics but possesses the highest level of political information.[1] The importance of political ignorance is revealed by comparing the sample's observed preferences with the imputed preferences if all respondents were "fully informed."

This article draws primarily on the latter approach to the study of political ignorance and expands upon it in three ways. First, I note that most of the political facts in typical information scales are unlikely to contribute directly to such judgments as vote choice or policy preferences. Although knowing which office Ted Kennedy holds or how many justices sit on the Supreme Court does distinguish the politically knowledgeable from those who are less informed, and respondents who score high in such knowledge do express political preferences different from those of otherwise similar respondents who score low, this knowledge per se is unlikely to influence political choices or preferences. Instead, as Zaller (1992) suggests, measures of general political knowledge capture an amalgam of information, interest, engagement, and cognitive capacity for understanding the political world. It is some unknown combination of these elements that leads "fully informed" respondents to express political preferences different from those of their less informed peers. (Following the convention of previous research in this paradigm, I will use "fully informed" to refer to the highest level of general political information, as reflected by the relevant survey measure of general information.)

Although the "opacity" of general information mea-

[1] The "highest level of political information" can reflect either an existing category of respondents or a hypothetical respondent at the extreme end of the underlying information distribution. Delli Carpini and Keeter (1996) and Althaus (1998) take the former approach. Bartels (1996) takes the latter, using a score of 1.0 to represent the highest level of political information based on a five-point measure on which actual scores range from .05 to .95.

sures has been recognized by researchers, these measures are usually thought to be related to specific political judgments because those who score high are more likely to possess the specific information that may contribute directly to particular political judgments (Althaus 1998, 547; Delli Carpini and Keeter 1996, 223; see also Downs 1957, 79–80). But as Kuklinski and others (1998) point out, even Americans who are politically well informed in general (including those identified as fully informed by the criteria of previous research) may well be ignorant of highly relevant policy-specific knowledge. For example, in 1998, only 28% of Americans with the highest level of general political knowledge knew that the crime rate was falling, despite seven consecutive years of declines (see analyses below). I will assess the effect of "policy-specific ignorance" on the policy preferences of those who are deemed fully informed by the criteria of prior research. In other words, I ask what effect policy-specific ignorance has on the political preferences of fully informed respondents.

The second way in which I expand on the literature is to supplement the calculation of imputed preferences with the experimental manipulation of political information. Combining elements of the two different approaches outlined above, I compare the policy preferences expressed by respondents who were randomly chosen to receive specific political information to the preferences of a control group. The experimental design affirms that the estimated effects of political information on policy preferences are causal and not spurious.

Finally, I ask how the effect of policy-specific information differs for respondents with different levels of general political knowledge. Two alternatives have been suggested. On the one hand, general political knowledge (and its correlates, such as political interest and cognitive sophistication) may enhance individuals' ability and motivation to respond to new policy-specific information (Delli Carpini and Keeter 1996). On the other hand, general political knowledge may provide a resource to resist the influence of new policy-specific information (Zaller 1992). And if both forces are at work we may find either a curvilinear relationship between general political knowledge and the effect of policy-specific information or no apparent relationship at all.

My analyses lead to three conclusions. First, studies of political information based on general knowledge scales offer a useful but incomplete account of the effect of political ignorance. The limits of this approach arise from the fact that many people who are fully informed in terms of general political knowledge are nonetheless ignorant of policy-specific information that would alter their political judgments. Second, policy-specific ignorance may well have a greater influence on political preferences than the lack of general knowledge as measured by political information scales. In five of the seven situations I examine, that is the case. Of necessity, I looked only at a limited number of examples, and two were chosen precisely because substantial policy-specific information effects were ex-

pected. Nevertheless, the results suggest that much of what separates actual political preferences from hypothetical "enlightened preferences" is due to ignorance of specific policy-relevant facts, not a lack of general political knowledge or the cognitive skills or orientations that measures of general political information reflect.

Third, policy-specific information has a stronger influence on respondents who display higher levels of general political knowledge. Rather than dilute the effect of new information, general knowledge (and the cognitive capacities it reflects) appears to facilitate its incorporation into political judgments.

DATA AND MEASURES

My analyses require three kinds of data from the same survey: a measure of general political knowledge, a measure of policy-specific information, and a measure of policy preference plausibly related to the specific information. (By "plausibly related" I mean only that the specific information may affect a respondent's policy preference, not that it should have an effect. I treat as an empirical question whether any policy-specific information X is related to political judgment Y, and I leave aside the normative question of whether judgment Y should be influenced by X.)

Policy-Specific Information

Survey questions that assess the level of general political information are relatively uncommon, and items that assess policy-specific information are rare indeed. I make use of two surveys, one of which happens to have the necessary elements and one of which I designed for this purpose. The first is the 1988 American National Election Study (NES), which includes a number of items about perceptions of change during the Reagan administration. I focus on three items: (1) whether "federal efforts to improve and protect the environment increased, decreased, or stayed about the same as they were in 1980"; (2) whether "compared to 1980, the federal budget deficit has gotten smaller, stayed about the same, or gotten larger"; and (3) whether "compared to 1980, the level of unemployment in the country has gotten better, stayed about the same, or gotten worse."

Responses to these questions, like the items used in general measures of political information, can be clearly classified as correct or incorrect. Federal efforts to improve the environment unquestionably declined between 1980 and 1988, whether assessed in terms of dollars spent, legislative changes, or environmental policy enforcement (e.g., Kraft 1990; Vig 1990). The federal deficit increased dramatically in both real and nominal terms over this same period (U.S. Bureau of the Census 1997). Unemployment declined from 7.5% in fall 1980 to 5.4% in fall 1988 (Bureau of Labor Statistics 1999).

To supplement the measures of policy-specific information available in existing surveys, I wrote two sets of questions for the 1998 Multi-Investigator Survey

(MIS), conducted by the Survey Research Center at the University of California at Berkeley. The MIS was a national random-digit telephone survey of English-speaking adults in the 48 contiguous states, supported by the National Science Foundation (SBR-9818742), and directed by Paul M. Sniderman and Henry Brady. It combined thirteen different projects designed by 20 social scientists from around the country. The 1,067 completed interviews were conducted between June 1998 and March 1999, and the response rate was 55.8%. In my analyses, these data are merged with another 105 cases from a national pilot sample.[2] The MIS data are weighted to take into account the number of eligible respondents and the number of voice-telephone lines per household and to match the population joint distributions of age, sex, race, and education.

The two policy-specific information items from the 1998 MIS relate to crime and foreign aid. The first asks: "Would you say there is more, less, or about the same amount of crime in the United States today as compared to 10 years ago?" The second reads: "Please give your best guess for this next question. For every dollar spent by the federal government in Washington, how much of each dollar do you think goes for foreign aid to help other countries?"

The correct answer to the crime question is fairly clear: FBI statistics show a decline of 18% in the overall crime rate between 1988 and 1998 (Federal Bureau of Investigation 1999). Respondents who answered that there is less crime than ten years ago are considered to be informed in this regard; those who responded that crime has increased, stayed about the same, or "don't know" are considered to lack this information.

More discretion is involved in defining a correct answer to the question about foreign aid. In 1998, foreign aid of all kinds amounted to eight-tenths of one percent of the federal budget (U.S. Bureau of the Census 1998, 339, 796). It would be unrealistic to expect respondents to know the exact percentage, so as a reasonable approximation, I considered responses that foreign aid accounts for 5% or less of the federal budget as "correct."[3]

Randomized Provision of Policy-Specific Information

My objective in designing randomized experiments for policy-specific information was to mimic the kind of information that a person might acquire in everyday life. Randomly chosen respondents were given information about crime or foreign aid in the guise of asking

whether they had heard about particular news stories. To make the treatment and control conditions as parallel as possible, both groups were asked about news stories on the topic in question, but only the treatment group was provided with the relevant information. The treatment version of these two questions read as follows.

> Our first questions are about two stories that have been in the news lately. The first story is: the crime rate in America has gone down for the seventh year in a row and is now lower than at any time since 1974. Have you heard about this story?
>
> The second story is about a new report that was just released about American foreign aid to help other countries. It said that the amount of money we spend for foreign aid has been going down and now makes up less than one cent of every dollar that the federal government spends. Have you heard about this story?

Respondents in the control group were asked the following questions.

> Our first questions are about two stories that have been in the news lately. The first story is about the release of a new government report about the crime rate in America. Have you heard about this story?
>
> The second story is about a new report that was just released about American foreign aid to help other countries. Have you heard about this story?

The crime and foreign aid stories were randomized independently, so that half of those who were provided with information about the crime rate were also given information about foreign aid and vice versa. All 1,172 MIS respondents received one of the two versions of the foreign aid question. The crime question, however, had three additional versions that were designed for other purposes and are excluded here. Consequently, fewer cases are available for analysis of attitudes toward crime than of attitudes toward foreign aid.

Policy Preference Measures

I associate a particular political judgment (or judgments) with each of the five policy-specific information items described above. I make no normative claim that these particular bits of information should influence respondents' policy preferences; I simply seek to determine whether they do so. Any particular social or political fact may have multiple consequences for a given individual's policy preferences. For example, knowing that crime has declined may influence one's views on a range of related policy matters, from prison construction, to court procedures, to police conduct. Whenever possible, I assess the influence of policy-specific information on more than one policy preference or political judgment.

I pair the measure of respondents' information about changes in the unemployment rate with preferences for federal government spending to help the unemployed and with respondents' attitudes toward "placing new limits on foreign imports in order to protect American jobs." My expectation is that knowledge of a decline in unemployment will, ceteris paribus,

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[2] In terms of sample frame, interviewing procedures, and sample weights, the pilot is identical to the MIS survey. After the pilot cases were collected, changes were made to some of the MIS items. Because none of the items used in my analyses were involved, I was able to combine the two samples.

[3] Sensitivity analyses showed that the particular cutoff for perceptions of foreign aid spending had little effect on the results. Similarly, combining respondents who answered that crime remained "about the same" as ten years ago with those who indicated it had fallen would not change the substantive conclusions of this research.

lessen the desire to increase spending for the unemployed and boost opposition to import limits. Similarly, I anticipate that knowledge of deficit growth will be associated with a greater willingness to pay higher taxes in order to shrink the deficit. I pair knowledge of a decline in federal efforts to protect the environment with a preference for increasing federal spending in that area and with the perception that George Bush, then vice president, was less concerned about the environment than Michael Dukakis, his opponent in the 1988 presidential election. The MIS crime item is paired with a measure of respondents' support for government spending on prison construction. Finally, perceptions of foreign aid spending are paired with support for federal spending for foreign aid.[4] (The question wordings and response distributions for the MIS items are provided in the Appendix; see Miller [1989] for details of the NES questions.)

General Information Measures

To measure general political information from the NES, I use the 16-item additive index developed by Delli Carpini and Keeter (1993, 1996) and employed by Althaus (1998). Fifteen of the items are binary measures, scored 1 for a correct answer and 0 for an incorrect or no answer. These questions asked about the offices held by various political figures, the majority party in the House and Senate, the relative ideological positions of Bush and Dukakis, the relative ideological positions of the Republican and Democratic parties, and the relative positions of the two parties on a series of issues (see Althaus 1998 for details). The final item was a five-point interviewer rating of the respondents' general level of information about politics and public affairs. The raw scale scores ranged from 1 (lowest interviewer rating and no correct answers) to 20. To ease interpretation and compatibility with the general information scale from the MIS, I converted the raw scores into percentiles based on the proportion of respondents at each value of the scale.[5]

The general information measure from the MIS is a seven-item additive index that consists of such questions as which political party has the most members in the House of Representatives and how large a majority is needed to override a presidential veto (see the Appendix). Among these seven are the five items recommended by Delli Carpini and Keeter (1993, 1996). Like the NES scale, raw scores were converted to percentiles based on the proportion of respondents

with each scale score. Alpha reliability for the NES and MIS scales are .88 and .68, respectively.

Control Variables

My analysis of the NES uses the same set of 25 control variables described in Althaus (1988), which covers demographic characteristics and partisan identification. The MIS analysis examines the 17 of these variables available on that survey, coded as similarly as possible to the NES measures (see the Appendix for details).

GENERAL AND POLICY-SPECIFIC INFORMATION

Some scholars argue that Americans tend to be political specialists, knowledgeable about the few issues in which they have a particular concern (Bennett 1990; Iyengar 1990; Krosnick et al. 1993). If so, we cannot hope to assess the importance of political ignorance without measures of policy-specific knowledge. Others believe that citizens tend to be generalists and that measures of policy-specific information add only modestly to our understanding of political knowledge (Delli Carpini and Keeter 1996; Neuman 1986; Smith 1989; Zaller 1986). I will not attempt to resolve this debate in any broad sense, but in most of the cases examined I will show that policy-specific facts have a substantial influence on policy preferences, even for those who are fully informed in terms of general political knowledge.

The first column of Table 1 shows the correlation between each policy-specific information measure and the general knowledge scale from the NES and MIS surveys. (The analyses in this table use only respondents assigned to the control conditions of the MIS questions; i.e., those not provided with information about crime or foreign aid). The correlations (Eta's) range from a fairly weak .19 (for knowledge about the crime rate) to a more robust but still modest .39 (for knowledge about the deficit). More important than the relationship between these two kinds of information is the extent of policy-specific ignorance, especially among highly informed respondents. The second column of Table 1 shows the proportion of all respondents who correctly answered each of the policy-specific information questions, and the third column shows the same proportion for respondents at the high end of the general knowledge scale (scores at or above the 87th percentile of general information for the NES and the 90th percentile for the MIS; the different cut points are necessitated by the different distribution of respondents across the values of general information in the two surveys).

Knowledge of the five policy-specific facts varied dramatically, both for the sample as a whole and for highly informed respondents.[6] Virtually all the highly

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[4] For most of the spending preference questions I assessed support by dividing respondents who wanted an increase from those who wanted to maintain or decrease spending. Because so few respondents indicated a desire for more foreign aid, I present estimates of the proportion who preferred cuts as compared with those who preferred to maintain or increase foreign aid.

[5] For example, 3.6% of respondents had the lowest value on the general information scale. I assumed that this group of respondents was distributed evenly across the bottom 3.6 percentiles of the underlying dimension of general information. I therefore assigned these respondents a score of .018, representing the midpoint of this set of percentiles (on a 0-to-1 scale). The next highest value on the information scale contained 3.2% of the sample, and these respondents were assigned the midpoint (.052) of their set of percentiles (which ranged from 3.6 to 6.8).

[6] For the sample as a whole, the levels of policy-specific information reported in Table 1 are consistent with earlier results (Delli Carpini and Keeter 1996). For example, in the 1988 NES data, 75% were aware that the deficit had increased under Reagan, and 53% knew that unemployment had declined. In comparison, Delli Carpini and Keeter (p. 80) report a 1985 survey in which 83% knew the deficit had risen since 1981 and a 1984 survey in which 48% knew the

TABLE 1. General and Policy-Specific Political Information

Policy-Specific Information	Correlation Between General and Policy-Specific Information[a]	Percentage of All Respondents with Policy-Specific Information	Percentage of Highly Informed Respondents with Policy-Specific Information[b]
1988 NES			
Deficit increased under Reagan	.39***	75.3	93.8
Unemployment declined under Reagan	.25***	53.6	80.0
Environmental efforts declined under Reagan	.36***	21.3	52.2
1998 MIS			
Foreign aid is 5% or less of federal spending	.23***	17.2	36.9
Crime rate declined over past decade	.19	12.2	28.0

Note: General information measures consist of additive 16-item and 7-item scales for the NES and MIS surveys, respectively. (See Appendix for details.) N's: deficit, 1,756; unemployment, 1,758; environment, 1,761; foreign aid, 586; crime, 208. ***$p < .001$.
[a]Eta with policy-specific information dependent; significance tests are likelihood ratio chi-square..
[b]Includes respondents scoring at or above the 87th percentile of general information for the NES and the 90th percentile for the MIS. (Different cut points are necessitated by the different distribution of respondents across general information categories in the two surveys.)

informed knew that the federal deficit increased during the Reagan administration, and 80% knew that unemployment declined, but only half were aware that environmental efforts declined, fewer than two in five estimated that foreign aid amounted to 5% or less of federal spending, and only 28% knew that crime had declined over the past decade.

Those who are generally more knowledgeable about politics are more likely to know each of the policy-specific facts examined. Nevertheless, the level of specific knowledge varies greatly from fact to fact, and in several cases ignorance of specific information appears widespread, even among those who are best informed according to general knowledge scales. As a consequence, it is possible that even Americans who are fully informed in terms of general political knowledge might hold very different policy preferences if they were also informed about the specifics relevant to particular political judgments.

THE EFFECT OF INFORMATION ON POLITICAL JUDGMENTS

The first stage of my analysis replicates the Althaus (1998) model of information effects and then extends it to include policy-specific knowledge. The model con-

current unemployment rate. Similarly, the 1998 MIS found that only 17% estimated within 5 percentage points the proportion of the federal budget devoted to foreign aid, and Delli Carpini and Keeter (1996, 93) report that 24% correctly estimated the federal education budget within that same range. The public's specific knowledge depends greatly on the salience of certain facts to current issues and debates. Nevertheless, it appears that at least over short periods there is considerable stability in this level of knowledge for any given type of information.

sists of a logistic regression that predicts policy preferences on the basis of political information, demographic and partisan control variables, and interactions between the measures of information and the controls. When both general and policy-specific knowledge are included in the model, an interaction term between these two forms of political information is also included. As in the models developed by Bartels (1996) and Delli Carpini and Keeter (1996), the interaction terms in these analyses allow for the differential effect of information for respondents with different demographic and partisan characteristics. In addition, the interaction term between general and policy-specific information allows for the possibility that general knowledge either facilitates or inhibits the influence of policy-specific information on political judgments.

To assess the effect of information on policy preferences, two separate equations are estimated. The first includes only general political knowledge among the predictors, and the second adds policy-specific information. The model for general information takes the form

$$\text{prob}(Y_i = 1) = \alpha + \beta_1 G_i + \Sigma\beta_k D_{ik} + \Sigma\gamma_k(G_i D_{ik}) + e_i, \quad (1)$$

and the model for both general and policy-specific information takes the form

$$\text{prob}(Y_i = 1) = \alpha + \beta_1 G_i + \beta_2 P_i + \beta_3 G_i P_i + \Sigma\beta_k D_{ik}$$
$$+ \Sigma\gamma_k(G_i D_{ik}) + \Sigma\delta_k(P_i D_{ik}) + e_i, \quad (2)$$

where Y_i is respondent i's policy preference, G_i is respondent i's general political information score, P_i is respondent i's policy-specific information score, D_{ik} is respondent i's scores on a vector of k control variables, and e_i is the error term for the ith observation. By way of example,

TABLE 2.   Effect of General and Policy-Specific Ignorance on Political Judgments by Issue: Nonexperimental Analyses (NES)

Policy Information:	Deficit Increased	Unemployment Declined		Environmental Efforts Declined	
Political Judgment:	Unwilling to Pay More in Taxes to Reduce Deficit	Oppose Import Limits to Save Jobs	Increase Spending to Help Unemployed	Increase Spending for Environment	Bush Cares Less for the Environment than Dukakis
A. Observed	74.4 (1.1)	24.3 (1.1)	30.6 (1.0)	63.7 (1.1)	31.8 (1.3)
B. Full general information	56.6 (3.9)	36.8 (3.0)	30.7 (2.6)	71.7 (2.7)	43.5 (3.6)
C. Full general and policy-specific information	51.4 (4.2)	38.3 (4.6)	24.7 (3.8)	89.9 (2.7)	58.1 (5.6)
D. Effect of general political ignorance (B–A)	−17.8 (3.8)	12.5 (3.0)	0.1 (2.4)	8.0 (2.5)	11.7 (3.3)
E. Effect of policy-specific ignorance for the fully informed (C–B)	−5.2 (2.2)	1.5 (2.9)	−6.0 (3.1)	18.2 (2.8)	14.6 (5.1)
N	1,422	1,562	1,620	1,597	1,384

Source: 1988 National Election Study.
Note: Entries in rows A through C show the percentage of respondents expressing the political judgment indicated. Predicted proportions (rows B and C) are based on equations 1 and 2, respectively. Standard errors are in parentheses.

the results of equation 2 for the perception that Bush cared less about the environment than Dukakis are shown in Table A-1.

Following the procedures used by Bartels (1996), Delli Carpini and Keeter (1996), and Althaus (1998), I use the estimated coefficients from the above models to impute statistically hypothetical policy preferences for respondents with less than full information. This is done by attributing to each respondent her observed demographic and partisan characteristics but changing her information score to 1.0. For general information, this score represents the hypothetical fully informed respondent at the 100th percentile of political knowledge; for policy-specific information this score represents "correct" information. Finally, the imputed fully informed preferences of all respondents are aggregated to produce a simulated percentage favoring either side of each political judgment. With this approach, the effect of political ignorance is assessed by comparing the observed policy preferences of the sample with the simulated policy preferences of a hypothetical fully informed sample.

Rows A and B of Table 2 show the observed policy preferences and the fully informed preferences for the NES respondents (row D indicates the difference between the two). Consistent with Althaus's results, the influence of general information on policy preferences varies considerably from issue to issue. For example, observed and fully informed preferences for not raising taxes in order to decrease the budget deficit differ by almost 18 percentage points (74.4% versus 56.6%). Yet, the two categories are almost identical with regard to an increase in government spending to help the unemployed.

The directional effects of general political knowledge revealed in row D of Table 2 are also consistent with Althaus's results. As shown in Table 2, general political

information is associated with both a greater willingness to pay higher taxes for deficit reduction and greater support for environmental spending. Both are consistent with Althaus's (1998, 552) more general finding that "fully informed opinion" reflects a liberal tendency across an array of fiscal issues. Similarly, Althaus's finding that fully informed opinion is more conservative on issues pitting the free market against government control is consistent with the results in Table 2 that show an association between general political knowledge and greater opposition to import limits.

Next, I expand the Althaus (1998) model to include policy-specific information to determine what (if any) additional effect it might have. Row C of Table 2 shows the imputed policy preferences that result by attributing to the entire NES sample both full general information and the policy-specific information indicated; row E reports the difference between the imputed preferences of a sample that is fully informed in terms of general information and a sample that is fully informed in both general and policy-specific terms. Again, there is considerable variation from issue to issue. For example, policy-specific ignorance about unemployment has almost no effect on opposition to import limits. In contrast, ignorance about the decline in environmental efforts is associated with a deficit of 18.2 percentage points in support for environmental spending and of 14.6 percentage points in the perception that George Bush is less concerned about the environment than Michael Dukakis.

These analyses suggest that policy-specific ignorance can have a significant influence on Americans' political judgments, above and beyond the effect of general political information. But drawing causal inferences from quasi-experiments is often risky (e.g., Achen 1986), and it is possible that the apparent effect of

policy-specific information is partly or wholly spurious. Respondents who correctly answer the policy-specific questions differ in a host of ways from those who do not. And despite the long list of control variables used in these analyses, we cannot hope to identify and adequately measure all the characteristics that might distinguish these two groups.

For example, one way in which respondents who correctly and incorrectly answer these policy-specific questions may differ is in their attitude toward President Reagan. Because the policy-specific items on the NES refer to changes in conditions during Reagan's term, his supporters may be reluctant to "admit" facts that reflect poorly on him, such as the federal deficit increase and decline in environmental efforts. Control variables were used in an attempt to address such concerns, and measures of respondents' partisanship should help in this regard.

As a further assessment of whether "willful ignorance" on the part of Reagan supporters might bias the results, I reestimated some of the equations reported in Table 2, using respondents' reported presidential vote in 1984 as an additional control. (Two dummy variables were used to indicate a vote for Reagan or a vote for another presidential candidate; nonvoters served as the comparison group. As with all the other controls, interaction terms were constructed between each control variable and both general and policy-specific knowledge.) Reagan voters did not differ from those voting for Mondale or a minor party candidate in their perceptions of changes in the deficit (80% of Reagan voters and 83% of the other voters said the deficit had increased), but they did differ with regard to environmental efforts (17% of Reagan voters and 38% of the other voters perceived a decline). Consequently, I looked for the effect of "willful ignorance" only in the two analyses of environmental perceptions shown in Table 2.

The results (available from the author) are virtually identical with those shown in Table 2 that do not include 1984 vote choice as a control variable, which suggests that any bias of Reagan supporters on the environmental issue is captured by the other control variables in the model.[7]

Randomized Provision of Policy-Specific Information

In response to the uncertainties involved in drawing causal inferences from traditional survey data, researchers have turned increasingly to survey-based randomized experiments (Piazza, Sniderman, and Tetlock 1989; Sniderman and Grob 1996). Embedding randomized experiments within traditional surveys combines the causal power of randomized assignment with the large-scale and representative nature of the sample survey.

Of course, randomized experiments have shortcom-

ings as well. One concern in this case is that the apparent "change" in political judgments produced by new policy-specific information may reflect a momentary response to the experimental stimulus. This may occur if exposure to the information influences responses by "priming" one aspect of the issue, rather than by prompting a genuine reevaluation of the issue in light of new information (Iyengar and Kinder 1987; Nelson, Clawson, and Oxley 1997; Nelson and Kinder 1996). For example, the crime rate is one consideration among many upon which respondents may focus in forming a preference toward prison spending. By informing some respondents about the crime rate, the interviewer may prime them to place more weight on that aspect. If so, their preferences might be altered not by new knowledge but by greater salience of something already known.

Priming effects cannot be completely ruled out as contributing to experimental effects reported below, but they are unlikely to play an important role. First, the questions were designed to provide both random groups with as similar an experience as possible except for the actual information provided. Thus, both the treatment and control groups were "primed" to think about the crime rate, even though information about it was restricted to the treatment group (see above for question wording). Second, concerns about priming do not apply to the cross-sectional analyses that compare respondents who already know the relevant policy-specific information with those who do not, and as Table 3 shows, the predicted effect of policy-specific information is quite similar using the experimental and cross-sectional approaches. Finally, to assess the broader category of "temporary effects," which might include priming, respondents' preferences on spending for prison construction and foreign aid were assessed twice during the MIS survey: at the beginning, in close proximity to the experimental stimuli (i.e., the news stories about crime and foreign aid), and toward the end of the 40-minute interview.[8]

As shown in row E of Table 3, providing information about the crime rate reduced support for prison spending by 18.9 percentage points using the first measure of policy preference. An otherwise identical analysis using the policy preference measure from the end of the interview showed a reduction in support of 15.4 points. The comparable figures for foreign aid spending are 16.6 points (Table 3) and 10.3 points. Thus, the information effect faded somewhat in both cases, but it persisted through the "distraction" of numerous questions on a range of political issues. The persistence of the information effect does not mean respondents' views were permanently changed by the information provided, but meaningful change need not be permanent. Indeed, in the real world also the influence of any given bit of policy-specific information is likely to fade over time unless it is reinforced through repetition.

[8] To reduce the awkwardness of asking respondents the same question twice, the second item on prison construction and foreign aid differed very slightly from the first (see the Appendix for wording).

TABLE 3. Effect of General and Policy-Specific Ignorance on Political Judgments by Issue: Experimental and Nonexperimental Analyses (MIS)

Information:	Foreign Aid Is 5% or Less of Federal Spending		Crime Has Decreased
Political Judgment:	Cut Spending for Foreign Aid (Nonexperimental)	Cut Spending for Foreign Aid (Experimental)	Increase Spending for Prison Construction (Experimental)
A. Observed	61.5 (2.5)	62.0 (1.8)	33.9 (2.5)
B. Full general information	60.3 (6.4)	58.6 (5.7)	45.8 (8.2)
C. Full general and policy-specific information	46.7 (14.8)	42.0 (5.1)	26.9 (6.4)
D. Effect of general political ignorance (B–A)	−1.2 (6.6)	−3.4 (5.3)	11.9 (8.1)
E. Effect of policy-specific ignorance for the "fully informed" (C–B)	−13.6 (15.4)	−16.6 (7.8)	−18.9 (9.7)
N	564	1,102	409

Source: 1998 Multi-Investigator Study.
Note: The first column reports the nonexperimental analysis of control group respondents only (i.e., those who were not provided with policy-specific information); the second and third columns report the experimental analysis of all respondents. Entries in rows A through C show the percentage of respondents expressing the political judgment indicated. The observed proportions (row A) reflect the preferences expressed by control group respondents. The predicted proportions (rows B and C) for the nonexperimental analyses are based on equations 1 and 2, respectively; the predicted proportions for the experimental analyses are based on equation 3. Standard errors are in parentheses.

A second shortcoming typical of experiments concerns external validity, or the extent to which they successfully mimic real world phenomena. In the MIS experiments, the phenomenon of interest is exposure to policy-specific information from the news. The experimental analogues ·are the questions about two stories that "have been in the news lately." Exposure to information in this way clearly differs from exposure to the same information in the real world. On the one hand, respondents may feel obliged to pay more attention to the survey interviewer than to the news media. On the other hand, respondents may doubt the interviewer's claim that these stories have been in the news, as well as the implicit claim that the information they contain is accurate.

In sum, neither the experimental provision of policy-specific information nor the nonexperimental comparison of respondents who already do or do not possess this information is without problems. But the consistent findings across these two approaches lend considerable confidence to the results.

As shown in the first two columns of Table 3, the MIS data were used to compare the nonexperimental and experimental approaches to information effects. The first column shows results from the identical nonexperimental model used above to analyze the NES data. (This analysis uses only respondents in the control condition of the MIS data: those who were asked for their perceptions of foreign aid but were not provided with correct information.) Based on this model, a fully informed sample would express nearly identical attitudes toward foreign aid (diminished by only 1.2 percentage points), but if the entire sample also possessed "correct" information (specifically, that foreign aid amounts to 5% or less of federal spending),

then opposition to foreign aid is predicted to fall by more than 13 additional percentage points.

The second column of Table 3 reports a parallel analysis of attitudes toward foreign aid. In this case, however, the effect of policy-specific knowledge is assessed by comparing preferences expressed by respondents in the treatment and control conditions. The same logistic equation is estimated as in equation 2, except that in place of a measure of respondents' observed policy-specific information (P), I use a variable (T) to indicate whether the respondent was randomly assigned to the treatment group (scored 1) or the control group (scored 0):

$$\text{prob}(Y_i = 1) = \alpha + \beta_1 G_i + \beta_2 T_i + \beta_3 G_i T_i + \Sigma \beta_k D_{ik}$$

$$+ \Sigma \gamma_k (G_i D_{ik}) + \Sigma \delta_k (T_i D_{ik}) + e_i. \quad (3)$$

Imputed preferences for a hypothetical sample with full general information are estimated by setting G to 1 and T to 0; preferences for a hypothetical sample with both full general information and specific knowledge about foreign aid are derived by setting both G and T to 1.[9] Like the nonexperimental analyses, this model allows for policy-specific information to· have differing effects for respondents with different demographic characteristics as well as different· levels of general political knowledge.

Row E of Table 3 shows that the estimated effect of

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[9] Note that setting $T = 0$ in equation 3 reflects the predicted policy preferences of a sample with the observed level of policy-specific information. In contrast, setting $P = 0$ in equation 2 would reflect the preferences of a sample in which all respondents were ignorant of the relevant policy-specific information. Consequently, the figures in row B of tables 2 and 3, and row A of Table 4, are estimated with $T = 0$ and P at its observed level.

telling respondents about foreign aid is quite similar to the estimated effect of this knowledge based on the nonexperimental model. The first column in row E indicates that opposition to foreign aid among fully informed respondents would be 13.6 percentage points lower if they were also aware that foreign aid accounts for 5% or less of federal spending. The second column of row E shows that actually giving this information to a random subsample of respondents leads to a predicted 16.6 percentage point decrease in opposition to foreign aid among those who are fully informed in general terms. Based on this one example, at least, the estimates derived from the nonexperimental model appear to be reasonable approximations of the causal influence of policy-specific information.

This same comparison between experimental and nonexperimental techniques cannot be made using the crime questions. First, the number of available cases is much smaller because most of the respondents were assigned to other experimental treatments not used in, these analyses. Second, only 12% of the respondents in the control group perceived a decline in crime over the past decade (Table 1). This represents only 25 cases, too few to produce a reliable estimate of the effect of this information.

The public's ignorance about the crime rate presents an obstacle to nonexperimental analysis, but there is no such obstacle to the experimental approach, since the 200 cases in the treatment group were told that crime had been falling. The results of this analysis are reported in the last column of Table 3. When informed that crime had declined, estimated support for prison construction among fully informed respondents fell by 18.9 percentage points.

The five specific examples of policy-relevant information and the seven political judgments examined in tables 2 and 3 are a small sample of all possible combinations of preferences and information. They were based on what happened to be available from existing surveys (in the case of the NES items) or were chosen because I expected to find the public misinformed about potentially influential policy facts. These results cannot, therefore, be taken as representative of some larger set of political judgments. Nevertheless, the findings are consistent enough to indicate that (1) ignorance of policy-specific facts is extremely important in creating a gap between the political preferences that Americans express and the preferences they would express if they were well informed about the issues at hand, and (2) measures of general political knowledge do not adequately capture the importance of policy-specific ignorance.

For the seven political judgments I examined, the difference between observed and fully informed preferences averaged 9.3 percentage points (roughly compatible with Althaus's [1998] average of 7.1 percentage points for the 45 issues he analyzed). But for respondents who are fully informed in terms of general political knowledge, I found an average difference of 11.6 percentage points between those who possess policy-specific information and those who lack it. In five of the seven cases, lack of general political knowl-

edge (and its cognitive correlates) has a smaller effect on political judgments than does ignorance of policy-specific information. Clearly, any assessment of the gap between expressed and "enlightened" preferences must take account of shortfalls in the public's knowledge of policy-specific facts as well as shortfalls in general political knowledge.

POLICY-SPECIFIC INFORMATION AND IGNORANCE: FOR WHOM DO THEY MATTER?

The analyses above reveal the importance of policy-specific ignorance for the political judgments made by fully informed respondents. Do these effects differ for people with different levels of general political knowledge?

One set of considerations suggests that "providing" policy-specific facts—either experimentally or by statistical imputation—should have less effect on the political judgments of respondents with higher levels of general political knowledge. First, respondents high in general knowledge are likely to have a larger stock of other information relevant to the political judgment at hand. Studies of learning and persuasion suggest that psychological judgments are based on a weighted average of available information. Consequently, each new fact becomes less influential as an individual's stock of existing information grows (e.g., Anderson 1981).[10] In addition, respondents with more general knowledge are more likely to already possess the particular policy-specific fact examined (Table 1).[11] For both reasons, policy-specific ignorance should be less important in explaining the views of those with high levels of general political knowledge.

But other considerations suggest that general knowledge should enhance rather than diminish the importance of policy-specific information. Respondents who score high in general knowledge possess greater cognitive capacity and a greater interest in politics (Delli Carpini and Keeter 1996), and they are likely to have more integrated belief systems (Converse 1964). These resources may provide the ability and motivation to incorporate new policy-specific information and reshape one's political judgments accordingly.[12] For ex-

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[10] In Zaller's (1992) model of political attitude formation, this phenomenon is labeled "inertial resistance." Note that the "dilution" of new information in a pool of existing considerations leads to a lower probability of attitude change, but it does not constitute "resistance" to the new information per se. For an earlier statement of this perspective, see Converse 1962.

[11] The greater likelihood that the politically knowledgeable will have encountered specific information is reflected in Zaller's (1992) discussion of "reception." It is important to note that if one is assessing the influence of information, a higher probability of reception is associated with a greater likelihood that the information will affect an individual's policy views. But if one is assessing the importance of ignorance, a higher probability of reception is associated with a lower likelihood that ignorance has "distorted" a given individual's policy views.

[12] It is important to distinguish between persuasive and informational communications. To the extent that a persuasive message is propaganda, we would expect the smallest change in policy preferences among the most sophisticated and informed individuals. To the

ample, Rhee and Capella (1997) and Capella and Jamieson (1997) found that people who scored high in political sophistication learned more from news stories on health care and a mayoral election. Fiske, Lau, and Smith (1990) similarly found that people with higher levels of political knowledge are more efficient and effective in processing information from newspaper articles about local political issues.

The relative strength of these two sets of considerations will determine the importance of policy-specific information across levels of general political information. If general knowledge and its correlates prompt resistence to new information, we would expect policy-specific information to have the strongest influence on the political judgments of the least knowledgeable respondents. But if cognitive ability and motivation dominate, then we would expect policy-specific information to have the strongest effect among those who score highest in general political knowledge. And if these two sets of offsetting factors are both important, then we might find either similar importance across levels of general knowledge or perhaps a curvilinear effect, that is, policy-specific ignorance might have its greatest influence among those with moderate levels of political knowledge (because a very high level of general political knowledge leads to resistence and a very low level to indifference or incomprehension).

As a first step I conducted a series of tests for nonmonotonicity in the importance of policy-specific ignorance. Using dummy variables in place of the continuous measure of general political knowledge, I estimated the following equation:

$$prob(Y_i = 1) = \alpha + \beta_1 G1_i + \beta_2 G2_i + \beta_3 P_i$$
$$+ \beta_4 G1_i P_i + \beta_5 G2_i P_i + \Sigma\beta_k D_{ik} + \Sigma\gamma_k(G1_i D_{ik})$$
$$+ \Sigma\delta_k(G2_i D_{ik}) + \Sigma\epsilon_k(P_i D_{ik}) + e_i, \qquad (4)$$

where Y_i is respondent i's policy preference, $G1_i$ is respondent i's score on a dummy variable that indicates low general political information, $G2_i$ is respondent i's score on a dummy variable that indicates high general political information, P_i is respondent i's policy-specific information score, D_{ik} is respondent i's scores on a vector of k control variables, and e_i is the error term for the ith observation. The analogous model for the experimental data from the MIS substitutes the indicator of randomized assignment (T) for the measure of policy-specific knowledge (P), as was done in equation 3.

Lacking strong theoretical expectations for the specific shape of any nonmonotonic pattern that might be found, I used two different specifications in constructing the measures of general political knowledge. The exact percentages differed in the NES and MIS analy-

ses because the cut points are constrained by the distribution of respondents on the general knowledge measures. For one set of equations I defined "high information" as the top 30% (NES) or 28% (MIS) of respondents on the general knowledge scale; "low information" was defined as the bottom 32% (NES) or 34% (MIS). For the second set of equations, I included only the top 13% (NES) or top 10% (MIS) in the "high information" category; "low information" included the bottom 16% (NES) or 20% (MIS).

The 14 analyses (two sets of cut points times seven political judgments) showed little evidence of departure from the logistic model. Only one of the 14 χ^2 tests comparing the logistic model (equations 2 and 3) with the dummy variable model in equation 4 was significant.[13] Given the apparent monotonicity, I will follow the lead of earlier analysts in using a logistic function to assess the relationship between policy-specific information and general political knowledge.[14]

Sections A and B in Table 4 show the observed and fully informed policy preferences for respondents at the 35th, 65th, and 100th percentiles of general political knowledge. These percentiles were chosen to represent rather poorly informed, moderately well-informed, and fully informed respondents, respectively. Section C of Table 4 shows the effect of policy-specific ignorance at each of these levels of general knowledge (i.e., the difference between imputed scores based on the observed level of policy-specific information at each percentile of general knowledge and the imputed scores based on a sample in which all respondents held the relevant policy-specific information).

Two of the seven political judgments show no appreciable differences in the importance of policy-specific ignorance across levels of general knowledge ("oppose import limits to save jobs" and "Bush cares less for the environment than Dukakis"). For the other five political judgments, policy-specific ignorance is most important in distorting the policy preferences of those who score highest in general knowledge, and in three of these cases the differences are quite substantial (regarding spending for the environment, foreign aid, and prison construction). These results support the "resource" hypothesis that policy-specific facts are more likely to be incorporated into the political judgments of those who are more knowledgeable, interested, and sophisticated about politics. Despite their greater store of political knowledge, these respondents do not appear to resist new information, at least not the kind of specific factual information examined here.

A complication in interpreting the differing importance of policy-specific ignorance across levels of general political knowledge stems from the possibility that people who score low on general knowledge may be

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extent that the message is informational, we might expect the largest changes in policy preferences among this group (see McGuire 1968). Identifying the extent to which a message falls into each of these categories can be problematic. Nevertheless, it seems safe to say that most people would regard the kind of policy-relevant facts examined here as primarily informational (although they can, of course, be included in broader, more propagandistic, messages).

[13] Using the 30%–32% cutpoints only, the effect of policy-specific ignorance about the Reagan administration's environmental policies was significantly smaller for moderately informed respondents than for those with either high or low general political knowledge.

[14] Bartels (1996) reports that his examination of a variety of nonlinear functions (both monotonic and nonmonotonic) did not produce any significant improvement in fit over his linear model of general information effects.

TABLE 4. Effect of Policy-Specific Ignorance on Political Judgments by Level of General Political Knowledge and Issue

Information: Political Judgment:	Deficit Increased — Unwilling to Pay More Taxes to Reduce Deficit	Unemployment Declined — Oppose Import Limits to Save Jobs	Unemployment Declined — Increase Spending to Help Unemployed	Environmental Efforts Declined — Increase Spending for Environment	Environmental Efforts Declined — Bush Cares Less for Environment Than Dukakis	Foreign Aid Is Less Than 1% of Federal Spending — Cut Spending for Foreign Aid	Crime Has Declined — Increase Spending for Prison Construction
A. Observed level of policy-specific information							
General knowledge percentile							
35th percentile	81.8 (1.5)	18.2 (1.6)	29.7 (1.5)	62.2 (1.7)	26.8 (1.7)	66.5 (2.9)	35.0 (5.2)
65th percentile	72.1 (1.8)	25.6 (1.4)	29.2 (1.8)	67.8 (1.8)	34.6 (2.0)	63.3 (3.3)	38.8 (5.1)
100th percentile	56.6 (3.9)	36.8 (3.0)	30.7 (2.6)	71.7 (2.7)	43.5 (3.6)	58.6 (5.7)	45.8 (8.2)
B. Full policy-specific information							
General knowledge percentile							
35th percentile	82.9 (1.8)	16.7 (2.0)	27.7 (2.4)	64.1 (4.7)	38.8 (4.9)	59.2 (3.3)	36.6 (5.5)
65th percentile	71.5 (2.1)	25.0 (1.8)	25.1 (2.5)	79.4 (2.8)	48.6 (3.5)	50.6 (3.3)	30.2 (4.5)
100th percentile	51.4 (4.2)	38.3 (4.6)	24.7 (3.8)	89.9 (2.7)	58.1 (5.6)	42.0 (5.1)	26.9 (6.5)
C. Effect of policy-specific ignorance (B–A)							
General knowledge percentile							
35th percentile	1.1 (1.2)	−1.5 (1.4)	−2.0 (1.9)	1.9 (4.5)	12.0 (4.7)	−7.3 (4.5)	1.6 (8.0)
65th percentile	−0.6 (1.0)	−0.6 (1.3)	−4.1 (2.0)	11.6 (2.8)	14.0 (3.1)	−12.7 (4.5)	−8.6 (6.6)
100th percentile	−5.2 (2.2)	1.5 (2.9)	−6.0 (3.1)	18.2 (2.8)	14.6 (5.1)	−16.6 (7.8)	−18.9 (9.7)
N	1,422	1,562	1,620	1,597	1,384	1,102	409

Sources: 1988 National Election Study and 1998 Multi-Investigator Study.
Note: Entries in sections A and B show the predicted percentage of respondents expressing the political judgment indicated. The first five columns report results based on nonexperimental analyses of NES data using equation 2; the last two columns report results based on experimental analyses of the MIS data using equation 3. Standard errors are in parentheses.

more likely to answer the policy-specific questions correctly by taking a random guess than are people who score high on general information (who may be more likely to answer these questions correctly because they actually know the answer). If so, this would attenuate the apparent association between general and policy-specific information and lead to an alternative interpretation of the finding that policy-specific ignorance matters more for those who score high in general information: Their correct answer is a better indicator of actually possessing the policy-specific fact in question.

Although some part of the relationship between general political knowledge and the importance of policy-specific ignorance may be due to "differential guessing" among respondents at different levels of general political knowledge, a number of factors suggest that any such biases are likely to be quite small. First, two of the three Reagan retrospective questions

have an explicit "don't know" filter (e.g., "Have federal efforts to protect the environment increased, decreased, or stayed about the same as they were in 1980, or haven't you paid much attention?"). Such filters reduce the tendency to guess when respondents do not know an answer (e.g., Schuman and Presser 1981). Second, among respondents who scored at the bottom of the general political information scale, the proportion who gave a correct answer to the environmental question was only 5%; 73% said "don't know." Similarly, only 7% of the least well-informed respondents gave the correct answer on the crime question.

These small proportions suggest that the tendency of poorly informed respondents to guess is not strong. These data also suggest that, unless people were more inclined to guess on the other policy-specific factual questions than on these, most of the poorly informed who answered the other factual questions correctly must have possessed the relevant information. Finally,

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TABLE 5. Effect of Policy-Specific Information on Political Judgments by Level of General Political Knowledge and Issue

Information:	Deficit Increased	Unemployment Declined		Environmental Efforts Declined	
Political Judgment:	Unwilling to Pay More Taxes to Reduce Deficit	Oppose Import Limits to Save Jobs	Increase Spending to Help Unemployed	Increase Spending for Environment	Bush Cares Less for the Environment Than Dukakis
A. No policy-specific information					
General knowledge percentile					
35th percentile	80.6 (2.7)	20.2 (2.3)	32.5 (2.2)	61.0 (1.9)	25.0 (2.0)
65th percentile	74.7 (4.5)	25.8 (3.0)	34.4 (2.8)	61.8 (2.1)	28.3 (2.2)
100th percentile	66.0 (7.8)	33.6 (5.3)	38.3 (4.5)	61.5 (3.4)	33.0 (3.8)
B. Full policy-specific information					
General knowledge percentile					
35th percentile	82.9 (1.8)	16.7 (2.0)	27.7 (2.4)	64.1 (4.7)	38.8 (4.9)
65th percentile	71.5 (2.1)	25.0 (1.8)	25.1 (2.5)	79.4 (2.8)	48.6 (3.5)
100th percentile	51.4 (4.2)	38.3 (4.6)	24.7 (3.8)	89.9 (2.7)	58.1 (5.6)
C. Effect of policy-specific information (B–A)					
General knowledge percentile					
35th percentile	2.3 (3.1)	−3.5 (2.9)	−4.8 (2.8)	3.1 (5.0)	13.8 (5.2)
65th percentile	−3.2 (4.5)	−0.8 (3.5)	−9.3 (3.7)	17.6 (3.5)	20.3 (4.0)
100th percentile	−14.6 (7.7)	4.7 (7.1)	−13.6 (6.3)	28.4 (4.0)	25.1 (6.8)
N	1,422	1,562	1,620	1,597	1,384

Source: 1988 National Election Study.
Note: Entries in rows A and B show the predicted percentage of respondents expressing the political judgment indicated based on equation 2. Standard errors are in parentheses.

guessing is unlikely to produce a correct answer on the open-ended question about level of foreign aid spending, and that item shows the same pattern of relationship with general knowledge as do the close ended questions, which can be guessed at more easily (Table 4).

The results in Table 4 reveal the practical importance of policy-specific ignorance at different levels of general knowledge. But these figures reflect both the effect of existing levels of policy-specific ignorance and the influence of policy-specific information on political judgments. For a better assessment of the more theoretically oriented question of how responses to policy-specific information vary across levels of political information, I next compare the imputed scores under the condition that all respondents are ignorant of the policy-specific facts or that all respondents are informed. (The randomized experiment in the MIS survey provides policy-specific information to the treatment group but cannot "take away" such information from the control group. Consequently, the comparison of the wholly ignorant and wholly informed conditions must rely on statistical imputation rather than the experimental approach.)

Section A of Table 5 shows the imputed judgments

of a hypothetical sample in which all respondents lack the relevant policy-specific information, section B shows the imputed scores for a sample in which all respondents possess this information, and section C gives the difference between the two.[15] Unlike tables 2–4, Table 5 does not report the actual (i.e., observed) levels of policy-specific information. Consequently, the tendency for respondents with higher levels of general political knowledge to possess the relevant policy-specific facts does not affect the results of these analyses, and the pattern that emerged in Table 4 is even more pronounced here. For each of these political judgments, the influence of policy-specific information is greatest at the highest level of general political knowledge. With the exception of opposition to import limits, in all cases knowledge of policy-specific facts makes a substantial difference in the political judgments of highly informed respondents. For the poorly informed, however, only knowledge about environ-

[15] Following the analogous procedure used to impute scores for a fully informed sample, I computed the scores reported in Table 5 by assigning to each respondent a score of 0 (section A) or 1 (section B) for policy-specific information and then aggregating the predicted probabilities as estimated with equation 2.

mental efforts during the Reagan administration seems to have any effect, and that appears to be restricted to judgments about the environmental concerns of Bush and Dukakis, not government spending to protect the environment.

The sort of policy-specific information examined here seems to be important in shaping the political judgments of the most politically sophisticated Americans. But the effect is weaker and less consistent even for those with moderately high levels of sophistication (i.e., at the 65th percentile), and it is mostly absent among the poorly informed. It appears that lack of political sophistication affects political judgments in two ways: directly, as previous research has demonstrated, and by diminishing the likelihood that policy-specific information will be incorporated into political judgments.

DISCUSSION

The analyses reveal considerable variation in the importance of policy-specific ignorance across the seven political judgments examined. Fully informed respondents with policy-specific information differ substantially from those who lack such information in terms of their perceptions of where Bush and Dukakis stand on the environment as well as in terms of preferences for spending on prisons, foreign aid, and the environment (tables 2 and 3). Policy-specific ignorance is much less important in explaining preferences for spending to help the unemployed or willingness to pay higher taxes to reduce the deficit, and it seems to have no effect on support for limiting imports in order to protect American jobs.

What accounts for this variation across political judgments? One factor already discussed is the variation in ignorance of the various policy-specific facts examined. In the cases of heaviest influence, between one-half and three-quarters of fully informed respondents lacked the relevant policy facts.[16] In contrast, only one in five of the fully informed was unaware that unemployment had declined, and only one in twenty was unaware that the federal deficit had grown.

Nevertheless, when we assess not the influence of policy-specific ignorance (which reflects, in part, the extent of ignorance about any particular fact) but the influence of policy-specific information (which reflects the effect of information on preferences irrespective of the prevalence of that information in the population), we still find considerable variation across the judgments examined (Table 5). Two factors contribute to this variation. The first is variation in the perceived relevance of the information to respondents' judgments of the issue at hand. For example, knowledge about a change in the unemployment rate may be unrelated to attitudes toward import limits because these attitudes are rooted in other kinds of considerations (such as nationalism or support for free trade). In this case, the information may be perceived as

simply not particularly relevant to the judgment at hand. In other cases, the information may be perceived as more relevant. Thus information about unemployment, which has little effect on attitudes toward import limits, has a larger influence on preferences for spending to help the unemployed (Table 5).

The second factor is that the precise content of policy-specific information can affect its influence on political judgments. For example, unemployment was about two percentage points lower in 1988 than in 1980. Knowledge of the decline might have had a larger influence on policy preferences if the size of the decline had been larger.

Further complicating the situation is that both perceived relevance and informational content are mediated by personal factors. My analyses allow the effect of policy-specific information (and general political knowledge) to vary among respondents with different demographic characteristics, but other individual characteristics may be more important (and more theoretically significant). For example, people with conflicting considerations on a particular issue may be more sensitive to policy-specific information on that issue. Thus, the influence of the unemployment rate on support for import limits may be substantial for those who place a high value on both American jobs and free trade, but it may be minimal for those whose thinking about import policy is dominated by nationalist feelings and concern for Americans' jobs, since both of these considerations lead to the same policy conclusion.

The broader project implied by these observations is to develop a theory of policy-specific information effects that can identify in advance the individual characteristics and the combination of information and political judgments that will produce the strongest and weakest effects.

CONCLUSION

Previous research demonstrates that "information matters" in shaping the public's political judgments. This article reveals that the kind of information that matters is not only general political knowledge, interest, or cognitive capacity but also the specific facts germane to particular political issues. More specifically, three conclusions can be drawn. First, policy-specific facts can be an important influence on political judgments. Second, this influence is not adequately captured by measures of general political knowledge. Third, the consequences of policy-specific ignorance and the effects of policy-specific information are greatest for Americans with the highest levels of general political knowledge.

It may seem obvious that respondents who know that environmental efforts declined during the Reagan administration are less likely to view George Bush as concerned with the environment, or that informing respondents that foreign aid represents less than one percent of the federal budget diminishes their desire to cut foreign aid spending. But the power of such information to shape the public's political judgments is anything but obvious. First, as tables 4 and 5 showed, such facts have a weak and inconsistent effect on the

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[16] That is, environmental efforts declined during the Reagan administration, crime declined over the previous decade, and foreign aid represents 5% or less of federal spending.

preferences expressed by less politically knowledgeable Americans. For these citizens, policy-relevant facts seem to carry little weight.

Furthermore, previous analysts of policy preferences have not expressed much faith that the kind of policy-specific information discussed here plays an important role in shaping Americans' political views. Zaller's (e.g., 1992) influential studies of change in mass opinion focus strongly on elite leadership as the source of preference formation and change. Even the foremost proponents of the "rational public" attribute the public's rationality primarily to the use of elite cues rather than raw policy-relevant information. Page and Shapiro (1992) allow that individuals may at times recognize the significance of new policy-relevant facts and adjust their policy preferences accordingly. But "more likely," they write, "responsiveness to new information results from individuals using cognitive shortcuts or rules of thumb, such as reliance upon trusted delegates or reference figures (friends, interest groups, experts, political leaders) to do political reasoning for them and to provide guidance" (p. 17).

The findings presented here do not contradict the belief that elite cues are more important in shaping the public's political judgments than are raw policy-relevant facts. But they do suggest that, at least for the more politically knowledgeable and sophisticated segments of the public, the influence of raw facts can be substantial.

Despite the central importance of the public's policy preferences to democratic theory, we remain surprisingly ignorant of the forces that shape them. Future research needs to bring together microlevel studies that examine the effect on individuals of different kinds of politically relevant messages and macrolevel studies of the distribution of such messages in American society. For example, attitudes toward anticrime policy surely reflect some combination of factual information (or misinformation), policy endorsements from social and political elites, personal experience with crime, deeply seated beliefs about human nature, and the subtextual messages about crime that permeate both fictional and nonfictional media. Much creative and valuable work along these lines has already been accomplished, but the patches of illumination make the darkness that surrounds them all the more conspicuous. The findings presented here contribute to this larger project by demonstrating the value of directing more light toward the role of basic policy-relevant perceptions and misperceptions in shaping the public's political judgments.

APPENDIX

Survey Items from the 1998 Multi-Investigator Study

General Information. Now for some questions about the federal government. Which party has the most members in the House of Representatives in Washington? (Democrats: 18%; Republicans: 56%; don't know: 26%)

How much of a majority is required for the U.S. Senate and House of Representatives to override a presidential veto—one-half plus one vote, three-fifths, two-thirds, or three-quarters? (one-half plus one vote: 17%; three-fifths:

5%; two-thirds: 46%; three-quarters: 13%; other: 1%; don't know: 18%)

In general, thinking about the political parties in Washington, would you say Democrats are more conservative than Republicans, or Republicans are more conservative than Democrats? (Democrats: 29%; Republicans: 60%; both equal: 4%; don't know: 7%)

Whose responsibility is it to determine if a law is constitutional or not—is it the president, Congress, or the Supreme Court? (president: 5%; Congress: 22%; Supreme Court: 68%; don't know: 4%)

How many four-year terms can the president of the United States serve? (two: 89%; some other number: 9%; don't know: 3%)

How many members of the U.S. Supreme Court are there? (nine: 20%; some other number: 39%; don't know: 41%)

What political office is now held by Al Gore? (vice president: 83%; some other office: 2%; don't know: 16%)

Policy-Specific Information and Policy Preferences. Please give me your best guess for this next question. For every dollar spent by the federal government in Washington, how much of each dollar do you think goes for foreign aid to help other countries? (mean: 26 cents; standard deviation: 20 cents) [Asked only of respondents who were not provided with correct information about foreign aid.]

Would you say there is more, less, or about the same amount of crime in the United States today as compared to 10 years ago? (more: 73%; less: 12%; same: 15%; don't know: 1%) [Asked only of respondents who were not provided with correct information about crime.]

How do you feel about the amount of money the federal government (in Washington) spends on foreign aid to other countries? Do you think the federal government should spend more on foreign aid, less, or about the same as it does now? (more: 9%; less: 56%; same: 34%; don't know: 1%)

How do you feel about the amount of money state and federal governments spend on building prisons? Do you think we should spend more on building prisons, less, or about the same as we do now? (more: 30%; less: 27%; same: 42%; don't know: 2%)

Reassessment of Policy Preferences (Asked Near the End of the MIS Interview). For each of the following government programs, please tell me if you think the government is spending too little money, too much money, or about the right amount. How about for foreign aid to other countries? Would you say the federal government in Washington is spending too little money, too much money, or about the right amount for foreign aid? (too little: 7%; too much: 61%; about right: 31%; don't know: 1%)

How about for building prisons? Would you say the state and federal governments are spending too little money, too much money, or about the right amount for building prisons? (too little: 25%; too much: 28%; about right: 46%; don't know: 1%)

Control Variables (Dummy Variable Categories Are Indicated by Italics). What is your present religious preference? Is it Protestant, Catholic, Islam, Jehovah's Witness, or something else? (*Protestant*: 48%; Catholic: 23%; Other religion: 20%; none: 9%)

What is the highest grade or year of school you completed? (eighth grade or lower: 2%; some high school: 10%; high school graduate (or GED): 40%; some college: 25%; college graduate: 15%; some graduate work or graduate degree: 9%)

What race or ethnic group do you consider yourself? (*Black*: 13%; nonblack: 87%)

Are you male or female? (male: 48%; female: 52%)

Are you currently married, living with someone in a marriage-like relationship but not legally married, separated, divorced, widowed, or have you never been married? (married: 58%; other: 42%)

Do you have any children? How many of your children are under 18? (one or more children under 18: 39%; no children under 18: 61%)

Region. (*East*: 17%; *Midwest*: 26%; *South*: 34%; *West*: 23%)

Which of the following best describes that area you live in—urban, rural, or suburban? (*Urban*: 32%; *Rural*: 32%; suburban: 36%)

Are you currently employed full-time, employed part-time, unemployed, retired, a student, keeping house, or what? (*Retired*: 15%; *Keeping house*: 5%; other: 80%)

Generally speaking, do you usually think of yourself as a Democrat, a Republican, an Independent, or what? (*Democrat*: 37%; *Republican*: 27%; other: 36%)

General Knowledge Items from the 1988 National Election Study

V555 (interviewer rating): Respondent's general level of information about politics and public affairs seemed: (very high: 10%; fairly high: 26%; average: 32%; fairly low: 21%; very low: 10%)

V871: I'm going to read the names of various public figures. We want to see how much information about them gets out to the public from television, newspapers, and the like. The first name is Ted Kennedy. Do you happen to know what job or political office he now holds? (Correctly identifies Kennedy as senator (from Massachusetts): 69%; identification is incomplete or wrong: 6%; makes no attempt to guess: 25%)

V872: George Schultz? (Correctly identifies Schultz as secretary of state (foreign affairs): 39%; identification is incomplete or wrong: 11%; makes no attempt to guess: 50%)

V873: William Rehnquist? (Correctly identifies Rehnquist as Chief Justice of the Supreme Court: 3%; identification is incomplete or wrong: 20%; makes no attempt to guess: 76%)

V874: Mikhaïl Gorbachev? (Correctly identifies Gorbachev as leader of the (Russian) Communist Party or of the Soviet Union (Russia)—i.e., General Secretary of the Communist Party; President or Prime Minister of the Soviet Union/Russia; the Russian leader; Head Red: 71%; identification is incomplete: 9%; makes no attempt to guess: 20%)

V875: Margaret Thatcher? (Correctly identifies Thatcher as Prime Minister of Great Britain (England): 60%; identification is incomplete: 19%; makes no attempt to guess: 21%)

V876: Yasser Arafat? (Correctly identifies Arafat as leader of the Palestinian Liberation Organization—i.e., Chairman of the PLO; leader of the Palestinian people; "PLO"; Palestinian leader; leader of Palestine: 37%; identification is incomplete: 19%; makes no attempt to guess: 44%)

V877: Jim Wright? (Correctly identifies Wright as Speaker of the House of Representatives: 14%; identification is incomplete: 9%; makes no attempt to guess: 77%)

V878: Do you happen to know which party had the most members in the House of Representatives in Washington before the election (this/last) month? (Republicans: 12%; Democrats: 59%; No, don't know: 28%)

V879: Do you happen to know which party had the most members in the U.S. Senate before the election

(this/last) month? (Republicans: 11%; Democrats: 54%; No, don't know: 35%)

V231: We hear a lot of talk these days about liberals and conservatives. Here is a 7-point scale on which the political views that people might hold are arranged from extremely liberal to extremely conservative. Where would you place Michael Dukakis on this scale? (extremely liberal: 9%; liberal: 22%; slightly liberal: 15%; moderate; middle of road: 14%; slightly conservative: 7%; conservative: 6%; extremely conservative: 2%; don't know: 24%)

V232: Where would you place George Bush (on this scale)? (extremely liberal: 2%; liberal: 3%; slightly liberal: 5%; moderate; middle of road: 11%; slightly conservative: 18%; conservative: 31%; extremely conservative: 9%; don't know: 21%)

V234: Where would you place the Republican Party (on this scale)? (extremely liberal: 2%; liberal: 6%; slightly liberal: 5%; moderate; middle of road: 12%; slightly conservative: 17%; conservative: 33%; extremely conservative: 10%; don't know: 15%)

V235: Where would you place the Democratic Party (on this scale)? (extremely liberal: 6%; liberal: 25%; slightly liberal: 20%; moderate; middle of road: 16%; slightly conservative: 10%; conservative: 7%; extremely conservative: 2%; don't know: 14%)

V321: There is much concern about the rapid rise in medical and hospital costs. Some people feel there should be a government insurance plan which would cover all medical and hospital expenses for everyone. Others feel that all medical expenses should be paid by individuals, and through private insurance plans like Blue Cross or other company paid plans. Where would you place the Republican Party (on this scale)? ([1] government insurance plan: 2%; [2]: 1%; [3]: 4%; [4]: 13%; [5]: 15%; [6]: 15%; [7] private insurance plan: 12%; don't know: 21%; haven't thought much about it: 16%)

V322: Where would you place the Democratic Party (on this scale)? ([1] government insurance plan: 8%; [2]: 12%; [3]: 16%; [4]: 13%; [5]: 7%; [6]: 4%; [7] private insurance plan; 2%; don't know: 22%; haven't thought much about it: 16%)

V307: Some people think the government should provide fewer services, even in areas such as health and education in order to reduce spending. Suppose these people are at one end of the scale at point 1. Other people feel it is important for the government to provide many more services even if it means an increase in spending. Suppose these people are at the other end, at point 7. And of course, some other people have opinions somewhere in between at points 2, 3, 4, 5, or 6. Where would you place the Republican Party (on this scale)? ([1] government provide many fewer services, reduce spending a lot: 6%; [2]: 12%; [3]: 20%; [4]: 22%; [5]: 11%; [6]: 5%; [7] government provide many more services, increase spending a lot: 3%; don't know: 21%)

V308: Where would you place the Democratic Party (on this scale)? ([1] government provide many fewer services, reduce spending a lot: 1%; [2]: 3%; [3]: 6%; [4]: 18%; [5]: 22%; [6]: 19%; [7] government provide many more services, increase spending a lot: 8%; don't know: 22%)

V315: Some people believe that we should spend much less money for defense. Others feel that defense spending should be greatly increased. Where would you place the Republican Party (on this scale)? ([1] greatly

TABLE A-1. Logistic Analysis of Preference for Environmental Spending

	Main Effects	Interactions with General Information	Interactions with Policy-Specific Information
General information	2.390 (1.792)	—	—
Policy-specific information	1.844 (1.579)	3.119 (.843)	—
Income	.858 (.600)	−1.275 (1.067)	−.092 (.881)
Age	−1.257 (.756)	2.210 (1.469)	1.277 (1.336)
Married	−.280 (.283)	.749 (.526)	−.101 (.435)
Education	1.907 (.910)	−1.178 (1.679)	−1.321 (1.468)
Republican	.624 (.368)	−2.335 (1.018)	−2.311 (1.155)
Democrat	.268 (.364)	−.911 (1.027)	−2.221 (1.132)
Black	−.139 (.365)	−.456 (.792)	.874 (.628)
Female	−.401 (.287)	.438 (.514)	.685 (.433)
Union family	−.118 (.342)	.304 (.623)	.318 (.471)
Homeowner	−.058 (.285)	−.470 (.547)	−.285 (.464)
Children under 18	.228 (.267)	−.649 (.505)	.184 (.407)
Worse off than last year	−.051 (.268)	−.547 (.515)	.414 (.396)
Protestant	−.051 (.428)	.025 (.724)	−.667 (.593)
Catholic	.730 (.475)	−1.073 (.815)	−1.026 (.642)
East	−.792 (.446)	1.745 (.775)	.136 (.609)
Midwest	−.252 (.409)	.270 (.691)	.186 (.498)
South	−.201 (.392)	.076 (.674)	.371 (.511)
Urban	−.101 (.332)	−.117 (.591)	.423 (.482)
Rural	−.273 (.272)	−.187 (.515)	−.284 (.444)
Retired	.043 (.493)	−.004 (.876)	−1.456 (.714)
Homemaker	.535 (.381)	−1.043 (.843)	.355 (.714)
Executive/professional	−.321 (.456)	.521 (.742)	.419 (.552)
Clerical	1.001 (.453)	−1.670 (.842)	.272 (.677)
Technical/sales	.003 (.467)	.300 (.805)	−.120 (.616)
Constant	−.848 (.856)	—	—

Source: 1988 National Election Study.
Note: The dependent variable is the preference for increasing spending to protect the environment (scored 1) versus decreasing spending or keeping spending the same (scored 0). Standard errors are in parentheses. Policy-specific information consists of knowledge that environmental efforts declined during the Reagan administration. Log-likelihood = −929.5, chi-square = 233.8, $p < .0001$, $n = 1,597$.

decrease defense spending: 1%; [2]: 2%; [3]: 4%; [4]: 14%; [5]: 23%; [6]: 24%; [7] greatly increase defense spending: 12%; don't know: 20%)

V316: Where would you place the Democratic Party (on this scale)? ([1] greatly decrease defense spending: 5%; [2]: 11%; [3]: 18%; [4]: 24%; [5]: 12%; [6]: 5%; [7] greatly increase defense spending: 2%; don't know: 22%)

V328: Some people feel the government in Washington should see to it that every person has a job and a good standard of living. Others think the government should just let each person get ahead on his/her own. Where would you place the Republican Party (on this scale)? ([1] government see to job and good standard of living: 1%; [2]: 2%; [3]: 4%; [4]: 15%; [5]: 18%; [6]:

18%; [7] government let each person get ahead: 10%; don't know: 17%; haven't thought much about it: 15%)

V329: Where would you place the Democratic Party (on this scale)? ([1] government see to job and good standard of living: 5%; [2]: 12%; [3]: 18%; [4]: 18%; [5]: 7%; [6]: 4%; [7] government let each person get ahead: 2%; don't know: 19%; haven't thought much about it: 15%)

Calculating Imputed Preferences

To clarify the analytic procedure used to produce the imputed preferences reported in tables 2–5, I illustrate the logic

of these calculations with an example of a single hypothetical respondent and a single model of policy preferences (environmental spending). As described in the text, the model used to impute fully informed preferences contains measures of general political knowledge, policy-specific information (in this case, whether federal environmental efforts declined during the Reagan years), a vector of control variables, interactions between each control variable and general knowledge, interactions between each control variable and policy-specific information, and the interaction between general knowledge and policy-specific information. The coefficients from this model, estimated with logistic regression, are shown in Table A-1 (all variables are rescored to a 0–1 scale). Note that, like any model with interaction terms, the main effects represent not some kind of average effect but the conditional effect of a given predictor for a respondent with a score of 0 on both general knowledge and policy-specific information.

To calculate imputed environmental spending preferences for the respondent (R) using these coefficients, R's scores on each of the predictors are multiplied by the coefficient for that predictor. These products are then summed along with the constant term to produce the estimated "logit" or "log-odds" of R scoring 1 on the dependent variable (in this case, of holding a preference for increased environmental spending). Finally, the resulting logit is converted into the predicted probability of R preferring increased environmental spending as follows:

$$P(Y = 1) = \frac{e^{logit}}{1 + e^{logit}}.$$

This entire process is repeated for each respondent, and the resulting probabilities are averaged to produce the imputed aggregate preferences reported in tables 2–5. By substituting alternative scores on the two information measures (i.e., general knowledge and policy-specific information), and their respective interaction terms, imputed preferences can be calculated for any combination of general and policy-specific information.

For example, to compute the estimated probability that R would favor increased environmental spending if fully informed in terms of both general and policy-specific knowledge, one would substitute a value of 1 in place of R's observed scores on both the general and policy-specific measures. To compute the analogous probability if R were at the 35th percentile of general knowledge but informed about the relevant policy-specific fact, one would substitute a value of .35 for R's general knowledge score and 1 for the policy-specific knowledge score. These substitutions would also affect R's scores on the interaction terms, since the scores on the interaction variables are calculated by multiplying a respondent's information scores by his or her scores on each of the control variables. For example, a 37-year-old respondent would score .26 on the 0 to 1 age variable. If R were 37 years old, the imputed score for the interaction between age and general knowledge at the 35th percentile would be .26 × .35 = .091, and the imputed score for the interaction between age and policy-specific knowledge would be .26 × 1.0 = .26.

Based on the coefficients shown in Table A-1, R's predicted logit under the hypothetical condition of being at the 35th percentile of general knowledge (general information score = .35) and aware that environmental efforts had declined (policy-specific information score = 1.0) would be the sum of the products shown in Table A-2 plus the constant term.

For each respondent, then, any specific pair of imputed

TABLE A-2. Calculating an Individual Respondent's Imputed Policy Preference at the 35th Percentile of General Information and Full Policy-Specific Information

	Coefficient	Value	Product
General information	2.390 ×	.35	= .837
Policy-specific information	1.844 ×	1.00	= 1.844
Age	−1.257 ×	.26	= −.327
All other control variables ...	...	...	...
General information × age interaction	2.210 ×	.35 ×.26	= .201
All other general information × control interactions ...	...		
Policy-specific information × age interaction	1.277 ×	1.00 × .26	= .332
All other policy-specific information × control interactions ...	...	...	...
General by policy-specific interaction	3.119 ×	.35 × 1.0	= 1.092

general and policy-specific information produces a set of imputed scores for the interaction terms. All these scores are then substituted into the model of policy preferences as described above, the resulting logits are converted into probabilities, and the probabilities are averaged to produce the imputed aggregate preferences of interest.

Standard Errors

Note that the large number of interaction terms in these equations leads to high standard errors for the logit coefficients, a result reported by Althaus (1998) and Bartels (1996) as well. Because the nonsignificant predictors are theoretically relevant, and to parallel more closely the approach of earlier research, I retain them in most models. When sample size is small, however (the nonexperimental analysis of opposition to foreign aid spending and the experimental analysis of support for prison construction), I exclude those sets of predictors for which neither the main effect nor either interaction effect produces coefficients that exceed their standard errors. This helps conserve degrees of freedom in these analyses and has a very minimal effect on the estimated coefficients.

The bootstrapped standard errors reported in tables 2–5 are estimated as follows. For each logistic analysis, 100 different samples of observations are drawn with replacement from the set of cases available for that analysis, and each of these samples has the same sample size as the original set of cases. From each of these samples, a complete analysis is

conducted that produces logit coefficients and then the projected policy preferences under the various information conditions identified in tables 2–5. The bootstrapped standard error for a particular projected policy preference is simply the standard deviation of the estimates from the 100 different samples. (For a more detailed discussion of the bootstrap and related techniques, see Efron and Tibshirani 1986; Mooney and Duval 1993.)

Note that the standard error for the difference between two information conditions (e.g., observed and fully informed) can be smaller than the standard errors for either or both of the two information conditions themselves. This simply reflects the fact that the variance of the quantity (X minus Y) is equal to the variance of X plus the variance of Y minus twice the covariance of X and Y.

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An Extension and Test of Converse's "Black-and-White" Model of Response Stability

JENNIFER L. HILL Columbia University
HANSPETER KRIESI University of Geneva

In one of the most influential works in the public opinion literature, Philip Converse proposed a "black-and-white" model that divided respondents into two groups: opinion holders and unstable opinion changers. We extend the model by allowing for a group that makes rational opinion changes over time. This enables us to (1) explore hypotheses about the adequacy of Converse's original model, (2) estimate the percentage of the population that belongs to each of the groups, and (3) examine the evidence for Converse's basic claim that unstable changers truly exhibit nonattitudes. Contrary to Converse's suggestion that the unstable group is essentially giving random responses, the results imply that the response behavior of this group may be best interpreted in terms of Zaller's notion of ambivalence. The results also undermine the measurement-error model, which maintains that unstable responses are mainly attributable to deficient survey instruments, not individual opinion change. We use data collected at four time points over nearly two years, which track Swiss citizens' support for pollution reduction.

The boldest claim in Converse's (1964, 245) seminal article on the nature of mass belief systems is that "large portions of an electorate ... simply do not have meaningful beliefs, even on issues that have formed the basis for intense political controversy among elites for substantial periods of time." This so-called nonattitude thesis rests largely on the now familiar fact that whenever the same people are asked the same question at two or more points in time, their opinion statements exhibit a high level of apparently random change.

Converse's interpretation of response instability as nonattitudes was first challenged by the "measurement-error" approach (Achen 1975); this attributes the chance variation in individuals' responses not to the lack of individual opinions but to the inherent difficulty of survey questions to measure them correctly. According to the measurement-error approach, we can put our trust in the citizens, who have real, stable opinions. Rather, it is our measurement instruments that are unreliable, this approach maintains, but their deficiencies are easily remedied by standard psychometric techniques.

In Converse's (1964) "black-and-white" model, there are two groups of individuals—a perfectly stable group and a random group: "There is first a 'hard core' of

opinion on a given issue, which is well crystallized and perfectly stable over time. For the remainder of the population, response sequences over time are statistically random" (p. 242). Converse was led to this simple model by an inspection of panel data on American elections in the late 1950s. Surprisingly, the correlations between the opinions given at the initial interview in 1956 and the terminal interview in 1960 were of the same order of magnitude as the correlations between opinions given in either one of these years and those measured at the intervening interview in 1958. In other words, one could just as easily predict 1960 individual opinions on most of the issues on the basis of opinions in 1956 alone as one could with a knowledge of the more proximal 1958 responses. Achen (1975) interpreted this same surprising stability of correlations as a result of measurement error. He argued that correlations would also be equal across time if respondents were stable in their views and if all observed variability were measurement error. This interpretation suggests that there is just one group of citizens, and everyone has basically stable opinions.

More recently, Zaller (1992), with his influential claim that most people's opinions on most issues are characterized by a tendency toward some degree of ambivalence, adopted an intermediary position in this debate. He believes individual response instability occurs because the particular considerations that happen to be on the top of the individual's head at the time of the interview tend to vary from one occasion to another. As he points out, this interpretation is not inconsistent with the measurement-error model. One reason individuals may bring different considerations to the evaluation of the same question may be that most questions are open to multiple interpretations, as the measurement-error tradition suggests. Another reason may be that the immediate survey context—the order or the framing of the questions—may influence the type of considerations most salient in the mind of respondents when asked to give an opinion.

There is a fundamental difference between these two models, however. The measurement-error tradition

views response error as simply so much noise, but in Zaller's (1992, 75) model "response variation is rooted in an important substantive phenomenon, namely the common existence of ambivalence in people's reactions to issues." The implication is that (p. 94) "even when people are temporally unstable, expressing completely opposing positions at different times, they may still ... be expressing real feelings, in the sense that they are responding to the issue as they see it at the moment of response. Although the perceptions of the issue may change over time, the responses they generate are not, for that reason, lacking in authenticity." Nothing suggests that "every member of the public is ambivalent on every issue, or that everyone is ambivalent to the same degree" (p. 93), but Zaller considers nonambivalence as unusual, indeed, and the thrust of his argument has been to deny the existence of any "true opinion" (p. 94).

Moreover, Zaller's model does not take into account the integration of issue-specific opinions in more elaborate individual belief systems, that is, he treats the various considerations in an individual's mind "with too much equanimity" (Chong 1996, 196). For these reasons, his model tends to be incompatible with the existence of a stable group of opinion holders, whose opinions are firmly embedded in a more integrated "belief system"—"a configuration of ideas and attitudes in which the elements are bound together by some form of constraint or functional interdependence" (Converse 1964, 207).

In the 35 years since Converse's article was published, some evidence has been reported for his nonattitudes thesis (see e.g., Pierce and Rose 1974; Smith 1989), but the debate continues. Nevertheless, the thesis is still considered by many to be the strongest argument in the field. In this article we build a modified version of Converse's two-group model to test his nonattitudes argument. In constructing our model, we start from Converse's assumption that there are two main groups of individuals—a group of opinion holders and a group that we shall call vacillating changers.

With regard to opinion holders, we assume with Converse that their opinions are embedded in an integrated belief system that reinforces them and renders them basically stable over time.[1] With regard to vacillating changers we do not follow Converse. Instead, we test for the possibility that, as Zaller claims, responses to questions may be entirely meaningful to a person at the time of the interview, even if, at first sight, they appear to be random to an outside observer, that is, in this group opinions are susceptible to change because they are weak. The weakness may be the result of a more or less unconscious ambivalence, as described by Zaller; it may be the result of an underlying inner conflict that cannot be resolved in the interview situation, which Alvarez and Brehm (1995, 1997) call ambivalence; it may be the result of uncertainty due to

a lack of information; or it may result from a combination of both ambivalence and uncertainty.

In addition, our model allows for a third group, durable changers, who form an opinion for the first time or who definitively change their opinion during a given period. Converse (1964, 244) noted some departures from the black-and-white model "in modest degrees," which he attributed to the "presence of a 'third force' of people, who are undergoing meaningful conversion from one genuine opinion at $t1$ to an opposing but equally genuine opinion at $t2$." He considered this third force to be small but sufficient to disrupt the fit between the data and the black-and-white model. In the subsequent discussions in the literature, however, this third force disappeared, although there is reason to believe that it may be more or less important, depending on the political events in a given period.[2]

We also assume that the size of these three groups depends on the type of issue and on the intensity and direction of the issue-specific public debates occurring at the time. Policies most likely to give rise to stable opinions are those that are familiar to respondents. Such policies may have been the subject of extensive public debates in the past, may have given rise to a policy consensus among elites, or may have been implemented by specific measures, the implications of which are well known to the respondents from personal experience. As Converse (1970, 177) observes, "there is a very real sense in which attitudes take practice." In addition, such constraining issues are likely to generate crystallized opinions. The constraints can be of a moral or a more material kind. On the basis of the principles or costs involved, people are likely to feel more strongly about a constraining issue.

The problem, of course, is that membership in any one of the three groups is unknown, that is, the people who belong to a given group are not in any way "tagged." Group membership has to be inferred from individual response behavior in a panel study. In order to make these inferences, some straightforward assumptions about the response behavior of each group have to be introduced. Thus, Converse was unable to "tag" the members of his two groups: He attributed all individuals who changed sides to the random group. Yet, these people did not exhaust the total set of respondents following random response paths. By chance alone, some individuals who really belonged to the random group may have given the same answers to an opinion question on successive occasions, which would make them appear to belong to the group of opinion holders, even though, in fact, they were answering in a purely random way. By assuming that all opinion change is purely random, Converse could calculate the aggregate size of the random group (but

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[1] Eagly and Chaiken (1993) and Krosnick and Fabrigar (1995) review a formidable array of evidence in support of the proposition that strong opinions are more resistant to change.

[2] Among such events are electoral campaigns (see Sears and Valentino 1997); great national crises, such as the Civil War and the Great Depression (Campbell et al. 1964, 89–92); or major catastrophes, such as the accident at Three Mile Island (Baumgartner and Jones 1993, 79f).

not individual membership) and test the goodness of fit of the model against the real data.

Our problem is even more complex because we have to distinguish among three groups. The class of statistical models that have become known as finite mixture models allow us to solve this problem. For each individual and for the sample as a whole, this type of model enables us to estimate the probability of belonging to each group on the basis of a set of assumptions about the response behavior of the putative members of the groups. For each group, a separate probability model is specified that describes with as few parameters as possible the response behavior of its members. The overall model can be conceived as a mixture of the three separate models. It is a mixture because, in general, we cannot deterministically separate one group or model from the next, since the members of the different groups cannot be identified as such.

The results support our modified version of Converse's model. Depending on the question, about 40–60% of survey respondents have highly stable opinions (i.e., any change is confined to one side of the agree-disagree continuum), and most of the rest are vacillating changers. By adding the durable changers category we can show that only a small number of those who alter their opinion statements are undergoing stable opinion change. Although we find that most change is due to vacillating changers, their response behavior is not completely random. This result weakens the case for the nonattitudes thesis and, we argue, supports the ambivalence interpretation of response instability. Our findings are least supportive of the measurement-error interpretation. If that interpretation were correct, we would have found just one group of respondents. Instead, our findings provide evidence for the existence of three groups of individuals with strikingly different response behavior.

THE DATA

The data are from a Swiss study on the readiness of the population to support policies aimed at curbing air pollution caused by private cars. This issue domain was chosen because it represents a crucial source of environmental pollution (traffic), it immediately concerns citizens' private lives (regulation of the use of their cars), and it was publicly debated in Switzerland at the time of the study. For six policy measures within this domain, the data comprise responses at four different time points. Each policy measure was discussed more or less intensively at the time of the first interview (December 1993) or just before. The six measures were: *Speed Limits, Tax on CO_2* (implying a price increase for gas of about 10 centimes per liter), large *Gas Price Increase* (up to 2 Swiss francs per liter), *Promotion of Electric Vehicles, Car-Free Zones,* and *Parking Restrictions.* Exact question wording is presented in Appendix A. All six measures refer to the same policy domain, but they differ with respect to two criteria: public familiarity and the degree of constraint they impose on individual behavior. At the time of the

interviews, speed limits, car-free zones, and electrical vehicles were very familiar to the Swiss public; parking restrictions and especially the two fiscal incentives (the CO_2 tax and further gas price increases) were much less well known. Fiscal incentives and speed limits were clearly considered more constraining by the Swiss public than the other measures (Kriesi 1999).

The interviews took place over roughly two years: The second and third interviews were held in spring and early summer 1994, the last in fall 1995. Between the first interview in December 1993 and the second, a national popular vote was organized on several transportation issues related to the policy measures. Moreover, a large public debate on the CO_2 tax was launched by the government just before the second interview. The third interview occurred after the respondents had received a choice questionnaire, which is a sophisticated survey tool invented by Willem Saris and his colleagues during the Dutch General Social Debate about the future of nuclear power in the Netherlands (Neijens 1987; Neijens et al. 1996; Saris, Neijens, and de Ridder 1983). The questionnaire provides both descriptive facts and information agreed upon by several independent experts about the issues in question; it also provides respondents with a procedure (based on decision theory) to use this information efficiently in order to arrive at a "considered opinion." Between the third and the fourth interview, no major issue-related events occurred, but half the sample was provided with a series of articles containing more information about the CO_2 tax.

The first two waves yielded complete responses from 1,062 respondents. There exist missing data in the third and fourth waves. Overall, complete data exist for 669 respondents. The number of complete observations varies slightly by question: The minimum is 669 for the car-free-zones question; the maximum is 688 for the parking restrictions question. In our study, complete case analysis is used for model fitting. Optimally, we would use a more principled approach to missing data (see, e.g., Little and Rubin 1987), but when we compare responses in the first and second waves for the group that responded at all four time points and the group that failed to respond to one or both of the last two waves, we find a relatively similar marginal distribution of responses. This is an encouraging, although by no means sufficient, reason for the assumption of data missing completely at random (Rubin 1976), which is needed for making valid inferences in complete case analysis.

Our analysis focuses on one question at a time. The coding used for responses is:

$$Y_{t,i} = \begin{cases} 1 & \text{for "strongly disagree,"} \\ 2 & \text{for "mildly disagree,"} \\ 3 & \text{for "no opinion,"} \\ 4 & \text{for "mildly agree,"} \\ 5 & \text{for "strongly agree,"} \end{cases}$$

Because the first three policy measures—speed limits and two fiscal incentives—are quite constraining for the Swiss public, they always receive less support than

TABLE 1. Marginal Percentage Distribution for Six Opinions at the First Interview and Subsequent Changes

	t1	t2	t3	t4
Speed Limits				
Strongly disagree	25	+5	−5	−6
Disagree	23	+3	+0	+2
No opinion	4	−2	+0	−1
Agree	25	+1	−2	+4
Strongly agree	24	−6	+7	+0
Tax on CO_2				
Strongly disagree	21	+5	−7	+0
Disagree	18	+2	+5	−1
No opinion	6	−3	+0	+0
Agree	34	−2	−1	+1
Strongly agree	21	−1	+4	+0
Gas Price Increase				
Strongly disagree	27	+5	−2	−8
Disagree	24	+4	−2	+4
No opinion	6	−4	+1	−1
Agree	26	−3	−1	+6
Strongly agree	17	−2	+5	−1
Electric Vehicles				
Strongly disagree	8	−4	−1	+3
Disagree	15	−1	+1	−2
No opinion	4	−2	+4	−5
Agree	49	+3	−3	−5
Strongly agree	25	+4	−1	+9
Car-Free Zones				
Strongly disagree	5	+1	−1	−1
Disagree	10	+4	−1	−1
No opinion	2	−1	+0	+0
Agree	34	+5	−1	−3
Strongly agree	50	−9	+3	+4
Parking Restriction				
Strongly disagree	14	+0	−6	+0
Disagree	18	+5	−1	+2
No opinion	3	−2	+2	−2
Agree	35	+4	+7	−12
Strongly agree	31	−7	−2	+11

Note: Due to rounding the numbers in the first column will not necessarily sum to 100 for a given question.

TABLE 2. Temporal Correlations for the Six Opinions

	t1	t2	t3
Speed Limits			
t2	0.65	—	—
t3	0.67	0.75	—
t4	0.64	0.66	0.70
Tax on CO_2			
t2	0.54	—	—
t3	0.53	0.66	—
t4	0.58	0.60	0.57
Gas Price Increase			
t2	0.58	—	—
t3	0.58	0.72	—
t4	0.56	0.61	0.61
Electric Vehicles			
t2	0.39	—	—
t3	0.36	0.49	—
t4	0.50	0.44	0.43
Car-Free Zones			
t2	0.50	—	—
t3	0.47	0.60	—
t4	0.52	0.51	0.54
Parking Restriction			
t2	0.49	—	—
t3	0.43	0.58	—
t4	0.48	0.45	0.44

THE PROBABILITY MODEL

In this section we describe the specification of the behavioral model we hypothesize to be true by a probability model. The description will be informal and intuitive. For a more formal, mathematical treatment, see Hill and Kriesi 2001.

There are many ways to model longitudinal survey data. Time-series models that postulate ordered multi-nomial logit or probit models (reflecting the discretization of the true continuous "latent" response) at each time point would be natural descriptive models for this data if our goal were to predict future responses. Our goal, however, is to classify different types of behavior. We therefore focus on the similarities in the different types of behaviors within an opinion-changing group rather than the individualities. These behavioral similarities potentially could be captured through complex constraints, but such an approach is less straightforward, given our goals, and these constraints could be complicated to implement. Perhaps most important, the parameters of this type of model would, for the most part, be less transparently relevant to the theory at hand. The model we propose is a direct probability representation of the theory we are trying to test.

Alternatively, we could focus on the product multi-nomial structure of the data, that is, we could envision a separate multinomial distribution (with constraints on the cell probabilities), in which each cell corresponds to a different pattern of responses for an individual over the four periods, for each of the three groups. The model we present is actually mathemati-

the other three measures. This is shown in Table 1, which displays the marginal distributions for the first period and the changes between each period and the one before (e.g., the t3 column displays the change in percentages from t2 to t3).

The bivariate correlations over time for our data, given in Table 2, display the same pattern that led Converse to his black-and-white model, but they suggest a rather high level of stability. For the less constraining issues, they are located in the range (.45 to .50) of the correlations for American social welfare items reported by Converse and Markus (1979), and for the more constraining ones, they are in the range of the American moral issues (.62 to .64).[3]

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[3] Note, however, that the issue-specific stability observed for the Swiss policy measures still falls far short of the stability (.81 to .83) that has been measured in the United States for a basic political orientation, such as one's party identification.

FIGURE 1.   Potential Series of Responses for Opinion Holders (Not Exhaustive)
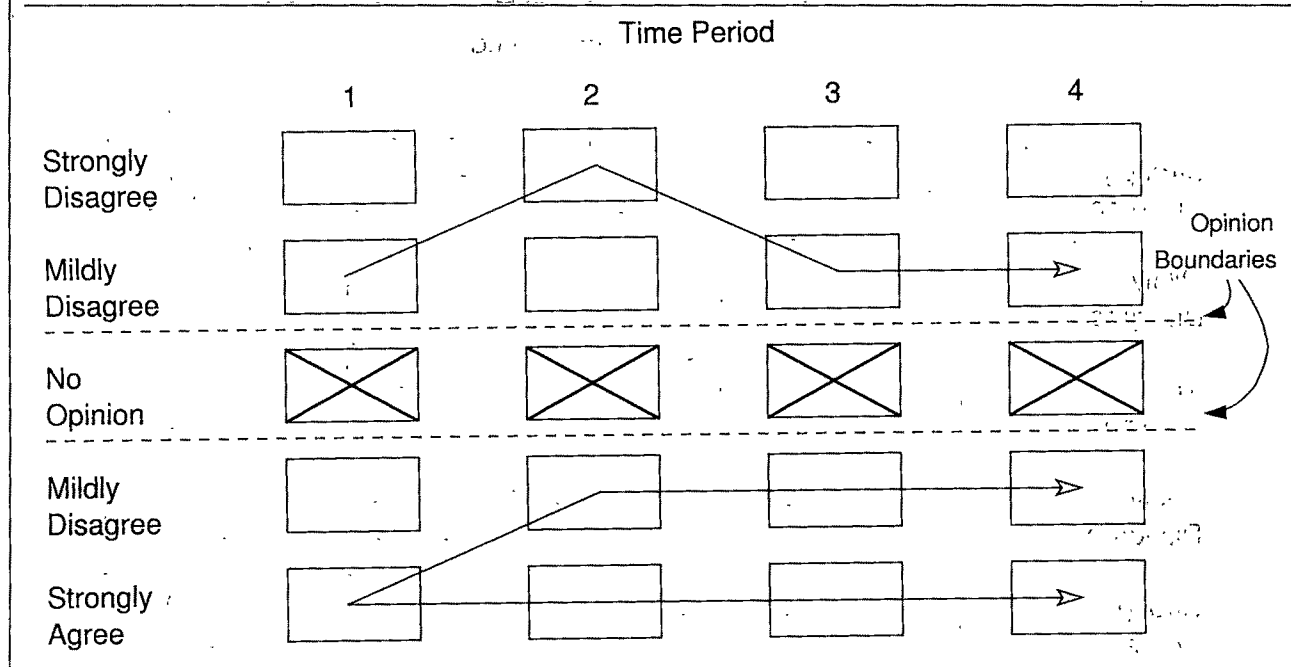
cally equivalent to the product multinomial model with a particular set of complicated constraints. We believe that our parameterization and its associated conceptual representation (such as the tree structure that will be displayed in Figure 4) constitute a clearer mapping from the theoretical model to the probabilistic model.

Parameterization: The Full Model

The Swiss respondents can be characterized as belonging to one of the three groups described above with regard to each policy measure for the duration of the study period. These qualifications are important: The labels used to describe people are policy-issue and time-period dependent. For instance, an individual could be a durable changer regarding the CO_2 tax during the period of this study but might be an opinion holder regarding the same issue ten years later. Similarly, an individual might be a durable changer with respect to the CO_2 tax during this study but could be an opinion holder with respect to speed limits during the study period.

Let π_j, $j = 1, 2, 3$, represent the marginal probability of belonging to each group, where opinion holders, vacillating changers, and durable changers correspond to groups 1, 2, and 3, respectively. These are the probabilities we eventually wish to estimate on the basis of the response patterns. The rest of the parameters are used primarily to characterize the response behavior of the three groups.

Opinion Holders. Opinion holders maintain an opinion either for or against an issue. Anyone who responds "no opinion" at any point cannot be in this group, and anyone who crosses an "opinion boundary" across points (i.e., an opinion holder cannot switch from an

agree to a disagree response, or vice versa) is excluded. Changes between "mildly" and "strongly" agree or disagree, however, are not interpreted as a change of opinion, and those patterns are included within the opinion holder group. Figure 1, which has boxes for each potential response over the four periods, portrays several possible patterns for opinion holders. Boxes that an opinion holder can never occupy contain an X.

Two parameters are sufficient to describe the behavior of a member of this group:

$$\alpha_1 = \Pr(\text{mildly disagree or strongly disagree}), \text{ and}$$

$$\delta_1 = \Pr(\text{strongly disagree} \mid \text{mildly disagree or strongly disagree})$$

$$= \Pr(\text{strongly agree} \mid \text{mildly agree or strongly agree}).$$

Both of these probabilities implicitly condition on the fact that the individual whose behavior they describe is an opinion holder.

These parameters allow for differing probabilities of being for or against an issue and, conditional on being for or against an issue, differing probabilities of feeling strongly or mildly about it. Note that the parameter for the extremity or strength of the reaction (δ_1) does not vary across time or across opinions (agree or disagree).[4] We shall make the same assumption for the other two groups, although an inspection of those who feel strongly about an issue among those who agree as opposed to those who disagree (as shown in Table 1) shows that such an assumption approximately holds in only two of the six cases. Thus, opposition to a gas price

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[4] Extremity is one of many indicators of opinion or attitude strength (see Krosnick and Fabrigar 1995).

FIGURE 2. Potential Series of Responses for Vacillating Changers (Not Exhaustive)
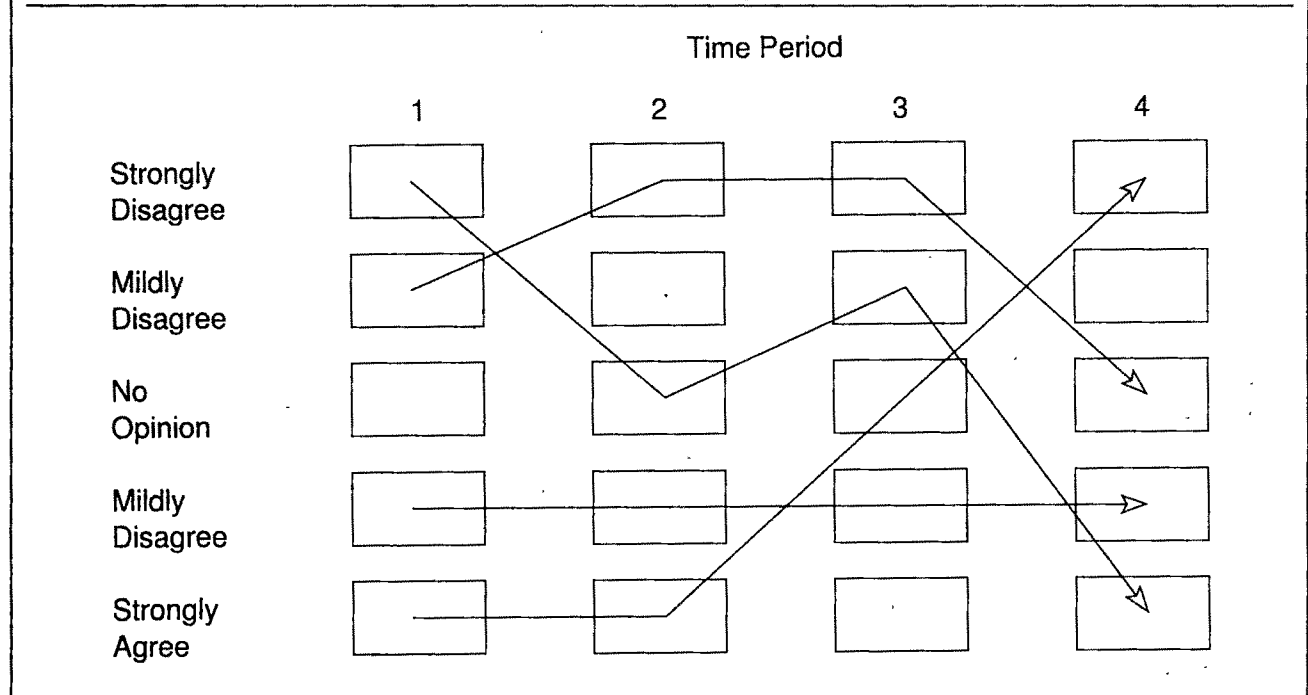
increase is stronger than support, and support for car-free zones is stronger than opposition to them. Moreover, opposition to parking restrictions became weaker in the course of the study, whereas the strength of support for them fluctuated. Only in the cases of speed limits and electrical vehicles did the share of extreme opinions appear to be similar and rather stable on both sides of the opinion divide. Although relatively little is known about the processes by which opinion strength develops (Eagly and Chaiken 1993, 681), we prefer to make this simplifying assumption to keep the number of parameters as small as possible.

Vacillating Changers. We make the simplifying assumption that vacillating changers do not alter their opinions in any particularly systematic way. Moreover, their responses are considered to be independent across time points. Therefore, any pattern of responses could characterize a vacillating changer, for a total of 625 possible response patterns. Figure 2 illustrates just a few.

On the basis of this assumption, the behavior of a vacillating changer at any point can be characterized by three parameters:

$\varphi_2 = $ Pr(no opinion),

$\alpha_2 = $ Pr(mildly disagree or strongly disagree), and

$\delta_2 = $ Pr(strongly disagree | mildly disagree or strongly disagree)

$= $ Pr(strongly agree | mildly agree or strongly agree).

Our model allows some minimal structure in responses of vacillating changers. They are allowed dif-

ferent probabilities (constant over time) for having no opinion, agreeing, or disagreeing. The model also allows them to have different probabilities (constant over time) for extreme versus mild responses, given that they express an opinion. As for "no opinion," there are various reasons members of this group would give such a response, including genuine lack of opinion, ambivalence, question ambiguity, satisficing, intimidation, or self-protection. The point is that these reasons may be somewhat different from the considerations that lead vacillating changers to choose one of the four substantive opinion categories. The fact that our model postulates that the probability to agree can be different from the probability to disagree is contrary to Converse's black-and-white model. His model implicitly assumes that the parameter α_2 is unnecessary and instead imposes the following constraint:

$$\frac{1 - \varphi_2}{2} = \text{Pr(mildly agree or strongly agree)}$$

$$= \text{Pr(mildly disagree or strongly disagree)}.$$

We will report below results from a test for the validity of Converse's constraint (see pp. 409–10).

As we have pointed out, we adopt Zaller's notion that vacillating changers do not respond entirely at random. Accordingly, just as for the opinion holders, the share of opponents among vacillating changers (α_2) is allowed to deviate significantly from the share of supporters. In other words, we assume that, on average, the responses of this group reflect the characteristics of the issues to some extent. Our model only assumes a minimal structure, however, since we consider the parameter α_2 to be constant over time. Note

FIGURE 3.   Potential Series of Responses for Durable Changers (Not Exhaustive)

Time Period
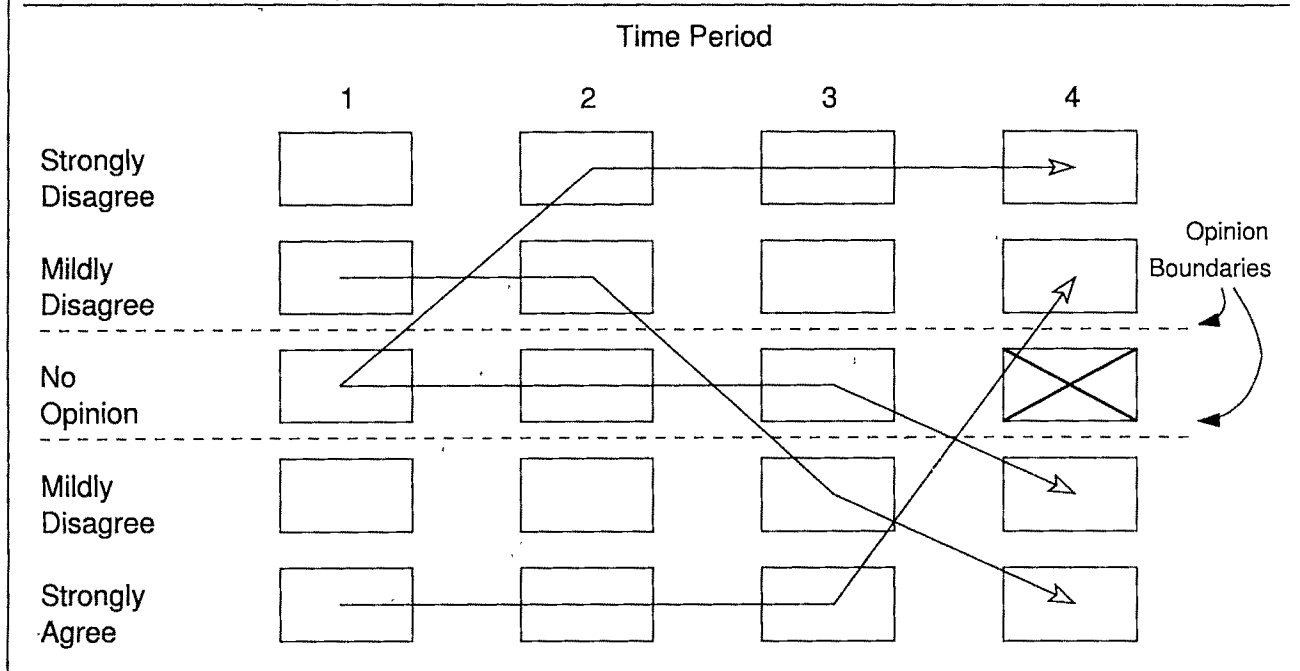
that this does not mean an individual will give the same response over time.

Durable Changers.   Durable changers alter their opinion or form an opinion based on some rational decision-making process, perhaps prompted by additional information or further consideration of an issue. They are allowed to change their opinion (i.e., cross an opinion boundary as displayed in Figure 3) exactly once across the four periods.[5] This characteristic distinguishes them from vacillating changers, who are allowed to move back and forth precisely as a result of their ambivalence or uncertainty. In contrast to vacillating changers, durable changers adopt a new, stable opinion, either by changing sides or by forming an opinion for the first time. They are not allowed to change to the "no opinion" position, but they can move out of this position. This implies that those who abandon a "for" position must switch to an "against" position and vice versa. Possible response patterns are exhibited in Figure 3, where the X indicates a box they cannot occupy.

Durable changers have a bit more structure to their behavior than the other groups, which is reflected in the increased number of parameters. First, we distinguish between a change of sides and a move away from "no opinion." This is the distinction between durably changing an opinion and forming a durable opinion for the first time. Second, we distinguish between the probabil-

ity that a switch of sides or a movement from "no opinion" occurs after the first period and the probability that it occurs after the second or third period. It is well known that respondents in a panel survey clarify their ideas in the course of the study and, as they reflect and learn more about the subject matter, get closer to their "true opinion" (Jagodzinski, Khnel, and Schmidt 1987; McGuire 1960; Saris and van den Putte 1987). This panel or "Socratic effect" typically occurs between the first two waves of a panel study. Accordingly, we allow the probability of durable changes to be different for the first period, but we assume the respective probabilities for the two following periods are identical.

Third, we allow the direction of change to differ between the first period, on the one hand, and the second and third period, on the other hand. Since durable changers are assumed to be strongly influenced by the additional information they receive, the direction of their change is a function of the "tone" of public debate. With respect to the fiscal incentives in particular, but not necessarily with regard to the other measures, this tone was rather negative during the first period; it became much more balanced for all issues during the following two periods.

Fourth, as for the other two groups, we again allow for a stable share of strong opinions. These assumptions translate into the following set of parameters:

$\varphi_3^{(pre1)}$ = Pr(preswitch response is no opinion

$\quad$ | switch occurs after 1st period;

$\varphi_3^{(pre2)}$ = Pr(preswitch response is no opinion

$\quad$ | switch occurs after 2d or 3d period;

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[5] It is arguable that a reasonable relaxation of this model would allow individuals to change from a given opinion (strong or weak) to the no opinion category, and then to the opposite opinion (strong or weak). We find that this pattern occurs quite infrequently (in one case for speed limits and car-free zones; five times each for the gas price increase and parking restrictions; and eight times each for the CO_2 tax and electric vehicles).

FIGURE 4. Tree Structure of the Model
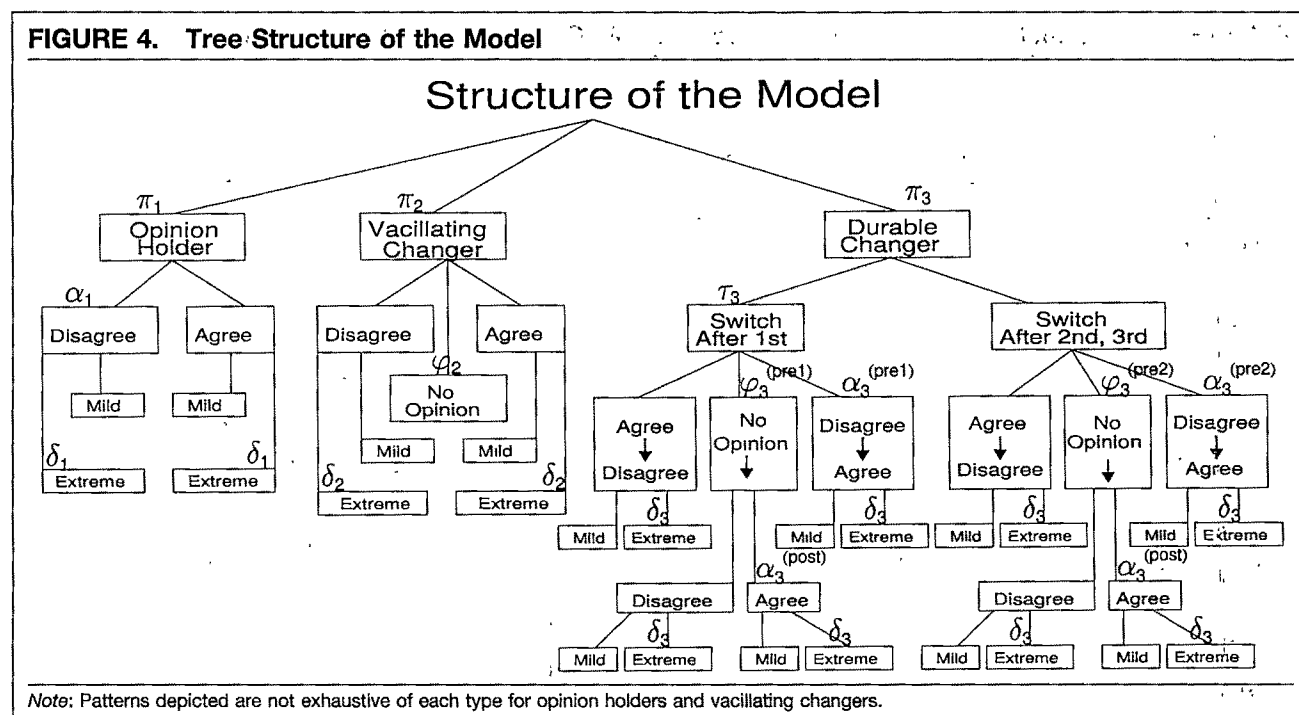
Note: Patterns depicted are not exhaustive of each type for opinion holders and vacillating changers.

$\alpha_3^{(pre1)} = $ Pr(preswitch response is mildly disagree or strongly disagree | switch occurs after 1st period;

$\alpha_3^{(pre2)} = $ Pr(preswitch response is mildly disagree or strongly disagree | switch occurs after 2d or 3d period;

$\alpha_3^{(post)} = $ Pr(preswitch response is mildly disagree or strongly disagree | switched from no opinion;

$\delta_3 = $ Pr(strongly disagree | mildly disagree or strongly disagree);

$= $ Pr(strongly agree | mildly agree or strongly agree);

$\tau_3 = $ Pr(switch occurs after 1st period).

We also impose the following constraint:

$$\frac{1 - \tau_3}{2} = \text{Pr(switch occurs after 2d period)}$$

$$= \text{Pr(switch occurs after 3d period).}$$

Since these probabilities condition on membership in the durable changers group, and this group comprises only individuals who switch exactly once across the four periods, the parametric descriptions of the behavior revolve primarily around descriptions of this opinion switch. Note that only one parameter need be specified for postswitch behavior because we do not differentiate this behavior by switching time and be-

cause switches to "no opinion" are not allowed for this group.

The Model as a Tree

It is helpful to think of the behavior model for these three groups as a tree structure, such as the one illustrated in Figure 4. This tree slightly oversimplifies the representation of our model. For instance, the vacillating changer branch shows the response for just one period. Yet, the figure illustrates the types of behavior pertinent for defining each group.

As noted earlier, we use a finite mixture model (for a description of these models and their properties, see any of Everitt and Hand 1981; Lindsay 1995; Titterington, Smith, and Makov 1985), which can be conceived as a mixture of three separate models and is useful when the "mixing proportions" are unknown. In our case the full model is a mixture of the model for each opinion-changing behavior group, and it is appropriate because, in general, we cannot deterministically separate the groups, since their memberships cannot be identified as such. Our model will be better behaved than some mixture models, however, because of the structure placed on the behavior of each group. In particular, there are certain individuals whose behavior can only be accommodated by the vacillating changers group.[6] Recent examples of fully Bayesian analyses of mixture model applications include Belin and Rubin (1995), Gelman and King (1990), and Turner and West (1993).

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[6] This identification creates a nonsymmetric parameter space that prevents label-switching in the algorithm used to fit this model.

FITTING THE MODEL

Reexpressing the Data

The tree structure illustrates the types of behavior that we need to measure. Rather than define a survey respondent by her response pattern, for example, 1-2-5-5, we need to characterize her in terms of a limited number of more general variables that allow us to reproduce her trajectory, in the case of 1-2-5-5 as someone who started out opposed to the issue (at first extremely, "1," then mildly, "2") and then crossed an opinion boundary and twice expressed strong agreement with the issue, "5, 5." Therefore, all the data have been reexpressed in terms of the variables described below.[7] These variables, along with group indicators, define the elements that are used in the parameter estimates (note that t_i^* denotes the period directly before a change in opinion). That is, whereas the model parameters represent the probabilities of certain types of behavior, the transformed data measure the incidence of these same types of behavior.

$$A_i = \begin{cases} 1 & \text{if the } i^{th} \text{ person's initial response} \\ & \text{is a 4 or 5} \\ 0 & \text{otherwise} \end{cases} ;$$

$B_i =$ the number of the ith individual's responses that are either 1 or 5 across all t;

$C_i =$ number of the ith individual's responses that are 3;

$D_i =$ number of times the ith individual crosses an opinion boundary;

$$E_i = \begin{cases} 0 & \text{if } D_i \neq 1 \\ t_i^* & \text{otherwise} \end{cases} ;$$

$$F_i = \begin{cases} 0 & \text{if the } i^{th} \text{ individual's preswitch} \\ & \text{response is a 1, 2, or 3, or } D_i \neq 1 \\ 1 & \text{if the } i^{th} \text{ individual's preswitch} \\ & \text{response is a 4 or 5} \end{cases} ;$$

$$H_i = \begin{cases} 0 & \text{if the } i^{th} \text{ individual's preswitch} \\ & \text{response is a 1, 2, 4, or 5, or } D_i \neq 1 \\ 1 & \text{if the } i^{th} \text{ individual's preswitch} \\ & \text{response is a 3} \end{cases} ;$$

$$M_i = \begin{cases} 0 & \text{if the } i^{th} \text{ individual's postswitch} \\ & \text{response is a 1, 2, or 3, or } D_i \neq 1 \\ 1 & \text{if the } i^{th} \text{ individual's postswitch} \\ & \text{response is a 4 or 5} \end{cases} ; \text{and}$$

$$Q_i = \begin{cases} 0 & \text{if the } i^{th} \text{ individual's postswitch} \\ & \text{response is a 1, 2, 4, or 5, or } D_i \neq 1 \\ 1 & \text{if the } i^{th} \text{ individual's postswitch} \\ & \text{response is a 3} \end{cases} .$$

The vector of all these random variables for individual i will be denoted Z_i.

The Data Augmentation Algorithm

The problem with using the above model to make inferences is that it relies on knowledge of group membership, which we do not have. The data augmentation (DA) algorithm (Tanner and Wong 1987) is an iterative procedure that sidesteps the problem by focusing on the fact that if we could observe this "missing" data, the calculations would be straightforward. The DA algorithm has two basic steps.[8] First, sample the "missing data," in our case group membership indicators, given the parameters. Second, sample the parameters, $\theta = (\pi_1, \pi_2, \pi_3, \alpha_1, \delta_1, \ldots)$, given the group membership indicators, from their posterior distribution.

Iterations continue until we converge to a stationary distribution, the posterior distribution, which summarizes our knowledge about the parameters, given the observed data.[9] This distribution can be used to calculate any number of quantities of interest, including, for instance, means and intervals. The parameter draws before convergence are treated as a "burn-in" period and discarded.[10] Then as many draws as are desired to estimate sufficiently the empirical distribution are taken.

Posterior distributions formally combine the distribution of the data given unknown parameter values with a prior distribution on the parameters.[11] This prior distribution quantifies our beliefs about the parameter values before we see any data. The priors used in this analysis reflect our lack of a priori information about the parameter values and are thus "noninformative." Sensitivity analyses indicate little sensitivity to a range of noninformative priors. The most sensitivity is demonstrated in the durable changer parameters for the most specific behavioral parameters because these have the least information from the data set (i.e., the

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[7] It is computationally impossible to fit the model as we have described it without reexpressing the data. The only other option would be to fit a model (the product multinomial model described earlier) to the untransformed data, which has a different parameter for each pattern of responses (e.g., 3-3-4-5) for each opinion-changing behavior group (for a total of 1,875 different parameters), and then reduce the effective number of parameters by constraining certain cells to be equal (e.g., for opinion holders a pattern of 1-2-2-2 is as likely as 5-4-4-4).

[8] For a general resource that provides a description of the expectation maximizing (EM) algorithm and the data augmentation (DA) algorithm, as well as some interesting applications, refer to a text on Bayesian statistics, such as Gelman et al. 1995. See Hill and Kriesi 2001 for more details regarding the specifics of the algorithm for this model.

[9] The EM algorithm (Dempster, Laird, and Rubin 1977), which provides only point estimates, was also an integral part of the model-fitting process; it is generally easier to implement and more stable than DA. It is a useful tool for generating starting values, exploring whether there are multiple modes, and even for checking the accuracy of DA code.

[10] Convergence can be assessed using a variety of diagnostics. We chose the $\hat{R}$ statistic proposed by Gelman and Rubin (1992) and its multivariate extension (Brooks and Gelman 1998). These diagnostics monitor the mixing behavior of several chains, each originating from a different starting point.

[11] The formal mathematics to calculate these posterior distributions is straightforward and described in Hill and Kriesi 2001.

least number of cases) to support them compared to the other parameters. Still, with noninformative priors this lack of information is reflected in the wide posterior intervals.

The DA algorithm was used in this problem because non-Bayesian techniques have generally been found to be flawed when applied to mixture models, particularly when calculating standard errors. In addition, the approximations that have been derived to accommodate testing of certain hypotheses are rather limited (see, e.g., Titterington, Smith, and Makov 1985), and they cannot approach the flexibility in the types of inferences that can be performed trivially once we can sample from the posterior distribution (for a discussion, see van Dyk and Protassov 1999). Assuming that the correct model is used, the DA algorithm will converge to the correct posterior distribution. The properties exhibited in our simulations lead us to believe that our DA algorithms had converged by the time we started saving draws from the posterior distribution.

RESULTS

We report the results of the model fit via the DA algorithm for each of the different policy measures. Table 3 presents the point estimate of the mean for each parameter as well as a 95% interval from the empirical posterior distribution (each with 10,000 draws). Note that some of the intervals are very large, which indicates very low precision of some parameter estimates. This reflects our uncertainty regarding some of these parameters caused by the lack of a sufficient number of people who engaged in the type of behavior to which these parameters correspond.

We discuss the plausibility of the results by examining the empirical distributions of some aspects of the response behavior in light of our expectations as to what would be reasonable to observe. These checks are mostly illustrative but should serve to increase our confidence in the model.

Size Estimates for the Three Groups

The most important result concerns the estimates of the sizes of the three groups, opinion holders (π_1), durable changers (π_2), and vacillating changers (π_3). Table 4 presents a summary of these estimates for the six issues, together with the corresponding shares of respondents who did not change sides throughout our study, those who changed sides once, and those who changed them more than once. Changing sides corresponds to any change in opinion among the three major categories: no opinion, agree (mildly or strongly), or disagree (mildly or strongly). As can be seen from this table, the model's estimates of opinion holders vary between roughly three-fifths (car-free zones) and roughly one-third (parking restrictions) of the respondents. The corresponding empirical shares of respondents who never changed opinion throughout the study are consistently higher but not by very much.

This is quite plausible, given that, just by chance, some vacillating changers may never have changed their opinion during the study, although they did not really hold a stable opinion. Car-free zones is the issue

with the largest share of opinion holders and is also the one most familiar to the Swiss public. All larger Swiss cities have had such zones for years, so people have experience with this policy. Another familiar issue is speed limits, which also has a comparatively high share of opinion holders. This confirms' Converse's notion that attitudes "take practice."

As we have noted, there was a public campaign against the two fiscal measures during the first period of our study. The natural experiments that resulted during the second and third periods of the study mimic to a certain extent the effect of a political campaign although they were of somewhat limited intensity and salience, compared to major electoral campaigns, scandals, or catastrophes. The debate about the fiscal incentives was part of a longer process of policy change and had not yet come to a conclusion in the course of our study. Accordingly, the number of durable changers is rather limited. Our model estimates that only a minority of people definitely changed their opinion during the study. The respective estimates vary between 2% (car-free zones) and 8% (speed limits, gas price increase). Undecided public controversies foster ambivalence and uncertainty. As long as no new consensus emerges on the level of the policy elites, citizens are likely to suspend judgment.

Comparing these estimates for durable changers with the corresponding empirical shares of respondents who changed their opinion just once during the period, we note that our estimates are all much smaller than the corresponding actual percentages of people who switched just once. This may be explained by the fact that, by pure chance, many vacillating changers, too, may have switched sides just once during the study. Alternatively, our model may be incorrectly specified or insufficiently complex to distinguish effectively between vacillating and durable changers. Taken at face value, our results confirm the idea that during a period of controversial, but inconclusive, public debate about a policy domain, durable changes in individual opinions occur only rarely, and public opinion progresses incrementally, at a rather slow pace.

We are left with estimates of very sizable groups of vacillating changers, ranging from two-fifths (.39) in the case of speed limits to almost three-fifths in the case of electrical vehicles (.57) and parking restrictions (.58). Even if we take into account that the parameterization is very liberal with respect to this group, and therefore we probably tend to overestimate its size, the estimates are impressively large.

The Implied Response Behavior of the Three Groups

It is not surprising that among opinion holders the estimated share of opponents (α_1) is larger for the more constraining policy measures than for the less constraining ones: The former impose higher costs on the individual, which means that they will give rise to greater resistance. This is shown in Table 5. In addition, the table shows a similar pattern for vacillating changers (α_2). In other words, on average, the responses of the vacillating changers reflect the characteristics of the issues in question.

TABLE 3. Estimates of Parameters and Their Uncertainty for Unconstrained Model

Parameters	Speed Limits Mean	95% Interval	Tax on CO_2 Mean	95% Interval	Gas Price Increase Mean	95% Interval
π_1	0.53	(0.49, 0.57)	0.40	(0.36, 0.45)	0.44	(0.40, 0.49)
π_2	0.39	(0.33, 0.45)	0.53	(0.48, 0.58)	0.48	(0.43, 0.53)
π_3	0.08	(0.05, 0.12)	0.07	(0.04, 0.10)	0.08	(0.05, 0.11)
α_1	0.53	(0.48, 0.59)	0.48	(0.41, 0.54)	0.63	(0.57, 0.69)
δ_1	0.67	(0.64, 0.70)	0.65	(0.61, 0.69)	0.68	(0.65, 0.71)
φ_2	0.03	(0.02, 0.04)	0.06	(0.04, 0.07)	0.07	(0.05, 0.08)
α_2	0.45	(0.39, 0.50)	0.37	(0.34, 0.41)	0.45	(0.42, 0.49)
δ_2	0.22	(0.19, 0.25)	0.27	(0.24, 0.29)	0.25	(0.22, 0.28)
$\varphi_3^{(pre1)}$	0.34	(0.16, 0.57)	0.45	(0.27, 0.66)	0.45	(0.26, 0.65)
$\alpha_3^{(pre1)}$	0.10	(0.00, 0.30)	0.17	(0.00, 0.37)	0.10	(0.00, 0.26)
$\varphi_3^{(pre2)}$	0.09	(0.00, 0.26)	0.36	(0.00, 0.98)	0.06	(0.00, 0.32)
$\alpha_3^{(pre2)}$	0.84	(0.56, 0.99)	0.48	(0.00, 0.98)	0.86	(0.47, 1.00)
$\alpha_3^{(post)}$	0.35	(0.11, 0.66)	0.71	(0.44, 1.00)	0.81	(0.53, 1.00)
δ_3	0.49	(0.35, 0.67)	0.50	(0.39, 0.60)	0.57	(0.43, 0.71)
τ_3	0.59	(0.42, 0.76)	0.88	(0.71, 0.99)	0.75	(0.57, 0.92)
Parameters	Electric Vehicles Mean	95% Interval	Car-Free Zone Mean	95% Interval	Parking Restriction Mean	95% Interval
π_1	0.39	(0.34, 0.45)	0.58	(0.53, 0.63)	0.37	(0.32, 0.43)
π_2	0.57	(0.51, 0.63)	0.40	(0.35, 0.45)	0.58	(0.52, 0.64)
π_3	0.04	(0.01, 0.06)	0.02	(0.00, 0.04)	0.05	(0.02, 0.09)
α_1	0.10	(0.06, 0.14)	0.07	(0.05, 0.10)	0.19	(0.08, 0.28)
δ_1	0.57	(0.52, 0.62)	0.68	(0.65, 0.71)	0.61	(0.54, 0.66)
φ_2	0.06	(0.05, 0.07)	0.02	(0.01, 0.03)	0.04	(0.03, 0.07)
α_2	0.24	(0.21, 0.27)	0.31	(0.27, 0.36)	0.33	(0.22, 0.40)
δ_2	0.20	(0.18, 0.23)	0.26	(0.23, 0.30)	0.23	(0.19, 0.27)
$\varphi_3^{(pre1)}$	0.15	(0.00, 0.55)	0.15	(0.00, 0.76)	0.36	(0.00, 0.86)
$\alpha_3^{(pre1)}$	0.31	(0.00, 0.75)	0.16	(0.00, 0.77)	0.17	(0.00, 0.64)
$\varphi_3^{(pre2)}$	0.41	(0.01, 0.98)	0.25	(0.00, 0.95)	0.13	(0.00, 0.87)
$\alpha_3^{(pre2)}$	0.47	(0.00, 0.95)	0.28	(0.00, 0.96)	0.79	(0.01, 1.00)
$\alpha_3^{(post)}$	0.10	(0.00, 0.74)	0.35	(0.00, 1.00)	0.33	(0.00, 0.96)
δ_3	0.23	(0.06, 0.45)	0.37	(0.02, 0.82)	0.36	(0.12, 0.62)
τ_3	0.73	(0.40, 0.95)	0.63	(0.14, 0.97)	0.60	(0.30, 0.92)

Note: Notational Convention: Subscripts—1 = opinion holders; 2 = vacillating changers; 3 = durable changers. Superscripts—pre1 = opinion before a switch occurring after wave 1; pre2 = opinion before a switch occurring after wave 2 or 3; post = opinion after a switch. π = group membership; δ = extreme; α = disagree; τ = switched after 1st time period; φ = no opinion.

The respective issue-specific differences are much more pronounced, however, among opinion holders than among vacillating changers. That is, the opinions of the latter are less discriminating between constraining and unconstraining issues than are those of the opinion holders. Vacillating changers are more supportive than opinion holders with regard to the more constraining measures (fiscal measures and speed limits) and less supportive with regard to the less constraining measures (electric vehicles, car-free zones, and parking restrictions). This result is compatible with an interpretation of

their response behavior in terms of ambivalence and uncertainty: On average, the responses of the vacillating changers reflect the characteristics of the issues, but as a result of their ambivalence and uncertainty, their overall response is less clear-cut than that of the opinion holders. The shares of opponents among the sample at large turn out to be mixtures of the corresponding shares of the two groups.

Assuming that vacillating changers are characterized by greater issue-specific ambivalence and uncertainty, we also expect them to have weaker opinions about

TABLE 4. Estimated Sizes of the Three Groups, and Shares of Corresponding Number of Respondents Who Changed Sides

Issue	π_1	No Change	π_3	1 Change	π_2	2+ Changes
Speed Limits	.53	.62	.08	.21	.39	.17
Tax on CO_2	.40	.52	.07	.21	.53	.27
Gas Price Increase	.44	.52	.08	.26	.48	.23
Electrical Vehicles	.39	.57	.04	.17	.57	.26
Car-Free Zones	.58	.70	.02	.13	.40	.17
Parking Restrictions	.37	.48	.05	.23	.58	.28

these issues than do opinion holders. Table 6 confirms this expectation: In each and every case, opinion holders (δ_1) are estimated to be much more likely to hold strong opinions than vacillating changers (δ_2). For all the issues, strong opinions are estimated to prevail among opinion holders, whereas weak opinions are estimated to predominate among vacillating changers. In fact, in most instances the model estimates that approximately two-thirds of the opinion holders will have a strong opinion, compared to only roughly one-fourth of the vacillating changers. For the durable changers (δ_3), the respective shares of strong opinions turn out to be more variable than in the other groups. As discussed previously, the model simplification that is least tenable is the constraint placed on the extremity reaction parameters, δ_1, δ_2, and δ_3, to be constant within groups. It is possible to fit a more general model that allows δ to vary across opinion boundaries. We have, however, not pursued this due to sample size constraints.

Evidence for the Measurement-Error Explanation of Response Instability

In order to explore the evidence for or against the measurement-error interpretation of response instability we use our model to calculate the posterior distribution of the average individual-level standard devia-

tion in responses for each group. Recall that for each iteration of the data augmentation algorithm there is a step that probabilistically assigns group membership labels to each person. Group membership is drawn from a multinomial distribution with parameters equal to the probability of membership in each group (π_1, π_2, π_3) for that iteration. Each time these labels are assigned (once per iteration), we treat the responses as ordinal and continuous (which is what is typically done in measurement-error models) rather than nominal and categorical, and calculate the standard deviation of these responses for each person. Averages of these standard deviations are then calculated for each group. These averages across many iterations (just like any of the other parameters in the model) create an empirical distribution for each group (for more details see Hill and Kriesi 2001).

Table 7 presents the means for each of these distributions; 95% intervals for each mean are presented in parentheses. This table demonstrates that the average size of individual-level standard deviation in responses is quite different across groups. The vacillating changers have, on average, between double and triple the amount of response variation compared to the opinion holders. The durable changers have routinely even more variation on average than the vacillating changers. Given the implausibility of assuming vastly different measurement errors for different people, it seems rather more likely that this variation is composed of both measurement error and true opinion instability.

TABLE 5. Estimated Proportion of Opponents among Opinion Holders (α_1), Vacillating Changers (α_2) and the Sample at Large

Issue	α_1	Estimate for Sample at Large	α_2
Gas Price Increase	.63	.54	.45
Speed Limits	.53	.50	.45
Tax on CO_2	.48	.42	.37
Parking Restrictions	.19	.32	.33
Electrical Vehicles	.10	.19	.24
Car-Free Zones	.07	.17	.31

Note: Estimate for sample at large is the average number of opponents across the four waves.

TABLE 6. Estimated Share of Strong Opinions in Each Group

	Group		
Issue	Opinion Holders δ_1	Durable Changers δ_3	Vacillating Changers δ_2
Speed Limits	.67	.49	.22
Tax on CO_2	.65	.50	.27
Gas Price Increase	.68	.57	.25
Electrical Vehicles	.57	.23	.20
Car-Free Zones	.68	.37	.26
Parking Restrictions	.61	.36	.23

| Issue | Group | | |
	Opinion Holders	Vacillating Changers	Durable Changers
Speed Limits	0.35 (0.34, 0.36)	1.03 (0.97, 1.09)	1.35 (0.89, 1.75)
Tax on CO_2	0.36 (0.35, 0.37)	1.07 (1.03, 1.11)	1.21 (1.08, 1.33)
Gas Price Increase	0.37 (0.36, 0.38)	1.09 (1.06, 1.12)	1.31 (1.21, 1.41)
Electric Vehicles	0.41 (0.40, 0.42)	0.88 (0.84, 0.93)	1.18 (0.95, 1.36)
Car-Free Zones	0.35 (0.34, 0.35)	1.03 (0.97, 1.09)	1.35 (0.89, 1.75)
Parking Restrictions	0.39 (0.37, 0.40)	1.08 (1.04, 1.12)	1.32 (1.15, 1.52)

TABLE 7. Differences in Average Individual-Level Response Variability across Groups

Note: Estimates and 95% intervals.

Model Simplifications

In order to test two of our most basic assumptions—the existence of three versus two groups and the nonequal chance of disagreeing versus agreeing with an issue among vacillating changers—two alternatives to the standard model were also fit.

1. Constrained model. This model imposes the constraint implied by Converse's black-and-white model, discussed on pages 402–3.

$$\frac{1 - \varphi_2}{2} = Pr(\text{mildly agree or strongly agree})$$
$$= Pr(\text{mildly disagree or strongly disagree}). \quad (1)$$

Therefore, in this model, α_2, the probability that a vacillating changer disagrees with an issue, need not be defined as a separate parameter (it has a one-to-one relationship with φ_2).

2. Two-group model. This model includes only the opinion holders, and the vacillating changers and uses the same parameterization for these groups as described on pages 401–4, except that it also imposes the Converse constraint formalized in equation 1.

The three models range in generality from least to most, in the following order: two-group model, constrained model, unconstrained (standard) model. Since these models are nested, it is sufficient to perform comparisons between contiguous models in this hierarchy (e.g., if the constrained fits better than the two-group, and the unconstrained fits better than the constrained, then we know that the unconstrained fits better than the two-group). Our checks confirm that the unconstrained model fits better than the two-group model.

Comparisons between the constrained model and the two-group model highlight the evidence for the existence of the durable changers group, given the existence of the other two groups. Two types of statistical diagnostics—posterior predictive checks (Gelman, Meng, and Stern 1996; Rubin 1984) and Bayes factors (Kass and Raftery 1995)—were used. For five out of the six issues, the statistical checks strongly support the three-group model over the two-group model. For car-free zones, the policy with the smallest estimated

average percentage of durable changers (.02), the results from these checks are inconclusive (for more details, see Appendix B).

Comparisons between the unconstrained model and the constrained model can be used to examine the evidence for the black-and-white model's definition of the vacillating changers group. If this constraint does not appear to fit the data adequately, then there is support for the theory that the behavior of this group is not truly random in choosing between agree (mildly and strongly) and disagree (mildly and strongly) responses. Both Bayes factors and posterior predictive checks provide evidence of the superior fit of the unconstrained model (for more details, see Appendix B).

The only issue for which the constraint appears reasonable is the gas price increase. In all the other cases the imposed constraint does not appear to be consistent with the data, which means that we cannot presuppose purely random (agree/disagree) behavior on the part of the vacillating changers. Even in the case of the price increase, the inconsistent result does not necessarily imply such random behavior on the part of the vacillating changers. We have estimates of φ_2 and α_2 in the unconstrained model of .07 and .45, respectively. This suggests that the estimated probability of agreeing (mildly or strongly) is .48, which is nearly equal to .45. It could just be that these proportions reflect the "true" opinion distribution of the moment among the vacillating changers with respect to a substantial increase in the price of gas.

Several other posterior predictive checks were performed (including a more global check using the log-likelihood statistic), and indicate good fit for the unconstrained three-group model (for more details, see Appendix B). It is probable that none of these models is detailed enough to capture the subtleties in opinion change during this period. The data were insufficient, however, to support the more complicated and highly parameterized models that we attempted to fit.

CONCLUSION

We conclude from the statistical checks and the substantive results that our three-group model is highly plausible. We have shown that finite mixture models provide a framework that allows reasonable estimates

of proportions of stable opinion holders, vacillating changers, and durable changers. According to our estimates, on average, between 37% and 58% of the Swiss sample have stable opinions about the policy measures for dealing with pollution by private cars. These figures compare favorably to those of Converse (1970, 176), who calculated that, in the case of the attitude items fitting the black-and-white model, 80% of the responses "represented confessions of 'no opinion' or statistically random responses." Our procedure confirms the impression of a comparatively high level of stability, which is also provided by a simple inspection of the correlation matrices shown in Table 2. But even in this case of comparatively high issue-specific stability, there are large shares of vacillating changers with unstable opinions who are subject to the influence of political campaigns and major events. Our results suggest that, short of major events, durable changes in individual opinions occur only rarely. Most change is likely to consist of short-term reactions to external stimuli.

The confirmatory results for our three-group model lead us to think the measurement-error model is not correct. According to our model, the massive response instability in all opinion data is not likely to be due to measurement error; instead, the cause is a more or less large group in the population with unstable opinions. In its classic form, the measurement-error model essentially assumes only one group of respondents, all of whom are characterized by the same measurement error. For example, Achen (1975, 1227ff.) did not find any evidence that different groups of people have different sizes of measurement errors. Most important, the well informed and the politically interested do not have substantially smaller measurement errors than the unsophisticated and uninterested. Accordingly, Achen concluded that measurement error is not primarily the fault of the respondents but of the instruments.[12]

But in our model we see sizable differences in response variation, which implies that this variation is likely to be a combination of both measurement error and opinion instability. This is consistent with earlier evidence presented by Kriesi (2000), who analyzed the variation in measurement error by applying separate measurement-error models of longitudinal opinion response data to various subgroups of respondents, defined by political awareness (political interest or political sophistication), strength of opinions, and other characteristics. Although awareness appears to be irrelevant for the size of the measurement error, some of the other characteristics (especially opinion strength) have strong (i.e., significant) effects.

Our three-group model provides considerable support for Zaller's model. The finding of the existence of a large group of people with unstable opinions is compatible with Zaller's notion of widespread ambivalence. This result would, of course, also be anticipated in view of Converse's claim about the widespread

existence of nonattitudes. The fact that the responses of the unstable group demonstrate different patterns for the constraining versus the unconstraining issues, however, seems contrary to Converse's "nonattitudes" thesis regarding the vacillating changers and supportive of Zaller's interpretation of ambivalence or uncertainty. Moreover, the fact that the vacillating changers generally also have much weaker opinions than opinion holders is consistent with Zaller's notion of ambivalence. But our results are not entirely compatible with the Zaller model. Because we find a large group of stable opinion holders as well as a much smaller group who either durably change opinion or form a durable opinion for the first time, Zaller's claim about the omnipresence of ambivalence seems overdrawn, unless one would want to define changes on the same side of an issue as ambivalence. We have included such small changes as part of stable opinion holding (Figure 1). Based on that decision and the resultant findings, we would argue that there is more structure in public opinion than Zaller assumes.

Our model takes an intermediary position between that of Zaller and Converse's black-and-white model, and it provides relatively little support for the measurement-error model and its claim that all respondents are alike in having stable opinions. On the one hand, we find respondents with stable, structured opinions who correspond to Converse's (1964, 242) " 'hard core' of opinion on a given issue, which is well crystallized and perfectly stable over time." On the other hand, we find respondents with unstable opinions whose response behavior corresponds to the Zaller model. In addition, by introducing the group of durable changers, our model leaves room for what Converse considered meaningful change of opinion, or "conversion," as a result of the public debate. We would suggest, too, that at least some of the change among vacillating changers is in reaction to the public debate. In order to keep the number of parameters within manageable proportions, however, we have made no room in the present model for meaningful reactions to the public debate on the part of vacillating changers.

Our assumption of constant probabilities of disagreeing or agreeing with an issue is probably too restrictive. As Page and Shapiro (1992) have shown, changes in public opinion on the aggregate level are not capricious, whimsical, or inexplicable; they follow clear principles and patterns. Public opinion nearly always changes in reasonable ways, that is, it responds to public events, to changes in reality that affect the consequences of policy alternatives, and to new information conveyed through the mass media. Overall support for measures to decrease pollution closely followed the flux of information provided to the respondents in our survey: Support generally declined after the first period (with the exception of electric vehicles), during which the public debate had a negative tone with regard to the fiscal policies in particular, and it generally became more favorable during the last two periods (with the exception of parking restrictions and electric vehicles between periods two and three), when the information provided was more balanced.

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[12] This, perhaps, is not surprising given that the measurement errors used in his analysis were obtained from a model that did not allow them to vary across different types of individuals.

Because the group of durable changers is very small (Table 3), the vacillating changers most likely contributed to these aggregate trends. Compared to the sizable proportions of the vacillating group, however, the changes in the overall issue-specific distributions presented in Table 1 are quite limited. This suggests that most of the vacillating changers' opinions oscillate, for reasons that are difficult to trace systematically, around this group's issue-specific expected value.

In the final analysis, how sure can we be that there is one large group that is perfectly stable and another that vacillates a great deal? If one accepts our model, and given our definitions of perfect stability (which allows for slight degrees of variation), one can be fairly certain that these groups are as large as our findings suggest. We leave room, however, for the possibility that others may interpret our findings differently or that in the future someone may build an alternative model that fits the data better. As a final cautionary note, we should reemphasize that the empirical sizes of the three groups not only depend on the parameters of the model but also are issue-, period-, and context-specific. That is, they also depend on the characteristics of the issues in question, on the characteristics of the issue-specific public debates during the period of study, and on the characteristics of the political system in which the public opinion is measured.

APPENDIX A

Question Wording (French Original)

Dans les questions qui suivent, je vais vous demander votre opinion sur une série de mesures qui sont actuellement discutées en Suisse. Pour chacune d'entre elles, vous me direz si vous êtes fortement contre, plutôt contre, plutôt pour, ou fortement pour.

Q1. Que pensez-vous de la limitation systematique de la vitesse maximale sur les autoroutes à 100 km/h?

Fortement contre
Plutôt contre
Plutôt pour
Fortement pour
Ne sais pas

Q2. Actuellement, on discute aussi de la fermeture des centre-villes, c'est-à-dire de zones commerçantes, à la circulation automobile. Ainsi, seuls les transports publics auraient le droit de circuler au centre-ville. Pour les résidents, on pourrait faire des exceptions, comme par exemple des autorisations de circuler à certaines heures. Que pensez-vous de la fermeture des centre-villes à la circulation automobile?

Fortement contre
Plutôt contre
Plutôt pour
Fortement pour
Ne sais pas

Q3. Certains experts prévoient des mesures concernant le stationnement en ville. Ils proposent l'augmentation des tarifs de stationnement à 2 francs l'heure au centre-ville et l'introduction généralisés de zones bleues dans toute la ville. Pour les résidents, on prendrait alors des mesures d'exception. Que pensez-vous de telles limitations du stationnement en ville?

Fortement contre
Plutôt contre
Plutôt pour
Fortement pour
Ne sais pas

Q4. Il est actuellement discuté d'introduire une taxe sur le CO_2 qui serait perçue sur tous les carburants. L'objectif de cette taxe est d'inciter chacun à consommer moins d'énergie. Pour l'essence, on discute de la perception d'une taxe de 10 centimes par litre. Les revenus de cette taxe seraient redistribués à la population: cela veut dire que chaque contribuable recevrait environ 60 francs par an en retour. Quelle est votre opinion sur une telle taxe sur le CO_2.

Fortement contre
Plutôt contre
Plutôt pour
Fortement pour
Ne sais pas

Q5. Certains experts prévoient une augmentation graduelle du prix de l'essence jusqu'à 2 francs par litre en l'an 2000 selon le même principe que la taxe sur le CO_2. Les bénéfices récoltés seraient donc redistribués à la population, mais on restituerait une somme d'environ 560 francs par an et par contribuable. Que pensez-vous d'une telle augmentation graduelle du prix de l'essence à 2 francs jusqu'en l'an 2000?

Fortement contre
Plutôt contre
Plutôt pour
Fortement pour
Ne sais pas

Q6. En remplacement des traditionelles voitures à essence, on parle aussi de l'introduction de mini-véhicules électriques. Qu'en pensez-vous?

Fortement contre
Plutôt contre
Plutôt pour
Fortement pour
Ne sais pas

Question Wording (English Translation)

With the following questions I shall ask for your opinion about a series of measures which are currently discussed in Switzerland. For each one of them, tell me whether you are strongly against, rather against, rather in favor, or strongly in favor.

Q1. What do you think about the systematic limitation of the maximum speed on highways at 100 km/h?

Strongly against
Rather against
Rather in favor
Strongly in favor
Don't know

Q2. Currently under discussion is the closing down of the city centers, i.e., the commercial areas, for car traffic. Thus, only public transportation would have access to the city centers. For the residents, one could make exceptions, such as allowing them access during certain hours. What do you think about closing down the city centers for car traffic?

Strongly against
Rather against
Rather in favor
Strongly in favor
Don't know

Q3. Certain experts talk about measures concerning parking in cities. They propose to increase the price for parking to 2 francs per hour in the city centers and to extend the blue zones throughout cities. For residents, one would again make exceptions. What do you think about such restrictions on parking in cities?

Strongly against
Rather against
Rather in favor
Strongly in favor
Don't know

Q4. Currently under discussion as well is the introduction of a tax on CO_2, which would be imposed on all types of fuel. The goal of such a tax is to encourage everyone to consume less energy. For gas, there is discussion about the introduction of a tax of 10 centimes per litre. The revenue raised by this tax would be redistributed to the population. This means that each taxpayer would receive about 60 francs per year in return. What is your opinion about such a tax on CO_2?

Strongly against
Rather against
Rather in favor
Strongly in favor
Don't know

Q5. Certain experts discuss a gradual increase of the price of gas up to 2 francs per litre until the year 2000, based on the same principle as the tax on CO_2. The revenue raised would again be redistributed to the population, but this time each taxpayer would receive a sum of about 560 francs per year. What do you think of such a gradual increase of the price of gas up to 2 francs until the year 2000?

Strongly against
Rather against
Rather in favor
Strongly in favor
Don't know

Q6. To replace traditional cars that use gas, one also talks about the introduction of small electrical vehicles. What do you think about this?

Strongly against
Rather against
Rather in favor
Strongly in favor
Don't know

APPENDIX B

Posterior Predictive Checks

Posterior predictive checks (Gelman, Meng, and Stern 1996; Rubin 1984) allow us to investigate the adequacy of specific aspects of a given model. The idea is simple. We choose a statistic that measures a facet of the model of interest. Then we generate many data sets, assuming our fitted model is true, and for each record the value of the statistic. Adequacy is assessed by comparing the observed value of our chosen statistic (i.e., the value for the real data) to the histogram of

the recorded statistics. If there are few values of recorded statistics from our generated data sets that are more extreme than the observed value, this is an indication that our model is adequately addressing the aspect of the model that the statistic measures.

If the two-group model were adequate, and if we chose a statistic that reflects behavior attributable to the third group, then the observed value of this statistic would look like the values recorded from data sets generated under the two-group model. The statistic we chose was the percentage of those who would be classified as durable changers if we were to fit a three-group model among all those who change sides exactly once. For all questions except car-free zones, at most 2% of the values generated under the two-group model were more extreme than the observed value of this statistic. This demonstrates a weakness in the two-group model. For car-free zones the generated values were more extreme than the observed statistic 12% of the time.

In order to compare the constrained to the unconstrained model, we use a statistic that reflects the assumption of the constrained model that vacillating changers are just as likely to agree as to disagree with an issue, namely, the difference between the estimates of these two probabilities. If we generate data under the constrained model and calculate this statistic for each data set, we obtain a distribution of reasonable values of this statistic if the assumption of the constraint holds. When we compare the observed value of this statistic to this generated (null) distribution we find that it falls entirely beyond the range of this distribution for all items except the gas prices increase, for which 18% of the generated values are more extreme.

Posterior predictive checks were also performed to evaluate more general goodness of fit for the unconstrained three-group model. Log-likelihood statistics were used for a general check of fit and provided no evidence against the fit of the model. In addition, several statistics were used that measure the frequency of some of the most popular response patterns in the observed data. It will be difficult for our model, which has only fifteen parameters, to replicate specific patterns, given that there are 625 possible patterns overall. Consequently, statistics that are restricted to measuring the frequency of just the two or three most popular patterns are not very successful. Comparability increased, however, when the breadth of the patterns was expanded to include a randomly chosen four from the twenty most popular patterns, and generated values were more extreme than observed values from 0 to 32% of the time.

Bayes Factors

Bayes factors measure the weight of evidence for one model versus another (Kass and Raftery 1995). Bayes factors are a more global check of one model versus another. We used Kass and Raftery's interpretation of the practical implications of the strength of the evidence provided by various ranges of Bayes factor values against the null model (in this case the two-group model).

Bayes factors that compare the three-group (constrained) versus the two-group model provide strong support for the three-group model. The transformed Bayes factor ($2 \log_e(B)$) for car-free zones (2) is interpreted as positive evidence against the two-group model. For parking restrictions this factor (9) is interpreted as strong evidence against the two-group model, and for speed limits (66), tax on CO_2 (58), gas price increase (77), and electric vehicles (49) it is interpreted as very strong evidence against the two-group model.

Bayes factors that compare the unconstrained with the constrained three-group model provide in most cases quite strong support for the unconstrained model. The transformed Bayes factor for gas price increase was 0, which indicates no preference for either model. The transformed Bayes factor for speed limits (2) is interpreted as positive evidence against the constrained model. The transformed Bayes factor for tax on CO_2 (23), electric vehicles (91), car-free zones (42), and parking restrictions (39) are all interpreted as very strong evidence against the constrained model.

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The Sum of the Parts Can Violate the Whole
DONALD G. SAARI *University of California, Irvine*
KATRI K. SIEBERG *College of William and Mary*

*W*e develop a geometric approach to identify all possible profiles that support specified votes for
separate initiatives or for a bundled bill. This disaggregation allows us to compute the likelihood
of different scenarios describing how voters split over the alternatives and to offer new
interpretations for pairwise voting. The source of the problems—an unanticipated loss of available
information—also explains a variety of other phenomena, such as Simpson's paradox (a statistical paradox
in which the behavior of the "parts" disagrees with that of the "whole") and Arrow's theorem from social
choice.

A t least since the late 1700s, when Condorcet introduced his voting cycles, it has been understood that even if voters vote sincerely over pairs of outcomes, the final conclusion may be supported by very few or even none of them. This troubling phenomenon, often described in terms of shifting majorities (the voters who define the majority shift with the issue), suggests that a basic tool of democracy can subvert the intent of the voters. As such, it is understandable why bothersome versions of this behavior continue to arise in political science. As it will be shown here, the Anscombe (1976) paradox (also see Nurmi 1999), which shows a majority of the voters can be frustrated on a majority of the issues, is a version of the Condorcet cycle. Ostrogorski ([1910] 1970; Nurmi 1999) uses this behavior to question the meaning of the "dominant party"; the choice can change by emphasizing the party liked by most voters for most of its stands on issues, or the party that wins elections over a majority of the issues. In his influential work on voting cycles, Kramer (1977) suggests that this troubling behavior can be avoided with supermajority q rules. (The "stable" choice of the q rule, where q votes are needed to win, depends on the number of issues; see McKelvey 1979, Nakamura 1978, Saari 1997, Schofield 1983, and Slutsky 1979.) In the special setting of "yes-no" elections, Brams, Kilgour, and Zwicker (1998) provide examples from initiatives in California elections.

This article originated with and focuses on conjectures developed by Sieberg while doing the statistical analysis for the Brams, Kilgour, and Zwicker article. For instance, it is reasonable to believe that the oddity whereby the totality of the parts can violate the intent of the whole extends to more general political science behavior. Moreover, the statistical analysis from the California initiatives suggests that shifting majority peculiarities are not isolated anomalies but occur with a reasonable likelihood. (Indeed, out of 28 proposi-

tions on the November 1990 California ballot, the Brams, Kilgour, and Zwicker article reports that not a single voter voted for the winning combination.) In verifying these conjectures, we discover that a common interpretation of what pairwise voting means is incorrect when there are several pairs. Alternative interpretations are offered.

To indicate why this odd behavior should occur in settings other than voting, we treat pairwise voting as a decentralized decision process that attempts to capture the voters' wishes by determining their societal choices for the pairs. If this decentralization causes the pairwise voting problems (it does), then we should anticipate similar oddities whenever other aggregation tools—from probability, statistics, or even strategies for game theory—decentralize and concentrate on disconnected parts.

We illustrate with a statistical example and recall that, during the impeachment process of President Clinton, various groups wished to gauge the level of public support for the three specific proposals advanced. The results of exit polls conducted by CNN and the major networks taken during the November 1998 U.S. elections uniformly reported that American voters by overwhelming percentages opposed (1) impeaching Clinton, (2) censuring him, and (3) imposing a fine. These survey outcomes suggest that most voters wished to excuse the president, but that is misleading, because an overwhelming majority (on that election day) also felt that he should be punished. It is arguable, of course, that this example illustrates the dangers of using poor public opinion methods, that is, these conflicting conclusions may reflect an inappropriate choice of questions. But such an argument further underscores our point that the part-whole conflict extends beyond voting to pose concerns for the proper use of statistical tools. "More appropriate" questions arise by identifying and incorporating issues that connect the parts.

BUNDLED VOTING

At the opposite extreme from voting on individual issues is the gathering of issues into one bill. The advantages of this approach are understood. For instance, it is accepted that logrolling and bundled voting can avoid the shifting majority obstacle by providing

structure to legislation. But, as can most tools, bundled voting can be used in many ways; for example, party leaders may use it to pass undesired bills or pork by bundling them with more popular alternatives. One way to measure the use of this tool is to determine whether the bundled parts form an intended, coherent, connected whole. An analysis of bundled voting requires understanding how legislators can split over component parts of the legislation. To do so, we develop an approach to determine what passage of the bill means relative to the legislators' views about the individual parts. A worrisome outcome occurs, for instance, if most lawmakers disagree with much, or a particular part, of the bill that they just passed. More encouraging conclusions that support a positive intent of logrolling emerge when the disaggregation indicates a balance among the legislators' goals.

A different kind of example is bundled voting that may frustrate the intent of the individual parts. Such a situation, also during the Clinton impeachment proceedings, occurred when White House Counsel Charles Ruff (McLoughlin 1999, 284) claimed in his opening defense statement to the Senate that the impeachment articles were "constitutionally deficient." His argument derived from principles of criminal justice, whereby "lumping multiple offenses together in one charging document [is prohibited because it] creates a risk that a verdict may be based not on a unanimous finding ... but instead may be composed of multiple individual judgments." Because the House bundled multiple alleged offenses into one article, Ruff argued, it was possible that even if two-thirds of the senators would not find Clinton guilty of any individual charge, the necessary two-thirds vote for conviction could emerge if some found him guilty on some charges and others found him guilty on other charges in the bundled indictment article. In this case the worry is not whether the sum of the individual parts violates the whole, but whether support of the whole violates the intent of having a two-thirds vote on each of the individual parts.

Ruff's concern is closely related to the principles of criminal justice. The troubling possibility is that a person may be convicted by unanimous vote even though the intended unanimity (of the jury members) on any specific issue is not assured. Are these highly unlikely situations? With the high thresholds imposed by "unanimity" and a "two-thirds" vote, does not the passage of a bill ensure there is "essentially" the required support? Or is it likely that only a few voters accept any part of the total bill? Our approach for finding all disaggregations of a vote provides a tool to analyze a variety of issues, ranging from "pork" and even to objectives such as logrolling, which can have generally positive outcomes.

SEPARATION LOSES INFORMATION

Beyond explaining these various phenomena, we emphasize why they occur. A main conclusion is that these paradoxical behaviors arise because the separation of inputs into disconnected parts can cause a concomitant loss of available and crucial information. If so, then we also know how to resolve these difficulties: Identify the nature of the lost information and then discover how to reincorporate it into the process. This approach is compatible in spirit with the earlier comments about logrolling and the design of appropriate survey questions.

By identifying and then measuring the lost information, we show that these oddities are not rare, essentially concocted settings but are surprisingly likely. Indeed, our approach makes it apparent that increasing the number of separated parts significantly increases the likelihood of paradoxical conclusions. Similarly, with bundled voting we show, for instance, that Ruff's concerns have merit because it is uncomfortably likely for a bundled bill to pass with a two-thirds or even a unanimous vote, even though only a few voters approve of any individual part. The conflict between disconnected parts and the whole must be taken seriously.

For expositional reasons we emphasize voting, but it must be stressed that with many (if not all) aggregation procedures, odd behavior can occur whenever "parts" are disconnected. Unexpected behavior arises if the decentralization loses important, available information or when aggregations (e.g., bundled voting) connect inappropriate parts. Thus, expect similar examples to arise whether the tools involve game theory, statistics, general decision theory, and so forth. To underscore this point, the following examples explain certain oddities by identifying the information lost by the parts.

Simpson's Paradox

Suppose that in Atlanta and Boston the recovery outcome of groups using an experimental drug is compared with the recovery of control groups. In Atlanta, the experimental group enjoyed a two-thirds recovery rate compared to only one-half for the control group, and in Boston the respective figures were one-third and one-quarter. Although the data from both locales support the experimental drug, the combined data could indicate that the control approach is more successful. In other words, the information from the parts need not predict the behavior for the whole. (For political science implications of Simpson's [1951] paradox see Nurmi [1998, 1999, 2000].)

This reversal occurs when the whole goes beyond the information about the parts to use also the number of subjects in each of the four groups. To illustrate, let d_A^e, d_A^c, d_B^e, and d_B^c denote the number of subjects in the Atlanta and Boston experimental and control groups. Then, $(2/3)d_A^e$ and $(1/3)d_B^e$ are the number of recovered Atlanta and Boston subjects with the experimental treatment. Examples are generated by finding d_j^i values, so that

$$\frac{(2/3)d_A^e + (1/3)d_B^e}{d_A^e + d_B^e} < \frac{(1/2)d_A^c + (1/4)d_B^c}{d_A^c + d_B^c}. \tag{1}$$

To create examples, set the left-hand side equal to any fraction, say, $\frac{9}{24}$, that is between the two recovery fractions of $\frac{1}{3}$ and $\frac{2}{3}$. Solving

$$\frac{(2/3)d_A^e + (1/3)d_B^e}{d_A^e + d_B^e} = \frac{9}{24}$$

leads to $7d_A^e = d_B^e$, so let $d_A^e = 30$, $d_B^e = 210$. An example is created by setting the right-hand side of equation 1 equal to a value larger than $\frac{9}{24}$ and between the two control group recovery values. For instance, choosing $\frac{11}{24}$ leads to $d_A^c = 5d_B^c$, so the values $d_B^c = 8$, $d_A^c = 40$ suffice.

So, the 20 recoveries from the 30 subjects in the Atlanta experimental group has a better record than the 20 recoveries from the 40 subjects in the control group. As is often the case, in Boston, 210 subjects volunteered for the experimental study, while only 8 joined the control group. Here, 70 of the experimental group recovered compared to only 2 from the control group. Using the above analysis, examples as varied as desired can be constructed.

Game Theory; Lotteries

Separate lotteries illustrate how conflicting outcomes can arise in game theory, or even individual decision making. To show that information is lost by considering only disjoint parts, a new strategy, admitted by the whole is identified.

Suppose Paul is a professional gambler who has $70 to wager on a basketball game between the Lakers and the Knicks. Bob is so confident of the Lakers that he offers Paul 2 to 1 odds. (If the Lakers lose, Bob pays $2 for each dollar bet with him.) Later Paul meets Sue; she is sufficiently confident of the Knicks to offer 3 to 1 odds. When considered individually, Paul's strategy and decision about whether and how much to bet depends upon information gathered about the two teams. Whatever choice is made, a bet with Bob and/or with Sue involves an element of randomness and risk.

By considering the two parts as a connected whole, Paul's newly admitted strategy is to bet against both teams in a manner to remove all risk. For instance, betting $40 on the Knicks (with Bob) and $30 on the Lakers (with Sue) eliminates all risk (and demonstrates the radically different nature of the connected game), as Paul is guaranteed a $50 profit no matter which team wins. For success, Paul must be sure that Bob and Sue are unaware of the odds the other is offering.

Arrow's Impossibility Theorem

Arrow's theorem ([1952] 1963), one of the more troubling results in decision theory, provides a third type of difference in structure between the parts and the whole. Because Arrow's independence of irrelevant alternative (IIA) assumption forces the decision process for the societal rankings of the parts—each pair—to be totally disconnected from one another, such a procedure can service a voter with cyclic preferences as well as one with transitive preferences. It

turns out that with a sufficiently heterogeneous society (Saari 1994, 1995, 1998), IIA loses the crucial information that the voters have transitive preferences. As argued in these three references, the Borda count is a way to include the information and resolve the difficulty. As pairwise voting satisfies IIA, versions of the Borda count resolve the problems we examine below.

GEOMETRIC INTERPRETATION

Our geometric approach is designed to explain why decentralizing societal outcomes into disconnected parts loses information and causes paradox. An advantage of the geometry is that it also displays the robust nature of this phenomenon.

It is natural to use a graph to represent the relationships between the inputs and outputs, but there is a serious obstacle; social science problems typically involve so many variables that the many dimensions required by a standard graph would make it impossible to visualize and use. Alternative representations are needed. Economists resolved this difficulty with the Edgeworth box (see any introductory textbook on microeconomics). On the same rectangle, the preferences and initial endowments of two agents for two commodities are simultaneously graphed. This representation allows the group outcomes and properties of, say, the Pareto sets and price equilibria to be geometrically determined and described. The power of the Edgeworth box comes from its clever, simple representation of the independent and dependent variables in a single, lower dimensional setting; it is a form of a graph. Moreover, the Edgeworth box makes it possible to predict what happens with more agents and/or commodities. Our goal is to create a related geometric tool to address the decision concerns of political science.

A particular difficulty in analyzing voting issues is the inability to represent profiles (i.e., a listing of the voters' rankings of the issues) and outcomes in a single sketch. To overcome this difficulty, we extend the geometric approach developed by Saari (1994, 1995) to analyze three and N-alternative elections.

Representing Elections of Pairs

To represent geometrically the outcome for a single pair of alternatives, $\{A, B\}$, we let $X_{A;\{A,B\}}$ be the fraction of the total vote received by A. So, $X_{B,\{A,B\}} = 1 - X_{A,\{A,B\}}$, and the values $X_{A,\{A,B\}} = 1, \frac{1}{2}, 0$ represent, respectively, these outcomes: A wins unanimously, A and B are tied, and B wins unanimously (because A received no votes).

When the election point is plotted on a unit interval, its location represents both the division of voter preferences and the majority vote tally. The support for a particular candidate is graphically represented by the distance from the other candidate's unanimity point. For instance, a point $\frac{3}{8}$ of the way from B to A, as represented by the • in Figure 1a, indicates with 100% certainty that 37.5% of the voters preferred A to B and that A received 37.5% of the vote. Conversely, the

FIGURE 1. Representation Lines and Squares

a. One Pair of Alternatives

b. Two Pairs of Alternatives
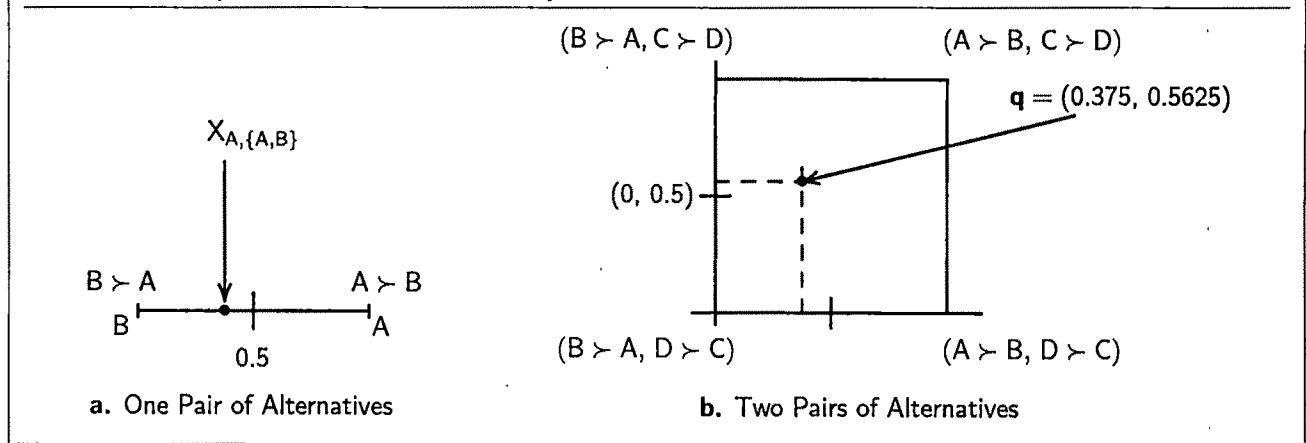
point is ⅝ of the distance from A to B, so B received 62.5% the vote.

The outcomes for the two pairs $\{A, B\}$ and $\{C, D\}$ are simultaneously represented by the • located at the point $(X_{A,\{A,B\}}, X_{C,\{C,D\}})$ in the Figure 1b square. The vertical and horizontal dashed lines indicate each pair's level of support. For instance, the horizontal dashed line is 56.25% of the distance above the axis—where D has a unanimous vote—so C beats D with 56.25% of the vote. Outcomes for $N \geq 2$ pairs are similarly represented with a point in an N-dimensional cube. Although actual geometric representations are impossible to draw for $N \geq 4$, as is true with the Edgeworth box, basic properties of election outcome can be found.

The issue is to determine what this Figure 1b outcome—B beats A (with 62.5% of the vote) and C beats D (with 56.25% of the vote)—means about the voters' preferences. With certainty, 62.5% of the voters prefer $B > A$, and 56.25% prefer $C > D$. But what can we say about the individual voter rankings over both pairs? Does this result mean, for instance, that most voters prefer both $B > A$ and $C > D$? Or is there a shifting majority effect whereby a sizeable majority of the voters is unhappy with at least parts of this total outcome?

To illustrate with numbers, we let the Figure 1b outcomes represent an election held with 80 voters. With certainty, 50 of them prefer $B > A$, and 45 prefer $C > D$, but this could mean that (1) more than half, 45, of these voters prefer both the ($B > A$ and $C > D$) outcomes, 5 show partial support by preferring ($B > A$ but $D > C$), and the remaining 30 are opposed to both outcomes, or (2) only 15 voters approve of both outcomes, whereas the sizeable number of 65 voters disagree with parts of the combined outcome, because 35 prefer ($B > A$ but $D > C$) and the remaining 30 prefer ($C > D$ but $A > B$). Both choices generate the same combined election outcome, but they admit different interpretations. For instance, the first scenario indicates support for the joint outcome, but the second creates doubt about its appropriateness because more than 80% of the voters disapprove of parts of the societal outcome.

Why does it matter which profile represents the voters? Imagine 80 legislators trying to cope with a rise in the number of school children. Suppose the first vote is between A = (raise teachers' salaries) and B = (leave the salaries at the current level). The second ballot imposes a choice between C = (increase the size of classes) and D = (keep the same class size).

Suppose a distinct minority of the legislators, 15, believe that the teachers can cope with larger classes on their current salary, so they choose B on the first vote and C on the second. The remaining legislators wish to do something, but none of them wants the dual expense of higher salaries and the same class size. Suppose 30 of the legislators favor compensating teachers with higher salaries for overcrowded classrooms, so they vote for A and C. The remaining 35 believe the current class size is already too large. Rather than raise salaries, they prefer to find extra classrooms for the additional students. They choose B on the first vote and D on the second.

Although more than 80% (65/80) of these legislators would like to resolve the problem by helping the teachers, their goal is frustrated as B and C win with, respectively, votes of 50/80 and 45/80; the teachers are forced to cope with larger classes without added compensation. Without a disaggregation of the vote, this outcome may appear to be the wish of most legislators. In other words, there is no way to distinguish between these two scenarios, which have a different intent and offer distinctly different messages.

Profile Line

We now develop a geometric representation of voter preferences and election outcomes on the same diagram, Figure 2. Two pairs of alternatives define four voter types according to how they rank each pair. Let $v(A, C)$ be the fraction of all voters who prefer $\{A > B$ and $C > D\}$; similar definitions hold for $v(A, D)$, $v(B, C)$, $v(B, D)$. Thus, a profile **p** becomes the four-vector

$$\mathbf{p} = (v(A, C), v(A, D), v(B, C), v(B, D)).$$

FIGURE 2. Profile Representations

(B ≻ A, C ≻ D)   (A ≻ B, C ≻ D)

(0, 0.9)

q   p_U

(0, 0.3)

p_D

$X_{A,\{A,B\}}$

(B ≻ A, D ≻ C)   (A ≻ B, D ≻ C)

a. Profile line   b. All supporting profiles
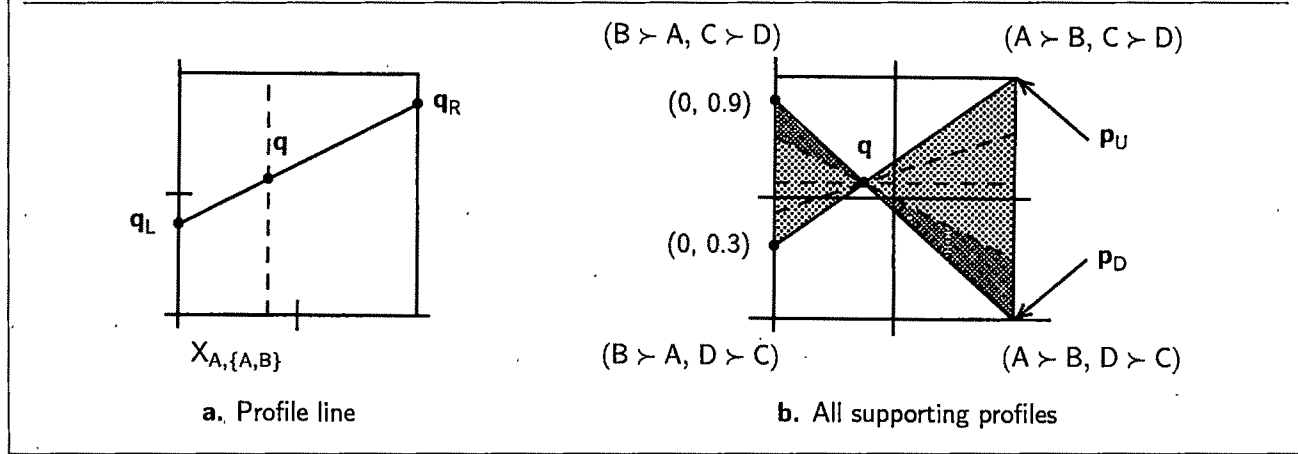
The above two profiles are, respectively, $\mathbf{p}_1 = (0, {}^{30}\!/_{80}, {}^{45}\!/_{80}, {}^{5}\!/_{80})$ and $\mathbf{p}_2 = ({}^{30}\!/_{80}, 0, {}^{15}\!/_{80}, {}^{35}\!/_{80})$.

To identify $\mathbf{p}$ with the pairwise outcomes $\mathbf{q} = (q_1, q_2)$, note that since $X_{A,\{A,B\}}$ is the fraction of all voters who prefer $A > B$, we have that $X_{A,\{A,B\}} = v(A, C) + v(A, D)$. Thus, the coordinates of $\mathbf{q}$ in Figure 1b are

$$\mathbf{q} = (X_{A,\{A,B\}}, X_{C,\{C,D\}}) = (v(A, C)$$
$$+ v(A, D), v(A, C) + v(B, C)). \quad (2)$$

To motivate what follows, we treat the $\{A, B\}$ rankings as dividing the voters into two parties. We further divide the "leftists," defined by their $B > A$ belief, according to their preferences over C and D; this division is given by the number of leftists with preferences associated with the two vertices on the left edge of the Figure 1b square. Similarly, the "rightists," with their $A > B$ preference, are subdivided by their C and D preferences as represented by the two vertices on the square's right edge.

The division of each party according to its $\{C, D\}$ preferences can be shown by using a Figure 1a type of representation. To do so, define and then plot the points q_L and q_R on the respective vertical edges. For instance, by letting q_R represent the fraction of the rightists who prefer $C > D$, the division is given by $v(A, C)$ (the fraction of voters who prefer $A > B$—a ranking needed to be a rightist—and $C > D$) divided by $X_{A,\{A,B\}} = [v(A, D) + v(A, C)]$ (the fraction of all voters who are rightist). Thus, q_R is at height $v(A, C)/[v(A, D) + v(A, C)]$ on the representation square's right edge. Each point's coordinates, as plotted on the appropriate edges of the Figure 2a square, are

$$q_L = \left(0, \frac{v(B, C)}{v(B, C) + v(B, D)}\right),$$

$$q_R = \left(1, \frac{v(A, C)}{v(A, C) + v(A, D)}\right), \quad (3)$$

or, equivalently,

$$q_L = \left(0, \frac{v(B, C)}{1 - X_{A,\{A,B\}}}\right), \quad q_R = \left(1, \frac{v(A, C)}{X_{A,\{A,B\}}}\right). \quad (4)$$

Intuitively, the combined $\mathbf{q}$ outcome should involve the relative strength of the rightist and leftist parties. This happens because, according to equation 4,

$$\mathbf{q} = (1 - X_{A,\{A,B\}})q_L + X_{A,\{A,B\}}q_R. \quad (5)$$

But equation 5 is the equation of a straight line, so the combined election outcome $\mathbf{q}$ is found by plotting the q_L and q_R outcomes for the left-hand and right-hand groups. Next, connect these points with a straight line. The actual election outcome $\mathbf{q}$ is the intersection of this line with the vertical line defined by $x = X_{A,\{A,B\}}$ (the dashed line in Figure 2a). In this manner, a profile is uniquely represented by a line and the point $\mathbf{q}$ on the line.

A specified profile $\mathbf{p}$ uniquely determines $q_L = (0, y_L)$, $q_R = (1, y_R)$, $\mathbf{q}$, and the line. Conversely, if a line is specified, then the defining profile can be recovered by use of algebra. As the specified q_L, q_R, and $\mathbf{q}$ values of a line uniquely determine a profile $\mathbf{p}$, we have the following definition.

DEFINITION 1. *A profile line with distinguished point* $\mathbf{q}$ *in a representation square is given by a straight line connecting points on the vertical edges and a distinguished point,* $\mathbf{q}$, *on the line. The points on the vertical edges represent how these two sets of voters rank the alternatives along the vertical edges. The distinguished point* $\mathbf{q}$ *is determined by the fraction of all voters who prefer one or the other of the two alternatives represented by the horizontal axis;* $\mathbf{q}$ *represents the election outcomes.*

To summarize, a profile with $\mathbf{q}$ as an outcome defines a profile line with distinguished point $\mathbf{q}$. Conversely, *each line that connects opposite edges and passes through* $\mathbf{q}$ *defines a profile line (i.e., it uniquely defines a profile) with outcome* $\mathbf{q}$. By using this representation, we now can identify all possible profiles that support a specified election outcome $\mathbf{q}$. This set of all possible disaggre-

TABLE 1. Extreme Profiles

Profile	Approve All	Approve Only $B > A$	Approve Only $C > D$	Approve None
p_D	56.25%	6.25%	0	37.5%
p_U	18.75%	43.75%	37.5%	0

gated outcomes is the cone of all possible lines that connect the vertical edges and pass through **q**.

To analyze the cone of profile lines represented by Figure 2b, we start with the two extreme profiles; they are the two lines that include a vertex on the right edge. As two points (**q** and the appropriate vertex) define a line, the equations for the two extreme profile lines are

$$y = \frac{1 - q_2}{1 - q_1} x + \frac{q_2 - q_1}{1 - q_1} \text{ and } y = \frac{q_2}{1 - q_1}(-x + 1).$$

(6)

A word of caution is necessary. The two extreme profile lines in Figure 2b involve vertices on the right edge only because this particular **q** satisfies

$$q_1 \leq 1/2 \left(\text{so } \mathbf{q} \text{ is to the left of } x = \frac{1}{2} \right),$$

and $q_1 + q_2 \leq 1, q_2 \geq q_1$ (7)

(the last conditions require the two vertices on the right edge to define the extreme profiles). If **q** were closer to the top edge, then an extreme profile line might involve a top left vertex rather than the bottom right vertex. Unless rightist and leftist define actual coalitions, however, these terms are just artifacts that assist the analysis. By changing which pair is on the vertical or horizontal axis, and which edge has lines ending in a vertex, all choices of **q** can be converted into a form that satisfies the equation 7 conditions with a Figure 2b type representation. (In what follows, we purposely vary the **q** location to illustrate how to handle other settings.)

The two extreme profiles supporting **q** in Figure 2b are p_D and p_U, which are, respectively, the extreme downward and upward sloping profile lines that end in a vertex. (For the earlier 80-voter illustration, p_D and p_U are, respectively, geometric representations of the p_1 and p_2.) How do we recover the profile representation from p_D? As ⅝ of the voters are leftist and as the y_L component on the left edge is 0.9, it follows that $0.9 \times ⅝ = 0.5625$, or 56.25% of all voters (all leftists) agree with both outcomes. In this algebraic manner, the extreme profiles are characterized relative to the joint outcome as shown in Table 1. Notice how Table 1 shows that the profiles supporting **q** vary in their support for both outcomes (the $v(B, C)$ term) from only 18.75% of all voters (with p_U) to 56.25% (with p_D). The levels of disagreement with at least one outcome ranges from 43.25% to a surprisingly large 81.25% of the voters. Complete disapproval (the $v(A, D)$ term) ranges from 0% to 37.5%.

The profiles that indicate a lack of support for the final combined conclusion underscore a cost of consid-

ering separate, disjoint parts: the potential loss of information about what the voters want for the total package. Instead of knowing the actual **p** = $(v(A, C), v(A, D), v(B, C), v(B, D))$, we merely know the outcome **q**. The problem, as in the school example, is that **q** can be the outcome for a surprisingly large variety of profiles with contrary interpretations. Thus, by ignoring the relevant information about how the voters connect the pairs, we introduce the danger that a majority of the voters do not embrace the combined outcomes.

With two pairs, however, there always must be voters who approve of both outcomes. For a proof, notice that if **q** is in the upper left quarter, then no supporting profile line can meet the (0, 0) vertex. This geometry requires $y_L > 0$ for all profile lines, so it means that any supporting profile must include voters who approve of both election conclusions.

Likelihood Estimates

Analyzing a single profile can, at best, illustrate a particular unusual behavior. The p_U profile, for instance, proves that a substantial percentage of the voters can disapprove of parts of the **q** outcome even though each victory is by a substantial vote. A more ambitious and crucial question is to understand whether examples such as p_U are isolated oddities or identify a troubling behavior that must be addressed. We now tackle this more general concern.

With a single $\{A, B\}$ election (which could be a "yes-no" election, or a choice between two alternatives), it is certain that the winning alternative is supported by at least 50% of the voters. To understand what replaces this certainty assertion when there are two pairs, we determine the likelihood that at least 50% of all voters agree with both **q** outcomes of Figure 2a and b.

The first step is to find all profiles that satisfy the 50% approval level. Since ⅝ of the voters are leftist (with the winning $B > A$ outcome), at least half of all voters will approve both outcomes only if $y_L \times ⅝ \geq \frac{1}{2}$, or if $y_L \geq 0.8$ of the leftists prefer $C > D$. To find all profiles that satisfy this condition, we draw the profile line defined by the $y_L = 0.8$ value on the left edge and **q**. As all profile lines with $y_L \geq 0.8$ have the desired 50% approval behavior, these profiles are represented by the profile lines in the heavier shaded region of Figure 2b. Notice that by computing where the defining line with left endpoint $y = 0.8$ hits the right edge, it follows that the cost of achieving this level of agreement is that at least 31.25% of the voters directly oppose both outcomes.

It should be noted that not all endpoints define profile lines. In the 80-voter example, as 30 of the voters are rightist (the smaller of 30 and 50), divide the regions on the right and left edges into 30 equally distributed regions, or 31 equally distributed points, starting at the lower point of each region and ending at the upper one. Each point defines a profile. If there are 8,000 voters, then each edge is divided into 3,001 points. As the number of voters increases, the better the continuum represents the possible voter views.

The likelihood estimates of voter satisfaction, or dissatisfaction, with the joint outcome now can be extracted from the figure. These values, however, depend upon assumptions made about the voters. We illustrate the approach with three scenarios; many others are possible.

Strict Partisan Vote. If the voters adopt a strict party line, then the party's vote is unanimous for each pair. Here, the profile line must start from a vertex on the appropriate edge. This means for our example that leftists do *not* enforce a strict party vote (as no profile line with the **q** outcome ends at a vertex on the left edge), and with certainty the profile is described by either p_U or p_D. Thus, with certainty, either more than half of all voters prefer the joint outcome (when p_D is the profile), or a sizeable percentage of the voters disapprove of parts of the **q** outcome (with profile p_U).

Uniform Distribution. Since the Figure 2b geometry captures all profiles supporting **q**, it is reasonable to assume that the likelihood of this 50% approval property is the ratio of the number of profiles with the desired property divided by number of profiles with the **q** outcome. This suggests use of the ratio of the heavily shaded area (the profiles satisfying this 50% property) relative to the fully shaded area (the set of profiles supporting **q**). With this assumption, the Figure 2b geometry (the darkly shaded region is much smaller than the shaded region) shows that a 50% approval of both outcomes is fairly unlikely. That is, with this assumption, likelihood estimates of various behaviors follow almost directly from the geometry. (This assertion holds even when the continuum is replaced by an equally distributed number of points for leftists and rightists.)

A simple way to compute this likelihood (see the Appendix) is to use one of the edges. The likelihood equals the ratio of the length of the heavily shaded region on the edge to the length of the fully shaded region on the same edge. Thus, the likelihood of a specified behavior reduces to computing the length of the segment that defines the behavior divided by the length of the segment of all possible outcomes.

To illustrate this computation with the Figure 2b example, we note that the segment of endpoints on the left edge satisfying the 50% agreement property is $0.8 \leq y \leq 0.9$, so it has length $0.9 - 0.8 = 0.1$. The segment of left endpoints where the profile has this **q** outcome is $0.3 \leq y \leq 0.9$; it has length $0.9 - 0.3 = 0.6$. The ratio of these lengths, only ⅙, defines the proportion of profiles that support this 50% approval property

of both outcomes. Consequently, under the assumption that each profile line is equally likely, we reach the surprising conclusion that, although each Figure 2b pairwise election is won with a strong majority vote, the likelihood that more than half the voters are dissatisfied with at least part of the outcome is the surprisingly large ⅚, or 83%.

As further illustration of the geometric approach, we determine the likelihood that at least one-quarter of the voters dislike both outcomes. These voters are represented by the $v(A, D)$ value, or the vertex in the lower right-hand edge, so our interest is in profiles whose right edge point is below height y_R, where $(1 - y_R) \times \frac{3}{8} \geq \frac{1}{4}$, or $y_R \leq \frac{1}{3}$. (The $1 - y_R$ value determines the distance from the C unanimity vote to the D unanimity vote.) It follows immediately from Figure 2b that (assuming all profile lines are equally likely), with probability of ⅓, at least one-quarter of all voters disagree with both outcomes.

The geometry, then, allows us to determine quickly the likelihood of various behaviors. For instance, a joint outcome in which each pair barely wins a majority vote has **q** near the completely tied point of (½, ½). The associated profile cone comes close to being described by lines connecting diametrically opposite vertices. In turn, those profile lines for which at least 50% of the voters approve of both outcomes come close to requiring an almost unanimous support from the leftists. This means that the line segment of a heavily shaded region must be very small, and the segment with a shaded region is nearly the full edge. The geometry, then, immediately proves that such support is highly unlikely. By using this computational approach, we obtain the following more general assertion.

THEOREM 1. *For two pairs, suppose the winning alternative wins with the majority m_j of the vote, $m_j > \frac{1}{2}$, $j = 1, 2$, where $m_1 \geq m_2$. Assume all profiles, as represented by their endpoints on an edge, are equally likely. The likelihood that at least the fraction α of all voters prefer both outcomes is the smaller of unity or*

$$Prob(\alpha) = \max\left(\frac{m_2 - \alpha}{1 - m_1}, 0\right). \tag{8}$$

Similarly, the likelihood that at least β of all voters dislike both outcomes is

$$Prob(\beta) = \max\left(\frac{1 - m_1 - \beta}{1 - m_1}, 0\right). \tag{9}$$

To illustrate this result with numbers, we recall that with one pair it is certain that the winning alternative enjoys at least 50% of the vote. To find the parallel conclusion for two pairs—that is, conditions in which we can say with certainty that at least 50% of the voters prefer both outcomes—then (according to equation 8, with $\alpha = \frac{1}{2}$ and $Prob(\alpha)$ set equal to unity) it must be that $m_1 + m_2 \geq \frac{3}{2}$. Since $m_1 \geq m_2$, this conclusion requires the first election to be won with at least a 75% vote, and the second victory must have nearly as strong a vote; "certainty" for two pairs requires surprisingly

strong votes. Using more commonly observed election outcomes, such as $m_1 = 0.52$ and $m_2 = 0.51$, equation 8 establishes that it is highly unlikely for even half the voters ($\alpha = 0.5$) to approve of both outcomes. (The likelihood is only $0.01/0.48 \approx 0.02$). But there is about a 54% chance that at least one-quarter ($\alpha = \frac{1}{4}$) of all voters approve both outcomes. These are disturbing results.

By setting $\text{Prob}(\alpha) = 1$ in equation 8, we obtain the interesting relationship that, with certainty, the small proportion of $\alpha = m_1 + m_2 - 1$ of all voters approve of both outcomes. Using the Figure 1b example, this means that, with certainty, $0.625 + 0.5625 - 1 = 0.1875$ of the voters prefer both outcomes. But for $m_1 = 0.52$, $m_2 = 0.51$, all we can say with certainty is that at least 3% of the voters prefer both outcomes. Similar results using equation 9 determine the fraction of all voters who do not like either result. Using $m_1 = 0.52$, $m_2 = 0.51$, equation 9 shows the likelihood that at least β of all voters disapprove of both outcomes is $[0.48 - \beta]/0.48$. For instance, there is about a 50% chance that 24% of the voters disapprove of both outcomes.

Normal Type Distributions. The results change dramatically and become much more troubling if the distribution of profile lines uses the binomial or normal distributions. For instance, with the 80-voter example, if each rightist is equally likely to prefer C or D, then the likelihood that none of these voters prefer C is $(\frac{1}{2})^{30}$, but the likelihood that 10 of them prefer C is $(^{30}\!/_{10})(\frac{1}{2})^{30}$, or more than 30 million times more likely.

Because the binomial and normal distributions concentrate most of the probability around the expected value, these distributions make it highly unlikely that a sizeable number of voters approve of the combined outcome. For instance, using the binomial distribution in which the 400 leftists have probability 0.6 of voting for C over D (the 0.6 value is the midpoint of the region of the left edge), standard computations with the central limit theorem prove that the 50% approval outcome occurs with probability essentially zero. (It is several standard deviations above the mean of 0.6.) Such distribution assumptions place a high emphasis on profile lines near the mean.

Interpreting Pairwise Elections over Several Pairs

When two pairs are voted upon separately, it may be that only a few voters approve of both outcomes; these are the profiles that create the paradoxes described in the literature. We adopt a radically different interpretation. We argue that these are "paradoxes" only if we incorrectly believe that the pairwise votes should accurately reflect the views of the voters over *all outcomes*. This is not the case; these difficulties reflect the information lost about how the voters connect the two issues. Rather than paradoxes, the real problem is our mistaken interpretation of what pairwise votes mean. But if standard interpretations are faulty, new ones are needed.

Any alternative definition must use the fact that the pairwise vote strips from the profile all information about the voters' preferences over all pairs. Except in a unanimity setting, the pairwise vote cannot distinguish a specified profile from any other profile in the profile cone. Consequently, more accurate interpretations must emphasize general aspects about the set of profiles in the cone. We illustrate this argument with two different "statistical" interpretations that describe the pairwise outcomes as capturing reasonable features of "most" supporting profiles.

We use Figure 2b to motivate the first interpretation and note that q is a reasonable outcome for the extreme profile line p_D. After all, more than half the p_D voters are leftists, and in this profile about 90% of the leftists prefer the elected $C > D$. Aspects of this example are emphasized with the following definition, which is specifically designed to address the Anscombe (1976) and Ostrogorski ([1910] 1970) concerns.

DEFINITION 2. *Profile p justifies the pairwise outcomes q = (q_1, q_2) in a "party dominant" way if*

- *the outcome for the pair defining the parties reflects the wishes of the larger party and*
- *the outcome for the pair describing a division within the two parties represents the majority wish of the majority party.*

The geometry of the representation square and profile lines shows how to find all profiles that agree with the q outcome and all profiles—such as p_U in Figure 2b—that cast doubt about the appropriateness of q's combined outcomes. In a very real sense, then, the q outcome does not reflect the actual profile but, rather, the likelihood q is an "appropriate outcome" for some portion of the supporting profiles. To use definition 2 to capture this sense and to interpret the pairwise vote, we note that, in addition to p_D, all profiles with left endpoint $y_L > \frac{1}{2}$ justify q in a party-dominant manner. The next theorem asserts that this party-dominant property is satisfied by most profiles that support any q. Thus, one "statistical" interpretation for the pairwise vote for two pairs is that it is the "party-dominant" outcome of most profiles with the q outcome. Notice how this result provides a positive statistical response for the Anscombe and Ostrogorski paradoxes.

THEOREM 2. *Let $q = (q_1, q_2)$ be pairwise outcomes over two pairs when the outcome for each pair is by a strict majority. Most (more than half) profile lines supporting q justify q in a party-dominant manner.*

Theorem 2 can be extended to involve any number of pairs ("most" is replaced with "the largest proportion"), but extensions are easier to make with the following alternative interpretation of pairwise voting. Again, notice from Figure 2b that all profile lines with a left endpoint satisfying $y_L > \frac{1}{2}$ have $v(B, C)$ as the largest value in the profile; that is, $v(B, C) > v(A, C)$, $v(B, D)$, $v(A, D)$. For these profiles, the $B > A$ and $C > D$ outcomes make sense, as they reflect the profile's dominant $v(B, C)$ component. Not all sup-

porting profiles allow this dominant component interpretation; for example, the largest $\mathbf{p}_U$ entry is $v(B, D)$. Indeed, it is precisely because $v(B, D)$ is the largest $\mathbf{p}_U$ component that it can be argued—with help from the tacit but incorrect assumption that pairwise votes reflect properties of the given profile—that B and D should be the $\mathbf{p}_U$ "winners."

To convert this "dominant component" argument into a "statistical" interpretation of pairwise voting, which shifts the emphasis from a specific profile to properties of the set of profiles, we note from the profile cone of Figure 2b that most profile lines supporting $\mathbf{q}$ have $v(B, C)$ as the largest component in the profile. Since the pairwise vote cannot distinguish which profile from the cone is the actual one, another statistical interpretation of $\mathbf{q}$ is that it identifies which entry of the supporting profiles is the largest in most of them.

THEOREM 3. *For two pairs, the combined pairwise outcome agrees with the largest component of most profiles when the probability distribution is the uniform distribution or the binomial distribution.*

Again, the flawed sense that the widely used pairwise outcomes reflect the properties of the actual profile can be replaced with a more accurate interpretation that the outcome represents a particular property of most supporting profiles.

Bundled Issues: Impeachment, Legislation, and Criminal Justice

As asserted, bundled voting lies at the other extreme from voting separately on issues. In bundled voting, several issues are combined into one vote. The many examples include the impeachment concerns of Ruff, what can happen in juries when charges are bundled, and even the standard legislative technique to ensure a bill will pass—bundling items of particular interest to different legislators.

Bundled voting, as in some examples of logrolling, may be an explicit attempt to overcome shifting majority difficulties by reintroducing information and connections that can be lost by voting on individual issues. In the following, these settings are captured by profile lines that indicate balance in the approval of parts of the final conclusion. Probability estimates for such situations require (as with the distribution used in the earlier special case of a straight party-line vote) using appropriate probability distributions that emphasize profile lines with balanced outcomes.

As also asserted, bundled voting can be misused by combining inappropriate parts. For instance, it is not an appropriate tool if the intent is to preserve the integrity of separate items. Ruff's statement about the impeachment proceedings specifically addresses these concerns: "a verdict may be based not on a unanimous finding of guilt as to any particular charge." This problem had arisen, Ruff noted, in the trial of Judge Nixon, when Senator Herbert Kohl (D-WI) had protested: "Please do not bunch up your allegations .... Charge each act of wrongdoing in a separate count ..."

and allow for a cleaner vote on guilt or innocence." In the Clinton case, Ruff continued, "the managers ... have searched every nook and cranny of the grand jury transcript and sent forward to you a shopping list of alleged misstatements, obviously in the hope that among them you will find one with which you disagree. But ... the record simply will not support a finding that the president perjured himself before the grand jury" (McLoughlin 1999, 284).

To analyze bundled voting, we need to discover how to disaggregate the outcome. As to the specific mechanism, a voter must choose either to approve a package of A and C or to disapprove of it. (As this could be a "yes-no" vote, it includes voting for a bill versus the status quo. Also, we changed the alternatives to be approved to A and C to demonstrate a different geometric setting.) So that the analysis does not become overly technical, we assume that a voter will opt for the package if s/he likes at least one of its options; that is, a voter can vote for a bill that offers a combination (A and C) without approving of both options. (A more realistic but technically more complicated assumption is that the voter compares the utility of voting for the bill versus that of the status quo.) This means that if a majority of voters support either option of the bundled bill, it will pass.

We now show why a bill can pass even if a majority of the voters do not approve of one part, say, C. This is because, similar to voting on separate issues, an inappropriately designed bundled vote can lose crucial information. In forcing a choice between A and C, or B and D, we lose the $(v(A, C), v(A, D), v(B, C), v(B, D))$ information about how the voters rank various combinations. As demonstrated next, unless a bundled bill is carefully designed, bundled voting can lose even more of this information.

Only minor modifications of the geometric approach developed for pairwise voting are needed to analyze what can happen when the whole consists of bundled issues. For purposes of comparison, Figure 3a represents a nonbundled setting in which each issue is voted upon separately, and passage of each issue requires a two-thirds vote. The profile cone (the shaded region of profile lines) immediately proves that, for most supporting profiles, less than two-thirds of the voters approve of both outcomes. In this example, both issues pass by the bare two-thirds vote, but only one profile (the extreme upward sloping line indicating a straight party vote) shows that two-thirds of all voters support both outcomes. Thus, other than a strict partisan vote, such an outcome is highly unlikely.

Bundled Bills with a Two-Thirds Vote. Now consider the bundled effect for a bill, or an impeachment verdict, or a trial verdict if the voter accepts at least one of the two bundled issues. As shown later, as long as $\mathbf{q} = (q_1, q_2)$ satisfies

$$q_1 + q_2 \geq \frac{2}{3}, \qquad (10)$$

FIGURE 3.   Voting with Two Bundled Issues

a. All supporting profiles   b. Impeachment via bundling   c. Likelihood estimates

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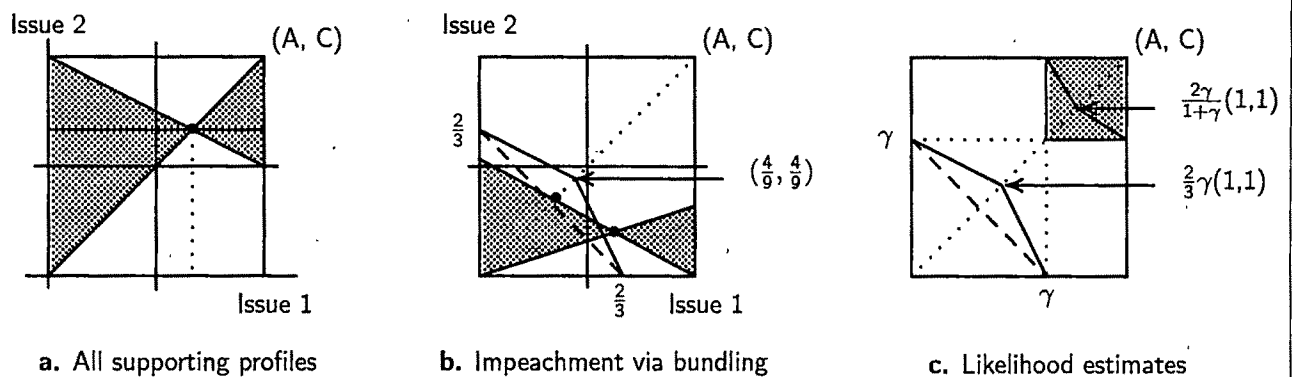
there are profiles that support **q** and that allow the bundled bill to pass with the two-thirds vote. To appreciate the flexibility offered by this condition, consider the downward sloping dashed line in Figure 3b, which is the graph of $q_1 + q_2 = \frac{2}{3}$. Thus, all **q** above this dashed line admit at least one profile that satisfies the two-thirds requirement.

A bundled bill with a two-thirds vote can significantly diminish the level of support needed for passage. The bullet on the Figure 3b dotted line represents **q** = (0.4, 0.4), which, by being above the dashed line, has supporting profiles that allow the passage of the bundled bill. Yet, because $q_1 = q_2 = 0.4 < \frac{1}{2}$, the bundled bill would satisfy the two-thirds vote barrier, even though neither issue would receive even a majority vote. Indeed, unless **q** is in the upper right-hand quarter, at least one issue would fail to receive a majority vote, even though the bundled bill could pass with a two-thirds vote. Because the geometry proves that it is quite likely for **q** not to be in this upper right-hand region, it is likely for a bundled bill to pass with a two-thirds vote, even though at least one, or both, individual issues would not receive even a majority vote.

Most Profiles.   Although equation 10 establishes conditions for a profile to pass the bundled bill with a two-thirds vote, not all profiles with the **q** outcome can pass the bill. Our earlier computational methods, for instance, show that only a small portion of the upwardly sloping profile lines in the cone supporting **q** = (0.4, 0.4) allow a two-thirds vote (as most profile lines would have $v(B, D) > \frac{1}{3}$).

This phenomenon further supports our statistical interpretation of the pairwise vote; rather than represent a specified profile, the pairwise vote represents a specified property of **q** that holds for "most" of the supporting profiles. Where do we find the **q**'s in which at least half the profiles in the profile cone pass the bundled bill? Our computational approach and algebra (see the Appendix) reveal that these **q** are above the Figure 3b solid line going from $(0, \frac{2}{3})$ (where all voters are leftists, and two-thirds of them approve of issue 2) to $(\frac{4}{9}, \frac{4}{9})$ to $(\frac{2}{3}, 0)$ (where no one supports issue 2, but two-thirds of all voters are rightists who support issue 1).

The farther **q** is above the solid line, the larger is the percentage of profiles that provide a two-thirds vote. To illustrate, **q** = $(\frac{3}{5}, \frac{2}{15})$ is on the line. The $q_1 = \frac{3}{5}$ value means that 60% of all voters are rightists, and, because they support issue 1, all of them vote for the bundled bill. To ensure passage, the extra 6.66%, or $\frac{1}{15}$ of all voters, must be leftists. They disagree with issue 1, so only those leftists who support issue 2 vote for the bundled bill. Thus, a profile leading to passage must have at least y_L of the leftists who support issue 2, where $y_L \times (\frac{2}{5}) \ge \frac{1}{15}$, or $y_L \ge \frac{1}{6}$. By computing the **q** profile cone, we find that the left-hand endpoints of profile lines are in the interval $[0, \frac{1}{3}]$. Thus, precisely half of these profile lines (those with endpoints satisfying $\frac{1}{6} \le y_L \le \frac{1}{3}$) permit the two-thirds passage of the bundled bill.

Compare this outcome with the point **q** = $(\frac{3}{5}, \frac{1}{6})$ plotted in the right-hand lower corner of Figure 3b. As this **q** is above the solid line, we should expect that more than half the profile lines ensure passage of the bundled bill. Verification only involves determining the profile cone indicated in Figure 3b. The extreme profile lines that pass through $(\frac{3}{5}, \frac{1}{6})$ connect $(0, 0)$ with $(1, \frac{5}{18})$ and $(0, \frac{5}{12})$ with $(1, 0)$. As above, since 60% of the voters are rightists (who vote for the bundled bill because of issue 1), only $y_L \ge \frac{1}{6}$ of the leftists need to approve of issue 2 for passage.

From Figure 3b and computations, we have the following conclusions about **q** = $(\frac{3}{5}, \frac{1}{6})$. First, 60% (given by $[(\frac{5}{12}) - (\frac{1}{6})]/\frac{5}{12}$) of the profile lines defined by **q** = $(\frac{3}{5}, \frac{1}{6})$ lead to passage of the bundled bill with a two-thirds vote. Second, neither issue considered separately would pass with a two-thirds vote. Indeed, issue 2 could not even pass with a majority vote. Third, for only one profile (the most extreme upwardly sloping line) will $\frac{3}{5} \times \frac{5}{18} = \frac{1}{6}$ of all voters agree with both issues. Thus, even though the bundled bill passes with a two-thirds vote, with a uniform distribution of profile lines it is with probability zero that $\frac{1}{6}$ or more of all voters approve of both issues. Finally, the likelihood that between $\frac{1}{10}$ and $\frac{1}{6}$ of all voters prefer both issues is about 40%. With a binomial or normal probability distribution, it becomes much more unlikely that even small fractions of all voters accept both issues. Ruff's concerns are very realistic.

Indeed, because our argument assumes a neutral stance by treating bundled voting as a tool, it includes a range of possibilities: from voters, jurists, or legislators unwittingly voting for a combination of outcomes when a majority does not support at least one of the bundled issues all the way to strategic behavior. Legislators·are obviously well aware of this opportunity to pass bills that may not gain a majority on their own by bundling in other issues. The 1991 congressional "corn for porn" deal is a prime example.

Appropriations Bill HR2686, the fiscal year 1992 appropriations bill, was frozen in conference as the conferees debated two key issues. The first pertained to an amendment to the bill that Senator Jesse Helms, R-NC, proposed to prevent the National Endowment for the Arts from funding projects that depicted "in a patently offensive way sexual or excretory activities or organs" (*Congressional Quarterly Almanac* 1991, 566). This amendment had been approved by the House 286-135 on October 16, 1991, and 287-133 on October 17. The amendment was also adopted by the Senate by a majority of 68 senators. Western House members, many of whom had supported the Helms amendment, were frustrated, however, by another amendment to the bill, to raise grazing fees, that had passed the House floor with a majority vote of 232 legislators on June 25, 1991. Initially, each side to the debate refused to concede. It became clear, however, that action on the appropriations bill would be impossible to complete if both amendments remained.

The intent of House and Senate conferees was to find a compromise in order to pass the bill with majority support in the House and Senate. The impasse was finally broken by Representative AuCoin, D-OR, who suggested a trade in which eastern House negotiators would drop the grazing fee increase in return for conservatives' abandonment of the amendment regarding NEA funding. "Seizing the compromise as the only viable way to complete action on the bill, House conferees voted 7-2 for it ... Senate conferees quickly followed suit" (*Congressional Quarterly Almanac* 1991, 567). The deal was labeled "corn for porn" in a catchy reference to a substitute choice of feed for livestock and the explicit art supposedly encouraged by the endowment. The bill, stripped of the two amendments, later passed the House, and, despite initial objections, passed the Senate as well, keeping the "corn for porn" deal intact.

The deal demonstrates that by bundling votes—or in this case by "bundling out" votes to negate previously approved changes—issues that could not obtain a majority on their own may be passed. In particular, conservatives in favor of the Helms amendment could vote for the final bill by referring to the removal of the portion describing grazing fees. Indeed, this compromise explicitly passed in the Senate for this reason. "Western conservatives, usually Helms' allies, seemed keenly aware of what that vote [in favor of the Helms' amendment] might portend—a vengeful Rep. Yates. Yates was likely ... to reopen the grazing fees issue if the corn for porn deal was broken [Senator] Byrd warned" (*Congressional Quarterly Almanac* 1991, 568).

Representatives not from the western states, who would find it difficult to vote against an increase in grazing fees, could support the bill by emphasizing their opposition to the Helms amendment. Thus, although neither the Helms amendment nor the grazing fee increase could have been defeated individually, by linking the issues, both were successfully removed from the 1992 appropriations bill. It is possible to argue either that this deal was beneficial, or that it was detrimental. In fact, both arguments were made, "An outraged [Representative] Dannemeyer called the conference deal 'arrogance of the worse order.' ... But the compromise drew applause from Western reachers" (*Congressional Quarterly Almanac* 1991, 566). In this case, the tradeoff and preferences were explicit; in other situations, voters may not be aware that a bundled vote is allowing a minority position to emerge victorious.

In a well-known article on logrolling, Ferejohn (1986) attributes the power of committees to their ability to bundle issues. In describing why food stamps became an agricultural program, he explains that this program is often cited as an example of logrolling between two groups—congressmen from urban and from agricultural districts—who favored two unconnected policies, but he maintains the logrolling hypothesis is inappropriate. Ferejohn (p. 223) cites collective choice results as evidence for his claim: "If a logroll is required to enact some set of bills, then there can be no package of bills that could win a majority against every other package." He further points out that logrolls are unstable because they are vulnerable to counteroffers from those who are excluded.

But if the food stamp program was not a logroll, what explains the deal? According to Ferejohn (1986, 225),

> the power of congressional committees in the legislative process provides opportunities for exchanges of support that span different stages of congressional action. ... This exchange of support should not be understood as a symmetric logroll organized in the Congress as a whole. Rather the agriculture committees bundle the two programs, one popular in committee and the other on the floor, into a single legislative package which is popular enough to survive the whole process.

Ferejohn's analysis adds strength to our argument that, as a tool, bundled voting can be used in a variety of ways.

Bundled Votes and Other Thresholds. Two other interesting settings involve a majority vote (say, for legislation) and a unanimous vote (say, for a jury verdict). In the majority vote setting, at least one profile leads to passage for any q satisfying $q_1 + q_2 > \frac{1}{2}$. In the case of unanimity votes, at least one profile leads to passage for any q above or on the line $q_1 + q_2 = 1$; this line connects the (0, 1) and (1, 0) vertices. To illustrate, we suppose that the unanimity vote over the bundled indictment is achieved with a ($\frac{3}{4}$, $\frac{1}{4}$) vote (so, 75% of the voters agree with issue 1). The associated profile cone—the extreme profile lines connect (0, 0) with (1, $\frac{1}{3}$) and (1, 0) with (0, 1)—shows that, even with the

unanimous vote, at most ¼ of these voters agree with both bundled issues, and the likelihood that between ⅛ and ¼ of all voters agree with both issues is ½.

The general assertion for these and some other concerns are described in the following theorem. In nonmathematical terms, it states that whenever a uniform distribution of profile lines is a reasonable assumption, there is serious doubt as to whether bundled outcomes reflect accurately the views of the voters.

THEOREM 4. *Suppose a bundled bill of two issues needs at least γ of all votes to pass. Suppose a voter votes for the bill if s/he approves of at least one issue. For $\mathbf{q} = (q_1, q_2)$ to ensure the existence of at least one profile that leads to passage of the bill, it is necessary and sufficient that $q_1 + q_2 \geq \gamma$.*

Assume that each profile line is equally likely. Suppose α, $0 \leq \alpha \leq 1$, is the proportion of all profile lines that support $\mathbf{q}$ and that pass the bundled bill with a γ vote. If $\alpha \leq 2(1 - \gamma)$, then it is necessary and sufficient that $\mathbf{q} = (q_1, q_2)$ satisfy

$$\begin{cases} q_1 + (1 - \alpha)q_2 \geq \gamma & \text{if } q_1 \geq q_2 \\ q_2 + (1 - \alpha)q_1 \geq \gamma & \text{if } q_2 > q_1. \end{cases} \quad (11)$$

If $\alpha > 2(1 - \gamma)$, then a necessary and sufficient condition is for $\mathbf{q}$ to be on, or above, the collection of line segments in the representation square that connects $(\gamma, 0)$ to $((\gamma + \alpha - 1)/\alpha, (1 - \gamma)/\alpha)$ to $((\gamma + \alpha - 1)/\alpha, (\gamma + \alpha - 1)/\alpha)$ to $((1 - \gamma)/\alpha, (\gamma + \alpha - 1)/\alpha)$ to $(0, \gamma)$.

For $\mathbf{q}$ to admit at least one profile (but perhaps only one) in which γ of the voters approve of both issues, then $q_1, q_2 \geq \gamma$.

A necessary and sufficient condition for $\mathbf{q}$ to have at least $\beta > 0$ of all profiles in which γ of the voters support both issues is

$$\begin{cases} q_2 + \beta(q_1 - 1) \geq \gamma & \text{if } q_1 \geq q_2 \\ q_1 + \beta(q_2 - 1) \geq \gamma & \text{if } q_2 > q_1. \end{cases} \quad (12)$$

The special case $q_1 + q_2 \geq \gamma + 1$ represents the situation in which all ($\beta = 1$) supporting profiles for $\mathbf{q}$ have γ of all voters who support both issues.

The different situations described in theorem 4 are captured in Figure 3c. In the region above the downward slanting dashed line are the $\mathbf{q}$ outcomes with at least one profile that supports passage of the bundled bill. The region above and on the solid line bent at the dotted diagonal represents equation 11, that is, at least 50% ($\alpha = ½$) of the profiles supporting $\mathbf{q}$ pass the bill. The shaded square consists of all $\mathbf{q}$ with at least one supporting profile in which at least γ of the voters approve both issues. Finally, in the region above the bent line in the shaded region, representing equation 12, $\beta = \gamma$ of all supporting profiles have at least γ of all voters who support both issues. The region in which all ($\beta = 1$) supporting profiles have at least γ of all voters who support both issues is the straight line connecting the endpoints of this bent line.

As for the $\mathbf{q}$ conditions in which α of all supporting profiles pass the bundled bill, it turns out that when

$\alpha \leq 2(1 - \gamma)$, the boundary lines of equation 11 meet on the $y = x$ diagonal below the $x + y = 1$ diagonal. For larger α values, the two equation 11 boundary equations meet the $x + y = 1$ diagonal. At this stage, passage of the bill at the indicated level requires a minimal number of voters of particular types. So, the two lines are continued either horizontally or vertically until they meet on the $y = x$ diagonal at the point where both values are $(\gamma + \alpha - 1)/\alpha$.

It is not necessary to provide estimates because the geometry already supplies the basic message: Whenever assumptions about $\mathbf{q}$ and profiles hold, such as a uniform distribution of profile lines, it is highly unlikely that a bundled bill will satisfy this γ property. Instead, the geometry proves that the likelihood an outcome allows even one profile with this γ property is small (it is the ratio of the area of the shaded square to the area of the region above the lower curved line), and this ratio approaches zero as γ approaches unity, the unanimous vote.

THREE AND MORE ALTERNATIVES

These conclusions extend far beyond the troubling properties of pairs. Only slight modifications are needed to prove that similar problems occur in any decentralized setting with disconnected parts. Related difficulties occur when the societal election outcomes are determined with triplets, or sets of four, or any number of alternatives. This is because with disconnected sets of alternatives a similar geometry illustrates the same loss of "connecting" information with similar disturbing behavior. To see this, divide a four-alternative problem into two subsets of three alternatives and rank each subset with the plurality vote or any other procedure. With minor modifications of the above, examples can be constructed in which the final outcome is supported by very few voters. Here, however, "statistical" arguments identify which ranking procedure (the Borda count) reduces the likelihood of conflict.

These comments suggest that a way to exacerbate the difficulties is to increase the number of parts. Adding pairs—or any other disjoint parts—significantly increases the already large likelihood of a conflict. To review: With certainty more than 50% of the voters approve the majority vote outcome of a single pair. For two pairs with majority votes m_1, m_2, the best we can say with certainty is that the fraction of voters who approve the combined outcome is $[m_1 + m_2] - 1$, a value that can be surprisingly small. As we show next, with more pairs the final outcome can fail to reflect the views of any voter. The arguments for $n \geq 3$ pairs mimic those for two pairs, so we indicate only differences in the geometry and certain outcomes.

Three Pairs

The three-pair setting uses the representation cube of Figure 4a. Vertex 4 at point $(1, 1, 1)$, for instance, represents $F > E, D > C, B > A$ unanimous votes. Let these rankings be the societal outcome for $\mathbf{q} = (q_1,$

FIGURE 4. Representation Cube

a. Three pair cube

b. The (0.52, 0.52, 0.52) outcome

c. Profile subcone
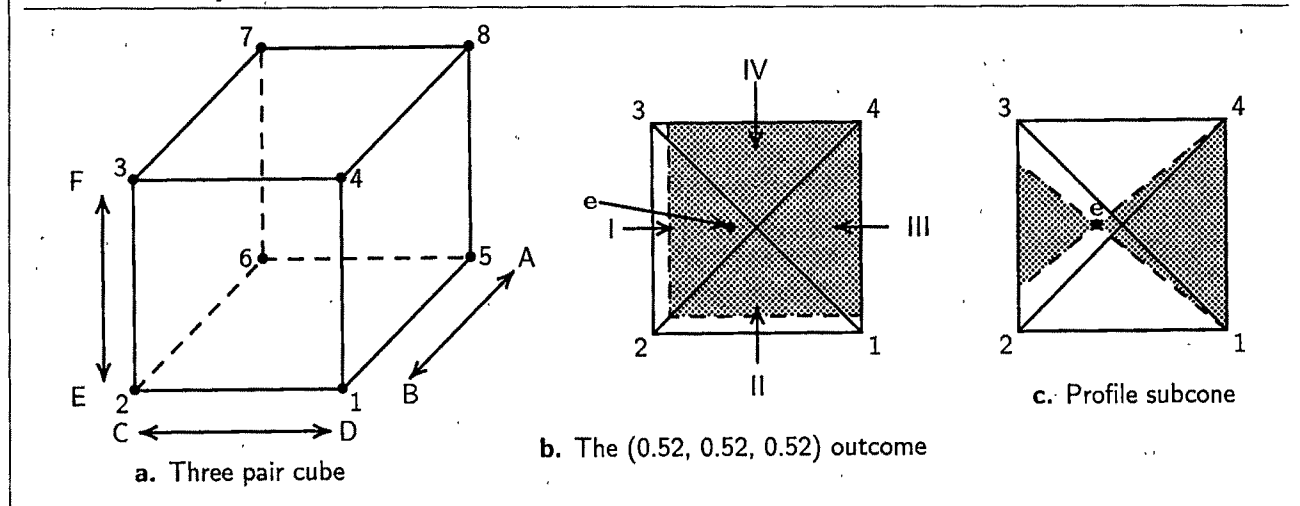
q_2, q_3). The geometric representation of profiles iterates our earlier approach. Again, use the $\{A, B\}$ ranking to divide the voters into two parties. The "frontists" prefer $B > A$, and their preferences are represented by the four vertices on the cube's front face. The "backists" prefer $A > B$, and their preferences are represented by vertices 5, 6, 7, and 8.

The geometric representation of a profile involves three line segments. The "primary" profile line segment passes through the "outcome-distinguished point" $\mathbf{q} = (q_1, q_2, q_3)$, with endpoints on the cube's front and back faces; call these the profile's primary endpoints. (The figure does not show q because of the difficulty of showing its location in a three-dimensional cube.) The front primary endpoint describes the pairwise outcomes of the frontist vote on the two pairs $\{C, D\}$ and $\{E, F\}$. Therefore, in precisely the manner described above for two pairs, disaggregate this primary endpoint with a "secondary" profile line; this line segment lies in the front face, passes through the primary front endpoint, and has its secondary endpoints on the side edges. This secondary line can be thought of as subdividing the frontists into leftist and rightist subparties. A similar decomposition describes a secondary profile line in the back face, which passes through the back face's primary endpoint.

Using the same arguments and techniques as above, these three profile line segments (the primary and two secondaries) uniquely represent a profile. So, a profile is represented by (1) a primary profile line passing through $\mathbf{q}$, with its (primary) endpoints on the front

and back faces; (2) a secondary profile line in the front face, which connects the side edges and passes through the front face primary endpoint; and (3) another secondary profile line in the back face, which connects the side edges and passes through the back face primary endpoint. Similarly, any collection of three line segments that satisfies this geometry uniquely defines a profile with $\mathbf{q}$ as its combined election outcome.

The set of all profiles that support $\mathbf{q}$ consists of all admissible combinations of these three line segments; as such, this set involves a complicated arrangement of three cones. First, the set of all admissible primary profile lines defines the primary profile cone. For example, Figure 4b depicts the primary endpoints on the front face defined by $\mathbf{q} = (q_1, q_2, q_3)$; the victorious alternative ($F > E, D > D, B > A$, a vertex 4 ranking outcome) in each pair receives 52% of the vote. To compute this region, find those extreme primary lines that pass through $\mathbf{q} = (q_1, q_2, q_3)$ and a vertex on the front or back face. As with the two-pair analysis, if $q_2, q_3, 1 - q_2, 1 - q_3 \leq q_1$, then the back primary endpoint can be anywhere on the back face, and the corresponding front primary endpoint is in the rectangle defined by the four "front point" vertices of Table 2. In this table, which identifies the corresponding front and back primary endpoints of the four extreme primary lines, the extreme primary profile line starting at vertex 6 (on the back) defines the front face primary endpoint, which is toward vertex 4; the one starting at vertex 7 is toward vertex 1, and so on (see Figure 4).

TABLE 2. Front and Back Vertices

Back Vertex	Front Pt.	Back Vertex	Front Pt.
6	$\left(1, \dfrac{q_2}{q_1}, \dfrac{q_3}{q_1}\right)$ toward 4	7	$\left(1, \dfrac{q_2}{q_1}, \dfrac{q_1+q_3-1}{q_1}\right)$ toward 1
5	$\left(1, \dfrac{q_1+q_2-1}{q_1}, \dfrac{q_3}{q_1}\right)$ toward 3	8	$\left(1, \dfrac{q_1+q_2-1}{q_1}, \dfrac{q_1+q_3-1}{q_1}\right)$ toward 2

Recall that the frontist party is subdivided into leftists and rightists. Thus, each primary endpoint on the front face, $e = (1, e_1, e_2)$, defines a profile subcone (Figure 4c). This subcone is the set of all secondary profile lines that lie in the front face and pass through this particular primary endpoint; it captures all divisions of the four voter types within the frontist party whose vote over the two pairs is e. A similar subcone is defined in the back face for each back face primary endpoint. So, each primary profile line, with its two primary endpoints, defines a pair of subcones.

The various consequences that can be extracted from the subcone geometry depend on which Figure 4b triangular region, $I, \ldots, IV$, contains the primary endpoint e represented by the •. For instance, the e given by the • in region I of Figure 4b defines the Figure 4c profile subcone for the frontist party. The geometry of this cone admits the extreme Figure 4c subprofile line that ends in vertex 1; for this profile, no voter supports all three elected outcomes. All remaining subprofiles (i.e., secondary profile lines) in this subcone, however, must include voters who accept all three outcomes. The same conclusion holds for all primary endpoints $e \in II$. The subcones for $e \in III \cup IV$, in contrast, have their extreme profiles connecting vertex 3 with a point on the right edge, and vertex 4 with a point on the left edge. Thus, all profiles with such a primary e include voters who approve of the vertex 4 combined outcome.

This three-pair geometry indicates why adding a pair loses more information about voter preferences. The subcone defined by each primary endpoint e is a two-pair profile cone, so the three-pair setting loses at least as much information as with two pairs. (This is one-dimensional because the profile line is defined by a point on an edge and the distinguished outcome.) But with three pairs, each e defines a new subcone with the associated lost information; for example, each point in the shaded region of Figure 4b defines a new subcone. (By dimension counting, two dimensions of indistinguishable information are represented by the primary lines, and another dimension for each subcone, which leads to a four-dimensional set of profiles that defines the same outcome.) Namely, the surprising variety of allowed e positions on the front and back faces demonstrates the enormous dimensional increase in lost information.

As a brief aside, this iterative process of describing profiles proves that adding more pairs radically escalates the loss of information about voters. For instance, with four pairs, the rankings of one pair can be used to define two parties. The primary profile lines pass through $q = (q_1, q_2, q_3, q_4)$, with its endpoints in two representation cubes—one for each party. Each primary endpoint defines this party's election outcomes over the remaining three pairs. Thus, each primary endpoint defines a three-pair subdivision of the above type. The fact that the four-pair setting uses a three-pair analysis for each of many possible choices of primary endpoints demonstrates the serious escalation in the amount and kind of lost information. (With n pairs, this disaggregation approach [see the Appendix]

shows that there is a $2^n - (n + 1)$ dimensional set of profiles with the same election outcome. For four pairs, then, the eleven dimensions indicate a potentially serious erosion of information.)

Geometric Consequences

We now show that with three or more candidates the erosion of information about the voters allows settings in which no one accepts all parts of a combined outcome. For three pairs, we characterize all profiles with this behavior. As q has a type 4 (i.e., vertex 4) ranking, this phenomenon occurs if and only if the profile has no type 4 voters. All such profiles must use the extreme subprofile line in an e subcone (see Figure 4c) with a vertex 1 endpoint. This, in turn, forces the front face primary endpoint e to be in regions I and II or on their boundary (see Figure 4b).

We defer probability comments to a later section, but our earlier arguments make it clear that with a uniform probability distribution on profile lines these extreme profiles are rare. (At most, one subprofile line comes from each subcone in the front face, so these profiles are in a three-dimensional set defined by those primary profile lines with an appropriate front endpoint and the associated subcones in the back face.) A rough sense of the likelihood of this behavior comes from the number of e endpoints that allow this extreme subcone line. According to Figure 4b, one such profile is associated with each e in the shaded region below or on the diagonal connecting vertices 1 and 3. So, those q that define a larger region of primary endpoints below the diagonal are more likely to admit such a profile.

Because e is above the bottom edge connecting vertices 1 and 2, and because the extreme profile starts from vertex 1, the subprofile line in the front must end on the left edge above vertex 2. This means the profile always must include voters of type 3. Similarly, because the front primary endpoint e must be below (or on) the diagonal connecting vertices 1 and 3, the back endpoint must be above the diagonal connecting vertices 5 and 7. Based on our two-pair subcone arguments, all back face subprofile lines include type 8 voters. Therefore, all profiles in which no voter agrees with the type 4 outcome have two properties. First, the profile must involve (and be dominated by) voters of three types: those whose preferences agree with the rankings of vertices 1, 3, and 8. For instance, if e requires the profile line to have only a few type 3 voters, then e is near the bottom edge in the front face. In turn, the geometry requires the endpoint on the back face to be close to the top edge and vertex 8; this requires the profile to have more voters of type 8. Second, because only one subprofile from each e subcone is used, this behavior is very rare with a binomial, normal, or uniform probability distribution of profile lines. Yet, the likelihood is represented by the size of the portion of the rectangle of e values that is below the diagonal connecting vertices 1 and 3.

To illustrate, we let a type 4 outcome consist of "yes" votes over three issues. Specifically, B, D, F represent

TABLE 3.	Where No Voter Is Happy		
Voter Type	{A, B}	{C, D}	{E, F}
1	Yes	Yes	No
3	Yes	No	Yes
8	No	Yes	Yes
Outcome	Yes	Yes	Yes

"yes" stands on the three issues (pairs), and A, C, E are "no" positions. Using this geometry and only voters of the three types that must be represented, in Table 3 we present a profile in which no one completely approves of the final vote. This behavior, caused by a loss of crucial information about how the voters connect the parts, may be called a paradox. According to the above geometric argument, however, or our earlier statistical interpretation of pairwise votes over several pairs, such profiles become anomalies. Since the iterative construction of profile lines for more pairs must involve the above three-pair construction, versions of these statements extend.

All three-issue pairwise voting examples that manifest this unexpected behavior must have the above characteristics, so it is easy to construct and analyze examples (Nurmi 1998) that exhibit the Anscombe and Ostrogorski paradoxes. To do so, just add two more voters. For instance, by adding two type 6 voters, Table 3 is augmented by Table 4. The final outcome is "no," by a 3:2 vote, on each of the three issues. If the "yes" and "no" parties are defined, respectively, by taking a positive and negative stand on all issues, then a majority of the voters (1, 3, 8) support the "yes" party on a majority of the issues, even though the "no" party wins over all issues. Similarly, each voter in a majority (voters 1, 3, 8) is frustrated on two of the three issues. Again, rather than a paradox, these examples reflect the loss of crucial information about the voters. Again, from the statistical interpretation, such profiles are in the minority.

As further support that such behavior is in the minority, we describe the q choices in which all supporting profiles have voters who approve of the combined outcome. Such a profile cannot have a subprofile with vertex 1, so the condition holds if and only if the rectangle of admissible e values is strictly above the diagonal connecting vertices 1 and 3; that is, the lower left vertex (coming from vertex 8; Table 2) of the rectangle must be above this diagonal. The line is $y + z = 1$, so the sum of the appropriate components of this vertex must be greater than unity, or (after collecting terms)

$$q_1 + q_2 + q_3 > 2.$$

TABLE 4.	Adding Voters		
Voter Type	{A, B}	{C, D}	{E, F}
6	No	No	No
6	No	No	No

TABLE 5.	Assigning Rankings to Vertices		
Vertex	Ranking	Vertex	Ranking
1	$\mathcal{A} > \mathcal{B} > \mathcal{C}$	7	$\mathcal{C} > \mathcal{B} > \mathcal{A}$
2	$\mathcal{A} > \mathcal{C} > \mathcal{B}$	8	$\mathcal{B} > \mathcal{C} > \mathcal{A}$
3	$\mathcal{C} > \mathcal{A} > \mathcal{B}$	5	$\mathcal{B} > \mathcal{A} > \mathcal{C}$

This inequality means that if q is such that $q_1 + q + 2 + q_3 = 2$, then q has precisely one profile in which no voter supports the outcome. To explain, this q allows only one primary profile line to have a front face endpoint on this diagonal. Then, the extreme front face subprofile line connects vertices 1 and 3, which are the only allowed frontist voter types. Similarly, because e is a vertex of the rectangle of endpoints in the front face, its associated back endpoint (see Table 2) is vertex 8. Consequently, the profile consists strictly of voters of types 1, 3, 8. For instance, according to Table 2, the outcome $q = (\frac{2}{3}, \frac{2}{3}, \frac{2}{3})$ places this particular front face vertex at the midpoint of the diagonal. By use of algebra, it now follows that the only profile supporting q that has no type 4 voters is

$$v(B, D, E) = v(B, C, F) = v(A, D, F) = \frac{1}{3}, \quad (13)$$

or that of Table 3. No voter approves the joint outcome, but the many remaining profiles that support q, which use all other remaining choices of e and their subcones, include type 4 voters. Thus, the Table 3, equation 13 profile is rare and unusual among those defining the election outcome $q = (\frac{2}{3}, \frac{2}{3}, \frac{2}{3})$; rather than a paradox, this profile should be treated as an anomaly among those defining this outcome.

Voting Paradoxes and Arrow's Theorem

To continue our geometric description, we relate the loss of information caused by pairwise voting to cyclic voting outcomes. To do so, we treat the Figure 4 pairs as the pairs of the three alternatives $\mathcal{A}$, $\mathcal{B}$, $\mathcal{C}$. By selecting the $\{\mathcal{A}, \mathcal{B}\}$, the $\{\mathcal{B}, \mathcal{C}\}$, and the $\{\mathcal{C}, \mathcal{A}\}$ rankings to be represented, respectively, on the x, y, and z axis of a standard coordinate representation. We show in Table 5 the Figure 4 vertices that represent the rankings. The remaining two vertices, 4 and 6, represent the cyclic preferences of $\mathcal{A} > \mathcal{B}$, $\mathcal{B} > \mathcal{C}$, $\mathcal{C} > \mathcal{A}$ and $\mathcal{B} > \mathcal{A}$, $\mathcal{C} > \mathcal{B}$, $\mathcal{A} > \mathcal{C}$. The individual rationality assumption, whereby each voter must have transitive preferences, restricts the voter types to those listed in Table 5.

The above discussion shows how to design all transitive profiles that cause cyclic outcomes. Equation 13 defines a profile with an equal number of voters of the three types,

$$\mathcal{A} > \mathcal{B} > \mathcal{C}, \quad \mathcal{B} > \mathcal{C} > \mathcal{A}, \quad \mathcal{C} > \mathcal{A} > \mathcal{B}, \quad (14)$$

and the result is a cyclic outcome in which each pair wins by a two-thirds vote. This profile is the Condorcet triplet, and we now know why it causes difficulties.

TABLE 6. Finding Probabilities			
Condition	Prob.	Condition	Prob.
$e_1 \leq 1 - e_2,\ e_2$, (in I)	$\dfrac{q_1 e_1 - \alpha}{q_1 e_1}$	$e_2 \leq e_1 \leq 1 - e_2$, (in II)	$\dfrac{q_1 e_2 - \alpha}{q_1 e_2}$
$1 - e_2,\ e_2 \leq e_1$, (in III)	$\dfrac{q_1 e_2 - \alpha}{q_1(1 - e_1)}$	$1 - e_2 \leq e_1 \leq e_2$, (in IV)	$\dfrac{q_1 e_1 - \alpha}{q_1(1 - e_2)}$

Specifically, even though voters are explicitly assumed to have transitive preferences, the pairwise vote loses all this connecting information—it loses the rationality assumption. Instead, as far as the pairwise vote is concerned, not only are voters with cyclic preferences admissible, but also the procedure treats them as being the more likely voter types. Thus, the discussion following equation 13 applies: As far as the pairwise vote procedure is concerned, it is highly unlikely for the profile in equation 14 to define the outcome; most profiles with this particular outcome involve cyclic voters. Stated in another manner, the appropriate probability distribution for the pairwise vote is either the binomial or uniform distribution of profile lines. The standard assumption of transitive voter preferences imposes a probability distribution that conflicts with the procedure because it does not allow voters to have preferences of type 4 or 6. (Essentially, the same explanation holds for Arrow's theorem [Saari 1998] because the IIA condition also drops "connecting" rationality information about pairs.)

A resolution requires finding a way to include this rationality assumption. As described in Saari (1995, 1999), this is the Borda count, that is, 2, 1, and 0 points are assigned to a voter's first, second, and third preferences. For the same reasons, a similar approach holds in general for all the difficulties described here.

Finding Likelihood Estimates

As a final illustration, we compute the likelihood that at least α of all voters approve of the combined outcome for $q = (0.52, 0.52, 0.52)$ and the Figure 4b endpoint $e = (1, e_1, e_2) = (1, 0.375, 0.5)$. Because q_1 of all voters are frontist, e_1 of the frontists are rightist, and y_R of all rightists prefer all three outcomes, the condition requires $q_1 e_1 y_R \geq \alpha$, or $y_R \geq \alpha/q_1 e_1$. The full right edge of the front face is in the profile subcone, so the uniform distribution likelihood is $1 - (\alpha/q_1 e_1) = (q_1 e_1 - \alpha)/q_1 e_1$. The numerator never can be negative, so with the specified values $q_1 = 0.52$, $e_1 = 0.375$, no more than 19.5% of the voters agree with all outcomes. Yet, with probability 0.73 at least 5% ($\alpha = 0.05$) of all voters accept all outcomes. A general statement is found in the same manner.

THEOREM 5. If $e = (1, e_1, e_2)$ is a front face endpoint for q, where $q_1, q_2, q_3 > 0.5$, and if the lines and profile sublines are uniformly distributed, the likelihood that α of all voters agree with all three outcomes is given in

Table 6. The α values are restricted to $0 \leq \alpha \leq \min(q_1 e_1, q_1 e_2)$.

Thus, "paradoxes" are surprisingly likely, but that label implies the combined pairwise outcomes should accurately describe what voters want; this is false, as the best we can expect is a probabilistic interpretation. Here, theorem 3 extends to any number of pairs to assert that if profiles are divided according to which component is the largest, the largest proportion of profiles have agreement between the rankings of the largest component and **q**. Pairwise rankings must be interpreted in a statistical rather than an absolute sense.

Much more is possible. For example, by integrating Table 6 values over all primary e positions, we can determine the likelihood that at least α of the voters prefer the combined outcome. The resulting complicated expressions, however, do not provide quick insight. Still, for purposes of completeness, we indicate how to perform the computations. If we assume that $q_3 \leq q_2 \leq q_1$, that each admitted e is equally likely, and that each subprofile line in each subcone is equally likely, then the likelihood of at least α of all voters preferring the combined outcome is determined by integrating the Table 6 values over the regions determined by Table 2. For instance, the likelihood over the region I portion is $\int_{(q_1 + q_2 - 1)/q_1}^{1/2} \int_x^{1-x} (q_1 x - \alpha)/q_1 x\, dy\, dx = 1/q_1^2[((1 - q_2 - q_1)/2)(q_1 + 2\alpha) - q_1^2/4 + (q_1 + q_2 - 1)^2 + q_1\alpha \ln ((q_1 + q_2 - 1)/q_1)]$. Because the expressions for the other three regions are equally complicated, for many purposes it is more reasonable to use the geometry supported by intuition derived from two-pair computations.

CONCLUSION

Even though pairwise and bundled voting seems attractive and intuitive in many political settings, there can be problems. In both procedures, it is possible for a significant percentage of the population to be dissatisfied with at least part of the overall outcome. We have shown why this can occur as well as the likelihood that any given percentage of the voters will be frustrated. The intuition developed from our analysis suggests that, to obtain reliable outcomes, these approaches should be replaced. But with what?

Both procedures become problematic by losing information about the profile **p**. For comparison, a Borda count procedure allows voters to rank combinations in

order of preference. By assigning the usual 3, 2, 1, 0 points to these priorities, information about the voters is reintroduced into the process. In particular, since this method requires voters to compare one option with all other combinations, it becomes possible to say, with certainty, that the selected options have support from the voters.

In order to be politically feasible, the procedure should be restricted to forming combinations of realistic size among relevant alternatives. (Asking voters to rank combinations of 20 or more unrelated choices is time consuming, unrealistic, and unnecessary.) This comment finds support in the Brams, Kilgour, and Zwicker (1998) article, which revealed that the most frequently chosen combination in voting for 52 initiatives (in 1992) was "all abstain."

Interestingly, in the case of bundled voting, the restriction on votes over related issues might hamper the legislative tendency to combine issues that, on their own, would not receive majority support. This includes unrelated issues important to certain legislators but sufficiently minor to others that they will not affect passage of the bundled bill. Restrictions on the relevance of alternatives in the ranking of combinations should discourage such practices.

This is an important concern because a basic goal of democracy is a government and mechanisms representative of the wishes of the populace. When we discover flaws in our decision tools, we should highlight and correct them to ensure that outcomes are not dictated by unrepresentative minorities. Indeed, legislators intuitively understand and manipulate the kinds of results demonstrated here. Amendments are routinely added to popular bills with full understanding on the part of all involved that these issues would not survive a vote if considered on their own. The awareness of the effects of bundling is so widespread that it is acknowledged in the *Congressional Quarterly Almanac* (1998, 2-112) description of the fiscal 1999 Omnibus Appropriations Bill: "Democrats felt shut out of the process, but they won concessions that they would not have obtained had the 13 bills [included in the appropriations bill package] been negotiated individually." These arrangements are made in the spirit of compromise—special favors are given in response to support for other issues. Nonetheless, these comments illustrate the familiarity of legislators with the manipulative opportunities afforded by the bundled vote.

APPENDIX: PROOFS

We provide here the proofs of the various assertions and more complicated computations. To begin, we indicate why, for probability computations, the edge lengths suffice. In Figure 2b, the three lines are the extreme profile lines, and the third is the boundary line for the heavily shaded region. The area of the heavily shaded region on the left side is half the distance from the left edge to q times the length of the region's base on the left edge. Similarly, the area of the shaded region is half the distance from the left edge to q times the length of its base along the left edge. Thus, the ratio of these two areas is the ratio of the lengths of their bases on

the left edge. That either the left or right edge can be used in probability computations follows from similar triangles. Because the heavily shaded triangle on the left is similar to the one on the right, and because the same statement holds for the shaded triangles, it follows that the values of the ratios are the same if computed on the right or on the left edge.

Proof of Theorem 1

According to the assumptions of the theorem, the two pairs are, respectively, $\{A, B\}$ and $\{C, D\}$, where $B > A$ with m_1 of the vote, and $C > D$ with m_2 of the vote. Thus, in equation 6, $q_1 = 1 - m_1$, $q_2 = m_2$, and the profile lines have endpoints on the left edge on the interval $[(m_1 + m_2 - 1)/m_1, m_2/m_1]$. The only way at least α of all voters can prefer both outcomes is if α of all voters are leftist with preferences listed by the upper vertex of Figure 2b. Thus, we are only interested in profile lines with a left endpoint that satisfies $y \times m_1 \geq \alpha$, or $y \geq \alpha/m_1$. The likelihood is given by the fraction $[m_2/m_1 - \alpha/m_1]/[m_2/m_1 - (m_1 + m_2 - 1)/m_1] = (m_2 - \alpha)/1 - m_1$.

Similarly, if at least β of all voters dislike both outcomes, they all are rightist with preferences on the lower corner. Because $(1 - m_1)$ of all voters are rightist, we need profile lines with a right endpoint that is y satisfying $(1 - m_1)(1 - y) \geq \beta$, or $1 - m_1 - \beta/1 - m_1 \geq y$. The conclusion follows.

Proof of Theorem 2

If q is in one of the quadrants, then the outcome for the pair defining the two parties must correspond to the wishes of the larger party. If q is above the horizontal line, then so are most profile lines; this proves the second result.

Proof of Theorem 4

A profile line with left and right endpoints $q_L = (0, y_L)$ and $q_R = (1, y_R)$, has the representation

$$(1 - t)(0, y_L) + t(1, y_R), \qquad 0 \leq t \leq 1. \qquad (15)$$

Since q is the point on this line when $t = q_1$, we have

$$(1 - q_1)(0, y_L) + q_1(1, y_R) = (q_1, (1 - q_1)y_L + q_1 y_R)$$
$$= (q_1, q_2), \qquad (16)$$

or

$$(1 - q_1)y_L + q_1 y_R = q_2. \qquad (17)$$

All of the rightists, who constitute q_1 of all voters, vote for the bundled bill. Thus, the $\gamma - q_1$ extra voters needed to pass the bundled bill must come from the leftists. In turn, this requires y_L to satisfy the inequality

$$y_L(1 - q_1) \geq (\gamma - q_1), \text{ or } y_L \geq (\gamma - q_1)/(1 - q_1). \qquad (18)$$

There are two situations. The first occurs when an extreme profile line passes through q and the $(1, 0)$ vertex; this is true if and only if $q_1 + q_2 \leq 1$. From the use of $y_R = 0$, $y_L = (\gamma - q_1)/(1 - q_1)$, and equation 16, it follows that a supporting profile exists for q, $q_1 + q_2 \leq 1$, which passes the bundled bill if and only if $(1 - q_1)[(\gamma - q_1)/(1 - q_1)] \leq q_2$, or if and only if $q_1 + q_2 \geq \gamma$.

The remaining setting of $q_1 + q_2 > 1$ has a profile line that passes through the $(0, 1)$ vertex, which represents where all leftists, $(1 - q_1)$ of all voters, vote for the bill. Here, the $y_L = 1$ value always satisfies equation 18, so the bill always passes.

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Since $q_1 + q_2 \geq 1$ always satisfies $q_1 + q_2 \geq \gamma$, this second inequality suffices to describe the conditions.

Next, we determine when α, $0 \leq \alpha \leq 1$, of the profile lines supporting $\mathbf{q}$ pass the bundled bill. This requires finding conditions on q_1, q_2, so that at least α of these profile lines satisfy equation 18. The two diagonal lines of the square, $y = x$ and $x + y = 1$, divide the representation square into four triangles; the four settings of $\mathbf{q}$ in each triangle are considered separately. If $q_1 \leq q_2 \leq 1 - q_1$ (this triangle has the square's left edge as a leg), then the extreme profile lines for the profile cone for $\mathbf{q}$ are defined by the two vertices on the right edge. Thus, the extreme profile lines $\mathbf{p}_U$ and $\mathbf{p}_D$ are defined, respectively, by $y_R = 1$ and $y_R = 0$. According to equation 17, the two respective left endpoints are $y_L = (q_2 - q_1)/(1 - q_1)$ and $y_L = q_2/(1 - q_1)$. Because the internal $(q_2 - q_1)/(1 - q_1) \leq y_L \leq q_2/(1 - q_1)$ include all possible left endpoints, α of these endpoints are above the value $y_L = \alpha[(q_2 - q_1)/(1 - q_1)] + (1 - \alpha)[q_2/(1 - q_1)]$. According to equation 18, these left endpoints define profile lines that pass the bundled bill if and only if $\alpha[(q_2 - q_1)/(1 - q_1)] + (1 - \alpha)[q_2/(1 - q_1)] \geq (\gamma - q_1)/(1 - q_1)$, or if and only if

$$q_2 + (1 - \alpha)q_1 \geq \gamma. \tag{19}$$

The triangular region with the square's bottom edge as a leg is where $q_2 < q_1 < 1 - q_2$. One extreme profile line has $y_R = 0$, so, according to equation 17, $y_L = q_2/(1 - q_1)$. As the other extreme profile line has $y_L = 0$, the top α of the left endpoints are given by $y_L \geq (1 - \alpha)q_2/(1 - q_1)$. Thus, according to equation 18, at least α of the supporting profiles lead to passage of the bundled bill if and only if $(1 - \alpha)q_2/(1 - q_1) \geq (\gamma - q_1)/(1 - q_1)$. Thus, replacing equation 19 is $(1 - \alpha)q_2 + q_1 \geq \gamma$.

The final two triangles satisfy $q_1 + q_2 > 1$. The triangle with $q_1 > q_2$ has the square's right edge as a leg. Here, more than half the voters are rightist, and the extreme profile lines involve the two vertices of the left edge. Thus, at least α of these lines support the bundled bill when $y_L \geq 1 - \alpha$. According to equation 18, the bundled bill passes if

$$q_1 \geq [\gamma + \alpha - 1]/\alpha. \tag{20}$$

If $\gamma + \alpha - 1 \leq 0$ (so the equation 20 restriction is $q_1 \geq 0$), just the fact that $\mathbf{q}$ is in this triangular sector suffices to ensure that at least α of the supporting profiles lead to the passage of the bundled bill. In fact, since $q_1 > \frac{1}{2}$, it follows (by setting the left-hand side of equation 20 equal to $\frac{1}{2}$) that equation 20 imposes a restriction only if $\alpha > 2(1 - \gamma)$.

Similarly, in the upper triangular region, the y_L values satisfy $(q_2 - q_1)/(1 - q_1) \leq y_L \leq 1$, so the condition for α of the supporting profiles to allow passage now is

$$\left[(1 - \alpha) + \alpha \frac{q_2 - q_1}{1 - q_1}\right](1 - q_1) + q_1 \geq \gamma.$$

Here, equation 20 is replaced by $q_2 \geq [\gamma + \alpha - 1]/\alpha$ with the same comments that these restrictions have meaning only when $\alpha \geq 2(1 - \gamma)$.

By graphing the restrictions for the four regions when $\alpha > 2(1 - \gamma)$, the description of the theorem is obtained.

For a profile to have at least γ of the voters who prefer both issues, $v(A, C) \geq \gamma$. Thus, $y_R q_1 \geq \gamma$, or $y_R \geq \gamma/q_1$. If $q_2 \geq q_1$, then the profile line most favorable for this condition has the vertex $(1, 1)$. Here, $y_R = 1$, so the condition is $q_2 \leq q_1 \leq \gamma$. If $q_1 > q_2$, then the most favorable profile line has $(0, 0)$ as a vertex. Thus, using equation 17 with $y_L = 0$ leads to the condition $y_R = q_2/q_1$. This means that $q_2/q_1 \geq \gamma/q_1$. Consequently, $q_1 \geq q_2 \geq \gamma$.

With $q_1, q_2 \geq \gamma \geq \frac{1}{2}$, if $q_1 \geq q_2$, the extreme profile lines have $y_L = 1$ and $y_L = 0$ leading to $(q_1 + q_2 - 1)/q_1 \leq y_R \leq q_2/q_1$. The β portions of largest y_R values satisfy $y_R \geq \beta[(q_1 + q_2 - 1)/q_1] + (1 - \beta)[q_2/q_1]$. When this value is at least as large as the required γ/q_1 and when the resulting inequality is solved, we have the condition that, when $q_1 \geq q_2$, $q_2 + \beta(q_1 - 1) \geq \gamma$. The only difference when $q_2 \geq q_1$ is that the profile lines satisfy $(q_1 + q_2 - 1)/q_1 \leq y_R \leq 1$. Thus, the threshold for the top β values is $y_R = \beta[(q_1 + q_2 - 1)/q_1] + (1 - \beta) \geq \gamma/q_1$. Therefore, when $q_2 \geq q_1$, the condition is $q_1 + \beta(q_2 - 1) \geq \gamma$.

Proof of Theorem 5

This is a direct algebraic computation.

Finally, a quick way to derive the comments concerning the dimensions of the set of profiles defining a specified outcome for n pairs is to use algebra. With n pairs, there are two ways for a voter to vote for each issue, so there are 2^n different voter types. Our choice of a profile describes the fraction of all voters who are of each type, so the space of profiles is $2^n - 1$ dimensions. The outcome $\mathbf{q} = (q_1, \ldots, q_n)$ is a point in n-dimensional space. Thus, an election can be viewed as a set of n linear equations with $2^n - 1$ variables. Standard rules from algebra state that the inverse set for any specified outcome $\mathbf{q}$ usually has dimension $2^n - 1 - n = 2^n - (n + 1)$.

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Asymmetric Information and Legislative Rules: Some Amendments

VIJAY KRISHNA *The Pennsylvania State University*
JOHN MORGAN *Princeton University*

*W*e reexamine the major tenets of the informational theory of legislative rules, focusing on the informational efficiency of rules with varying degrees of restrictiveness. When committees are heterogeneous, full efficiency is attainable under the unrestrictive open rule as well as the somewhat restrictive modified rule. In contrast, the restrictive closed rule always leads to inefficiencies. When committees are homogeneous, the situation is different. All equilibria are inefficient regardless of legislative rules, but the closed rule leads to greater informational efficiency than does the open rule. Furthermore, the efficiency gains under the closed rule more than offset distributional losses regardless of the degree of preference divergence. We also examine the incentives provided by the different rules for information acquisition and committee specialization.*

It is universally acknowledged that the process of legislation can have a dramatic effect on policy. Legislative rules determine how bills are introduced, amended, and voted upon. Since the mid-nineteenth century, specialized standing committees have come to play an important role in the U.S. congressional process, because almost all bills are drafted in committee before being sent to the floor for a vote. The House of Representatives has eighteen committees specializing in a range of issues, including the important Rules Committee. It sets the rules under which each bill is considered, and these determine whether and how the bill may be amended. The closed rule is a restrictive procedure that does not permit amendments to be offered on the floor, whereas the open rule allows amendments to be freely offered. Currently, some intermediate type of restrictive rule is used for most bills. Sinclair (1994) points out that restrictive rules are used with increasing frequency. They were applied to only 15% of the bills in the 95th Congress, compared to 66% in the 102d Congress.[1] A practical effect of these amendment limitations is to give considerable power to committees in guiding the final form of the bill to the House floor.

Why does the House adopt restrictive amendment procedures, thereby ceding substantial authority to the committees? Theoretical explanations fall into two major categories, and the debate about them has been termed the institutional design controversy (Sinclair 1994). Distributive theories postulate that the legislative process is organized so as to facilitate rent extraction on the part of members. As a result, a committee

is likely to be the subset of the legislature with the most to gain from the actions of the committee, and restrictive rules allow these members to appropriate rents from legislation. Informational theories postulate that the role of the committee is to gather information relevant to the legislation. The legislative process itself is a device for conveying information from the committee to the legislature, and restrictive rules may be more effective at this than unrestrictive rules. This article reexamines the major tenets of informational theories.

In two important works, Gilligan and Krehbiel (1987, 1989) develop formal models that constitute the underpinnings of informational theory. Their analysis is based on two premises. First, information gathering is the primary reason for the existence of specialized committees. By holding hearings and other investigations into the effect of various policies, they obtain a degree of knowledge not possessed by other members of the House. Second, the interests of committee members are not the same as those of the median voter on the House floor. Thus, there is both an asymmetry of information and a divergence of preferences between the committee and the legislature. Various actions by the committee convey information to, and thus indirectly influence, the legislature. Legislative rules affect how much information transmission takes place as well as how this information is translated into policy outcomes.

Open, closed, and modified (an intermediate form) legislative rules are evaluated on two grounds. First, how effective is a particular rule in overcoming the asymmetry of information? Different rules result in different amounts of information flowing from the committee to the legislature, and the more information that flows, the better it is for all parties. It is argued that greater informational efficiency may be fostered by rules that evolve in practice.[2] Second, how effective is a particular rule in providing the committee with the incentives to acquire relevant information? Different rules provide different incentives, and if information

[1] The degree of restrictiveness varies a great deal. The vast majority of bills are assigned "modified open" or "modified closed" rules. Completely open or closed rules are applied much more rarely. See Sinclair 1994.

[2] A rule is said to have greater informational efficiency than others if it transmits more information from the committee to the legislature. Put another way, the residual uncertainty faced by the legislature under that rule is smaller than under others.

acquisition is costly for the committee, it may be that one set of rules leads it to acquire the information and become specialized, whereas another set does not. It is argued that greater specialization may be fostered by rules that evolve in practice.

In this article, we study the most informationally efficient equilibrium arising under each rule. We do this separately for two cases: heterogeneous and homogeneous committee member preferences. We also study the incentives to specialize. Our three main findings can be summarized as follows. First, with heterogeneous committees, the open and modified rules are fully informationally efficient, and the legislature faces no residual uncertainty (propositions 1 and 5, below). In contrast, all legislative equilibria under the closed rule are informationally inefficient (proposition 2). Second, with homogeneous committees, the closed rule is more informationally efficient than the open rule, regardless of the divergence of preferences among the committee and the legislature (propositions 6 and 8). Third, with heterogeneous committees, the modified rule provides the best overall incentives to specialize. With homogeneous committees, the closed rule provides the best overall incentives.

RELATED THEORETICAL WORK

The models we study were introduced by Gilligan and Krehbiel (1987, 1989), who found that informational inefficiency is pervasive, regardless of the particular rule or committee preferences. When the committee is heterogeneous, they found the closed rule most efficient. When the committee is homogeneous, the relative efficiency of rules depends upon the degree of preference divergence between the committee and the legislature. Closed are superior to open rules if that divergence is not too great, and the reverse is true when the divergence is large.

Our results differ significantly from those of Gilligan and Krehbiel. Since our model and methods are identical, it is worth exploring the reasons for this discrepancy. Informational theory models typically have multiple equilibria. Hence, a comparison of rules is also a comparison of a single equilibrium selected for each rule. It is important that the criterion used to select a particular equilibrium be applied in a consistent manner across all rules. Gilligan and Krehbiel (1987, 1989) propose maximal informational efficiency as the appropriate criterion (see the next section), but we show that they do not apply it consistently. We apply this criterion uniformly for each rule under consideration, which accounts for the difference in our results and theirs. Detailed comparisons and contrasts are discussed later.

Our work is also related to Epstein (1998), who looks at equilibria arising in the Gilligan and Krehbiel (1989) model when the majority committee median has gatekeeping power and all committee members are specialized. He points out that the equilibrium selected under the open rule by Gilligan and Krehbiel is not robust to the introduction of asymmetric committees. The equi-

libria we analyze under the open or modified rule are not subject to this criticism.

Baron (1999) modifies the Gilligan and Krehbiel (1987) model and allows the legislature to design contracts conditional on the bill proposed by the committee. Thus, his framework is a screening model of legislative rules as opposed to a signaling model.

Absent any rules restrictions, the actions of the committee are just "cheap talk." Crawford and Sobel (1982) analyze a general cheap talk model with a single sender and find that full informational efficiency cannot be attained. Battaglini (2000) studies two-sender cheap talk models in which the underlying uncertainty may be multidimensional. He introduces a model in which committee members are perfectly informed only with probability $1 - \varepsilon$ and examines the limiting case as ε becomes small. Equilibria that are robust to such a modification are called ε-stable. Battaglini's main result is that full informational efficiency can be achieved only when there is more than one dimension of uncertainty. Since the equilibria we construct under the open rule are fully efficient, they are not ε-stable. The full implications of ε-stability in the one-dimensional case are yet to be explored. For instance, there is no characterization of the most efficient ε-stable equilibrium, even under the open rule. Furthermore, how this requirement affects equilibria under restrictive rules remains an open question as well.

Austen-Smith (1990) examines the influence on the legislative process of cheap talk, that is, debates. He finds that these play a significant role in setting the legislative agenda, even if they are informationally irrelevant. Austen-Smith (1993) also examines belief formation in a setting in which the committee and the legislature have different prior distributions that are not commonly known. He then evaluates how the process by which bills are referred to committee can affect outcomes. Again, the issue of restrictive rules is not central to his analysis.

Restrictive legislative rules are a means of agenda control on the part of the committee. Agenda control has been studied in other contexts as well. For example, Romer and Rosenthal (1979) use a model of monopoly agenda control by a budget-maximizing bureaucrat to show that the median voter theorem may not hold in these circumstances. Banks (1990) extends their model to a situation of asymmetric information, so that only the agenda-setting bureaucrat knows the "disagreement outcome" if the proposal put forward is rejected.

Shepsle and Weingast (1987) study a model that seeks to isolate the sources from which committees derive political power. They show how the legislative process confers such power on committees and highlight the important role played by committees during the conference procedure in a bicameral legislature.

Dewatripont and Tirole (1999) examine the specialization question in a model of advocates. Although the information gathering role of an advocate is similar to that of a committee member, there are many important differences. First, the decision maker can commit to direct monetary compensation of the advocates (as in Baron 1999 also). Second, the advocates are ideologi-

cally neutral: They are not directly interested in the decision (the legislation) itself and care about it only to the extent that their reward may be contingent on the decision. Finally, the advocates are given only limited strategic power: They can either conceal the information they have acquired or reveal it truthfully. Dewatripont and Tirole show the informational benefits of heterogeneity in areas of specialization, not preferences. One may question, however, whether a framework in which the legislature can, in effect, sign an optimal contract with ideologically neutral committee members and compensate them monetarily is appropriate as a model of congressional committees. Moreover, the model is silent on the effect of process, in the form of restrictive rules, on legislative outcomes.

We will begin by sketching the informational theory models of legislative rules. We then study informational efficiency for open, modified, and closed rules when committees are heterogeneous. We next consider the case of a homogeneous committee and compare open and closed rules. We then endogenize the specialization decision of the committee and study the efficacy of restrictive rules with both heterogeneous and homogeneous committees. Finally, we discuss some of the empirical implications of our results and conclude with some comments on what our results say regarding the institutional design controversy. Detailed proofs of all propositions are contained in the Appendix.

THE MODEL

We will briefly sketch the Gilligan and Krehbiel (1989) model of legislation that originates in heterogeneous committees. The reader should refer to their article for further details and motivation. To facilitate comparison, we adopt their notation exactly.

There are three players in the game. Initial proposals are made by two *committee members*, $c1$ and $c2$. The third player is the *legislature*, ℓ, which ultimately determines the policy to be adopted. All players care about a unidimensional *outcome* $x \in X$. Each committee member has an ideal outcome, denoted by x_{c1} and x_{c2}, respectively. The legislature's ideal outcome, x_ℓ, is set equal to zero without loss of generality. All players use "quadratic loss" utility functions to evaluate actual outcomes. Thus, the legislature's utility from an outcome x is

$$u_\ell(x) = -(x_\ell - x)^2 = -x^2,$$

whereas the committee members' utilities are

$$u_{c1}(x) = -(x_{c1} - x)^2 \text{ and } u_{c2}(x) = -(x_{c2} - x)^2,$$

respectively. It is supposed that $x_{c1} \geq 0$, and $x_{c2} = -x_{c1}$. Thus, each committee member is "biased" relative to the legislature, and the two members are biased in opposite directions. In what follows, we write $x_{c1} = x_c$ and $x_{c2} = -x_c$.

The committee proposes one or more bills, b, and the legislature then chooses a *policy* $p \in P \subset \mathbf{R}$. The policy p results in an uncertain outcome, $x = p + \omega$,

that depends on some underlying state of nature $\omega \in [0, 1]$. Because the state of nature is assumed to be uniformly distributed, ω has mean $\bar{\omega} = \frac{1}{2}$ and variance $\sigma_\omega^2 = \frac{1}{12}$. It is assumed that $x_c < 3\sigma_\omega^2 = \frac{1}{4}$.

There is an exogenously given status quo policy, p_0. We suppose that $-1 \leq p_0 \leq 0$, so it is not the case that the status quo policy is never optimal from the perspective of the legislature. Indeed, it is optimal if $\omega = -p_0$.

The sequence of moves is as follows. First, nature reveals the state ω to both committee members. Second, the committee members send bills to the legislature in accordance with the rules set out below. Third, the legislature adopts a policy in accordance with the rules.

Legislative Rules

Three different rules governing the legislative process are considered. (1) The open rule allows both $c1$ and $c2$ to propose bills, $b_1 \in P$ and $b_2 \in P$, respectively. The legislature is free to choose any policy $p \in P$ it wishes. (2) The closed rule allows $c1$ to propose a bill $b \in P$ but does not allow $c2$ to make a proposal. Instead, $c2$ can influence the policy only by making a speech of the form $\omega \in [a, b]$. The legislature is constrained to choose from the set $\{b, p_0\}$, where p_0 is the status quo policy. (3) The modified rule also allows both $c1$ and $c2$ to propose bills, b_1 and b_2. The legislature is constrained to choose among the policies $\{b_1, b_2, p_0\}$.

Strategies and Solution Concept

A strategy for committee i, $b_i(\omega)$, specifies a bill to propose for each state of nature. A strategy for the legislature $p(b_1, b_2)$ specifies a feasible policy for each pair of bills. Finally, the legislature forms a posterior distribution $g(b_1, b_2)$ over the state space.

We use exactly the same solution concept as Gilligan and Krehbiel (1987, 1989), that of legislative equilibrium, to determine the set of outcomes arising under the various rules.[3] Formally, strategies and beliefs, $(b_1^*(\omega), b_2^*(\omega), p^*(b_1, b_2), g^*(b_1, b_2))$, comprise a legislative equilibrium if (1) the legislature selects the policy that maximizes expected payoffs given beliefs; (2) each committee member chooses b_i to maximize payoffs, given $p^*(b_1, b_2)$; and (3) beliefs are formed using Bayes's rule wherever possible.

Equilibrium Selection Criterion

We choose the most informationally efficient equilibrium occurring under each rule. This is the natural criterion in the context of informational theories. Indeed, Gilligan and Krehbiel (1989, 461, emphasis in the original) write: "the primary and unique focus of the analysis is on the *informational efficiency* of rules."

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[3] A legislative equilibrium is the same as a perfect Bayesian equilibrium of a costless signaling game. See Banks 1991 for a detailed account of the use of signaling games in political science.

Because greater informational efficiency leads to a greater reduction in uncertainty, it is a collective good that is unanimously preferred.

We contend that the major implications of informational theory to date are derived by selecting equilibria under the different rules in an inconsistent manner. For instance, the conclusions of Gilligan and Krehbiel (1987) are based on comparing an informationally efficient equilibrium under the open rule to an informationally inefficient equilibrium under the closed rule. Likewise, the equilibria selected by Gilligan and Krehbiel (1989) for each of the rules (open, modified, and closed) are not the most informationally efficient.

Measuring Informational Efficiency

In order to obtain a measure of informational efficiency, we define the equilibrium outcome function as

$$X(\omega) \equiv p^*(b_1^*(\omega), b_2^*(\omega)) + \omega.$$

We can then write the expected utility of the legislature as

$$Eu_\ell = - \underbrace{\text{Var} X}_{\text{Informational}} - \underbrace{(EX)^2}_{\text{Distributional}},$$

where Var X, the variance of the random variable X, represents informational losses to the floor median, and $(EX)^2$ represents distributional losses. Likewise, for a committee member, let us say $c1$, we have:

$$Eu_{c1} = -E(x_c - X)^2$$

$$= -\underbrace{\text{Var} \ X}_{\text{Informational}} - \underbrace{(EX - x_c)^2}_{\text{Distributional}}. \quad (1)$$

The variance in outcomes is a measure of informational efficiency and can be used to compare both different equilibria under a given legislative rule and, for given equilibrium selections, the rules themselves.

Homogeneous Committees

The model of a homogeneous committee is identical to that above if one deletes player $c2$ from the game. Absent $c2$, the definitions of modified and closed rules are identical; hence, only two rules (open and closed) are considered in this environment.

HETEROGENEOUS COMMITTEES

In this section, we compare the informational properties of the open, modified, and closed rules in the case of heterogeneous committees.

Open Rule

With heterogeneous committees, when no rule restrictions are in place, it is possible to obtain full informational efficiency through the bills proposed by the committee.

Several definitions will prove helpful. Fix an equilibrium under the open rule. Given proposals b_1 and b_2 from committee members $c1$ and $c2$, respectively, we will say that the two committee members agree if there exists an ω such that $b_1^*(\omega) = b_1$ and $b_2^*(\omega) = b_2$. In other words, proposals b_1 and b_2 are consistent with the equilibrium. If there is no such ω, then the committee members are said to disagree. In that case, the legislature can be sure that at least one committee member has deviated from the equilibrium strategy.

PROPOSITION 1. *A legislative equilibrium under the open rule is*

$$b_1^*(\omega) = -\omega,$$

$$b_2^*(\omega) = \begin{cases} -\omega - 2x_c & \text{if } \omega \leq 1 - 2x_c \\ -\omega + 2x_c & \text{if } \omega > 1 - 2x_c \end{cases}$$

If c1 and c2 agree, then

$$p^*(b_1, b_2) = b_1.$$

If c1 and c2 disagree and $b_1, b_2 \in [-1, 0]$, then

$$p^*(b_1, b_2) = \begin{cases} b_1 & \text{if } u_{c2}(b_2 + \omega_1) > u_{c2}(b_1 + \omega_1) \\ b_2 & \text{if } u_{c2}(b_2 + \omega_1) \leq u_{c2}(b_1 + \omega_1) \end{cases},$$

where $\omega_1 = -b_1$ and $u_{c2}(x) = -(-x_c - x)^2$.
If c1 and c2 disagree, $b_i \notin [-1, 0]$, and $b_j \in [-1, -0]$, then

$$p^*(b_1, b_2) = b_j.$$

Otherwise,

$$p^*(b_1, b_2) = p_0.$$

The beliefs of the legislature are

$$g^*(b_1, b_2) = -p^*(b_1, b_2).$$

The expected utilities are

$$Eu_\ell = 0,$$

$$Eu_{c1} = -x_c^2 = Eu_{c2}.$$

Figure 1 illustrates the open rule equilibrium identified in proposition 1.

The strategies in proposition 1 can be explained intuitively as follows. The majority bills coincide with the legislature's ideal policy in each state. The minority bills, however, do not coincide with the ideal policy for the legislature and are not fully revealing. If the majority and minority members agree, that is, a state exists that would result in the bills proposed, then the legislature infers that both are telling the truth and adopts the majority bill.

In case of a disagreement, the legislature first hypothesizes that the majority is reporting accurately. Under this hypothesis, if the minority proposal is "self-serving," in the sense that adoption of its bill would benefit it relative to adoption of the majority bill, then the legislature accepts the working hypothesis that the majority is reporting accurately and adopts the majority bill. Under the same hypothesis, if the minority proposal is not deemed self-serving, the legislature

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FIGURE 1.   An Informationally Efficient Equilibrium under the Open Rule

Note: The horizontal axis measures the state of nature (ω)—known to committee members but not to the legislature. The vertical axis simultaneously measures bills proposed (b_1, b_2), policies adopted (p), and outcomes (x). The legislature's ideal outcome is zero. Since the outcome is simply the sum of the state and the policy, the legislature's ideal policy lies along the negative 45-degree line. The line segments in the figure denoted with asterisks are the bills, policies, and outcomes occurring in equilibrium. The figure depicts an equilibrium where, in each state, the majority proposes the legislature's ideal, which is then adopted, so the outcome, labeled x^*, is zero in every state. The minority proposes more extreme bills, which serve to discipline the majority. The discontinuous line, labeled b_2^*, depicts the minority's bills. The discontinuity occurs in state $\omega = 1 - 2x_c$, depicted on the horizontal axis, where the minority is indifferent between the legislature's ideal policy, $p = -1 + 2x_c$, and the lowest possible policy, $p = -1$, both depicted on the vertical axis.

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rejects the hypothesis that the majority is reporting accurately and adopts the minority bill. Notice that the minority has no incentive to induce disagreement because in exactly the circumstances in which the bill it proposes is advantageous, such a bill will be viewed as self-serving and not be adopted.

The argument that the majority also wishes to follow the equilibrium strategy is slightly more involved. In equilibrium, the majority never prefers the minority bill to its own, so it will never induce a disagreement that leads to adoption of the minority bill. Could the majority benefit by inducing a disagreement that leads to adoption of its own bill? That would require the minority bill to appear self-serving. When the minority

bill is lower than the legislature's ideal, it will be deemed self-serving only if the majority bill is also lower than the legislature's ideal. Since the majority's ideal is higher than the legislature's, this cannot be advantageous for the majority. When the minority bill is higher than the legislature's ideal, it will be deemed self-serving only if the majority bill is even higher. Notice, however, that the bill the minority proposes in these situations is already so high that the majority is just indifferent between it and the legislature's ideal. Thus, proposing an even higher bill cannot be advantageous for the majority.

In equilibrium, the majority bill is always adopted, so it seems the minority has little influence. Yet, the

minority bills have an important preventive role to play, since they act to discipline the majority and are the key to generating full informational efficiency.[4]

It is useful to contrast the fully revealing equilibrium in proposition 1 with the partially revealing equilibrium constructed by Gilligan and Krehbiel (1989) in their proposition 1.[5] To facilitate the comparison, consider an example in which $x_c = \frac{1}{8}$. In the Gilligan and Krehbiel construction, the legislature is able to obtain its ideal policy only when the state is extreme, either very small ($\omega < \frac{1}{4}$) or very large ($\omega > \frac{3}{4}$). In intermediate states, a single default policy ($b = -\frac{1}{2}$) is adopted. Their equilibrium is sustained as follows. When the states are extreme, both the majority and the minority propose their ideal bills, $b_1 = -\omega + \frac{1}{8}$ and $b_2 = -\omega - \frac{1}{8}$, respectively. Because the difference in two ideal bills is always the same ($b_1 - b_2 = \frac{1}{4}$), the legislature is able to infer that they "agree." The legislature then splits the difference between these two bills by amending one of them to policy $-\omega$, which is also the legislature's ideal policy. In moderate states, the majority and minority propose bills chosen randomly from $[-\frac{7}{8}, \frac{1}{8}]$ and $[-\frac{9}{8}, -\frac{1}{8}]$, respectively. This results in a disagreement almost always, in which case the legislature infers only that the state is between $\frac{1}{4}$ and $\frac{3}{4}$. It is then optimal for the legislature to adopt the default bill.

Notice that when the state is sufficiently extreme ($\omega < \frac{1}{4}$ or $\omega > \frac{3}{4}$), both the minority and the majority prefer the legislature's ideal policy, $-\omega$, to the default bill, $-\frac{1}{2}$. For more moderate states ($\omega \in [\frac{1}{4}, \frac{3}{4}]$), at least one of the committee members prefers the default bill to $-\omega$ and so chooses to send a bill at random. Thus, the legislature's ideal policy is adopted only half the time. The (residual) variance in outcomes is $\frac{1}{96}$.

In our construction, the strategies specified in proposition 1 require that the minority propose the bill $-\omega - \frac{1}{4}$ if $\omega \leq \frac{3}{4}$ and propose $-\omega + \frac{1}{4}$ if $\omega > \frac{3}{4}$. In every state, ω, the majority always proposes the bill $-\omega$. Two key features generate full revelation in this equilibrium. First, in every state ω the majority never prefers the minority bill to $-\omega$. Second, the minority bill is not adopted if it is viewed as self-serving. In the example, a minority bill is self-serving if and only if $b_1 > b_2 > b_1 - \frac{1}{4}$, that is, when the minority bill is lower than the majority bill, but not by too much. We have already explained why neither member has any incentive to deviate. In equilibrium, the legislature's ideal policy is always adopted, and the (residual) variance in outcomes is 0.

Our construction differs in several important respects. First, it is intuitive. The legislature simply adopts the bill proposed by the majority. The minority bill (mostly) is biased in the direction that the minority favors. In the event of a disagreement, it seems likely that the legislature may try to judge the motives of the

minority using the yardstick set by the majority and decide which bill to adopt on this basis. In addition, the bills proposed by the committee in our construction are neither random nor extreme, that is, outside the set of policies the legislature might conceivably wish to adopt, $[-1, 0]$.

Second, the preceding arguments do not make explicit use of the particular structure of the model. Our construction above is for the case in which the committee members are symmetrically opposed to one another, but it is easy to verify that a similar construction is valid for all values of x_{c1} and x_{c2}, such that $x_{c1} - x_{c2} < \frac{1}{2}$. Moreover, we do not rely on the assumption that the distribution of states is uniform or that preferences are quadratic. Indeed, one can show that the legislature, by using the self-serving yardstick to judge majority and minority bills, can successfully induce full informational efficiency in a more general set-up.

A key feature of our construction is that the legislature is flexible in resolving disagreements among the committee members. That is, in the event of disagreement, the bill adopted by the legislature depends on the nature of the disagreement. This flexibility is essential to obtaining full informational efficiency.

The equilibrium described in proposition 1 leads to the outcome 0 in every state, so it follows that this is the best equilibrium for the legislature. But notice that $EX = 0$ for all equilibria under the open rule. This is because in any equilibrium, following any pair of bills b_1, b_2, the legislature must choose $p^*(b_1, b_2) = -E(\omega|b_1, b_2)$, the expectation of $-\omega$ conditional on the signals. Because, in equilibrium, the beliefs are formed using Bayes's rule, this implies that $EX = 0$. Thus, all equilibria arising under open rules are optimal from a distributional perspective. Combining this with the efficiency properties of the equilibrium in proposition 1 implies that this equilibrium is unanimously preferred to all others arising under the open rule. Finally, we have:

COROLLARY 1. *The equilibrium described in proposition 1 is unanimously preferred to all equilibria under the open rule.*

Closed Rule

In the previous section we showed that under the open rule full informational efficiency can be achieved. Here we show that the additional restrictions imposed by the closed rule preclude such outcomes. In particular, for a nontrivial interval of states, the status quo policy is chosen even though it is not the ideal policy for any of the parties.

In contrast to proposition 1, we have:

PROPOSITION 2. *Every equilibrium under the closed rule is informationally inefficient.*

The intuition for this result is that when the status quo policy coincides with the ideal policy of the majority member, he can guarantee this by proposing $b = p_0$. In other words, the majority chooses to kill the proposed legislation in committee, which leaves the

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[4] It is intuitive that the minority can discipline the majority by proposing very extreme bills. In our construction, however, the minority bills are not too extreme: They all lie within $2x_c$ of the legislature's ideal policy $-\omega$.

[5] The reader may wish to compare our Figure 1 with Figure 1 in Gilligan and Krehbiel 1989, 470.

status quo as the only option available to the legislature. When the status quo policy is lower than the ideal policy for the majority, disagreement between the majority and the legislature leads the majority to kill that bill as well. It is not in the interest of the majority to propose a policy lower than the status quo, and it is not in the interest of the legislature to adopt a policy higher than the status quo. Hence, there is an interval in which neither party gets its ideal policy.

We next show that informational losses are unavoidable under the closed rule, but distributional losses are not. Fix an equilibrium under the closed rule. Given a bill b from $c1$ and a speech s from $c2$, we will say that the two committee members agree if there exists an ω such that $b = b^*(\omega)$ and $s \subseteq s^*(\omega)$. In other words, b and s are consistent with the equilibrium. If there is no such ω, then the committee members are said to disagree. If there is a disagreement, then the legislature can be sure that at least one committee member has deviated from the equilibrium strategy.

PROPOSITION 3. *A legislative equilibrium under the closed rule is:*

$$b^*(\omega) =$$

$$\begin{cases} -\omega & \text{if } \omega \leq -2x_c - p_0 \\ -2\omega + (-2x_c - p_0) & \text{if } -2x_c - p_0 \leq \omega \leq -x_c - p_0 \\ p_0 & \text{if } -x_c - p_0 < \omega < x_c - p_0 \\ -2\omega + (2x_c - p_0) & \text{if } x_c - p_0 \leq \omega \leq 2x_c - p_0 \\ -\omega & \text{if } \omega \geq 2x_c - p_0 \end{cases} ;$$

$$s^*(\omega) = -b^*(\omega).$$

If $c1$ and $c2$ agree, then

$$p^*(b, s) = b.$$

If $c1$ and $c2$ disagree, then

$$p^*(b, s) = p_0.$$

The beliefs of the legislature are

$$g^*(b, s) = \begin{cases} -b & \text{if } b = -s \neq p_0 \\ U[-x_c - p_0, x_c - p_0] & \text{if } b = -s = p_0 \\ -p_0 & \text{if } b \neq -s \end{cases}.$$

The expected utilities are

$$Eu_\ell = -\frac{4}{3} x_c^3,$$

$$Eu_{c1} = -\frac{4}{3} x_c^3 - x_c^2 = Eu_{c2}.$$

Figure 2 illustrates the equilibrium described in proposition 3.

The strategies used in proposition 3 can be understood as follows. If both committee members prefer the ideal policy of the legislature to the status quo, then the ideal policy is proposed by the majority, supported by the minority, and adopted. If only the majority (minority) prefers the status quo to the ideal policy, then a compromise bill, b, such that the majority (minority) is indifferent between b and the status quo, is proposed

by the majority, supported by the minority, and adopted. If the compromise bill is worse than the status quo for either the majority or the minority, then the bill is killed in committee. A feature of the equilibrium is that all bills are supported by the minority, that is, "cosponsored."

Again, it is useful to compare the equilibrium in proposition 3 to the equilibrium under the closed rule constructed by Gilligan and Krehbiel.[6] The two constructions are not too dissimilar, but in the Gilligan and Krehbiel equilibrium the majority party is able to wield its power and obtain its ideal policy a significant proportion of the time. As an example, if $x_c = \frac{1}{8}$ and $p_0 = -\frac{1}{2}$, then the majority's ideal policy results half the time (when $\omega < \frac{1}{8}$ or when $\omega > \frac{5}{8}$), but the ideal policy of the legislature is never realized. The fact that the majority wields such power in the Gilligan and Krehbiel equilibrium leads to significant distributional losses relative to our equilibrium. In addition, there are also informational losses. Continuing with the example, in the Gilligan and Krehbiel equilibrium the variance of outcomes is $\frac{5}{768}$, whereas the distributional losses are $\frac{1}{256}$.

In contrast, in the equilibrium of proposition 3, the ideal policy of the legislature is adopted half the time (when $\omega < \frac{1}{4}$ or when $\omega > \frac{3}{4}$), and the ideal policy of the majority is never realized. In addition, the frequency with which legislative compromise tilts in favor of the majority is exactly the same as the frequency with which it tilts in favor of the minority. Thus, although there are distributional effects in specific instances, these exactly offset each other. As a result, there are no distributional losses, and the variance of outcomes is $\frac{1}{216}$. Thus, on both informational and distributional grounds this equilibrium dominates the Gilligan and Krehbiel equilibrium.

A key feature of all equilibria under the closed rule is a substantial amount of inertia; that is, the ideal policy for the legislature must diverge widely from the status quo for any change in policy to take place. For instance, in the example studied above, there is no change in policy from the status quo for all states $\omega \in [\frac{3}{8}, \frac{5}{8}]$.

Our next result shows that from the perspective of the legislature the equilibrium constructed in proposition 3 dominates all equilibria under the closed rule.

PROPOSITION 4. *The legislative equilibrium identified in proposition 3 is the best for the legislature among all equilibria under the closed rule.*

Under the restrictive closed rule, the legislature cedes substantial power to the majority member on the committee. As proposition 2 demonstrates, this inevitably leads to informational losses. But this need not necessarily lead to distributional losses: In the equilibrium of proposition 3 there are none. As a consequence, the most informative equilibrium under the open or modified rule is unanimously preferred to the above equilibrium under the closed rule.

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[6] The reader may wish to compare our Figure 2 with Figure 3 in Gilligan and Krehbiel 1989, 479.

FIGURE 2.   An Equilibrium under the Closed Rule

Note: The horizontal axis measures the state of nature (ω)—known to committee members but not to the legislature. The vertical axis simultaneously measures majority bills (b), policies adopted (p), and outcomes (x). Notice that under the closed rule, only the majority can propose bills. The status quo policy (p_0) is also depicted on the vertical axis and is set equal to negative one-half. The legislature's ideal outcome is zero. The line segments in the figure denoted with asterisks are the bills, policies, and outcomes occurring in equilibrium. The figure depicts an equilibrium where, in extreme states (close to zero or close to one), the majority proposes the legislature's ideal. In states close to one-half, the status quo is maintained. The status quo policy, p_0, depicted on the vertical axis, first gets adopted in state $\omega = -x_c - p_0$, depicted on the horizontal axis. In intermediate states, the majority proposes a "compromise" policy midway between the legislature's ideal and the status quo. In the figure, these compromise policies begin in state $\omega = -2x_c - p_0$, depicted on the horizontal axis, where the minority is indifferent between the legislature's ideal policy, $p = p_0 + 2x_c$, and the status quo policy, p_0, both depicted on the vertical axis. The majority's proposals are always adopted. The dark line, labeled x^*, depicts the resulting outcomes.

Modified Rule

We have demonstrated that under the open rule full informational efficiency is possible (proposition 1) and that under the closed rule it is impossible (proposition 2). Together these seem to suggest that restrictive rules also restrict the amount of information transmitted from the committee to the legislature. Our next result shows, however, that this view is mistaken: Under the restrictive modified rule, full informational efficiency can be achieved.

PROPOSITION 5. *Fix any status quo p_0. The legislative equilibrium under the open rule described in proposi-*

tion 1 is also a legislative equilibrium under the modified rule.

Proposition 5 follows immediately from an examination of the equilibrium strategies given in proposition 1: The strategy of the legislature always consists of a choice from the set $\{b_1, b_2, p_0\}$, thus conforming to the modified rule.

It is useful to contrast the restrictions on possible policies under the closed rule with those under the modified rule. Under the closed rule, in the event of a disagreement between committee members, the only recourse of the legislature is to adopt the status quo,

TABLE 1. Legislature's Preferences over Rules with a Heterogeneous Committee		
Rank	Informational	Distributional
1.	Open/Modified	Open/Modified/Closed
2.	Closed	—

which leads to the "inertia" observed under the closed rule. In contrast, under the modified rule, the legislature can choose the minority proposal in the event of a disagreement. As proposition 1 shows, this additional flexibility is all that is necessary to generate full efficiency.

A Comparison of Rules

Using the expected utility calculations in propositions 1, 3, and 5, we obtain the ranking in Table 1 among the equilibria under each of the rules.

We see that, in contrast to the trade-off illustrated in Gilligan and Krehbiel (1989), the open and modified rules dominate the closed rule according to both informational and distributional criteria. Moreover, regardless of the status quo action, modified rules are just as effective as open rules and superior to completely closed rules in terms of informational efficiency, regardless of the degree of preference divergence.

HOMOGENEOUS COMMITTEES

We now examine informational efficiency for the case of homogeneous committee preferences, that is, the committee may be represented by a single pivotal actor. In this case, the modified and closed rules are the same, so we compare only the open and closed rules.

Open Rule

Under the open rule, the Gilligan and Krehbiel (1987) model is isomorphic to the costless signaling model of Crawford and Sobel (1982). That is, all legislative equilibria arising under the open rule have only a finite number of equilibrium policies (see lemma 1 of Crawford and Sobel 1982) and thus are informationally inefficient.

COROLLARY 2. *Every equilibrium under the open rule with a homogeneous committee is informationally inefficient.*

Theorem 3 of Crawford and Sobel (1982) characterizes the most informationally efficient equilibrium under the open rule with homogeneous committees. For future reference, we state this as follows.

PROPOSITION 6. *The most informationally efficient equilibrium under the open rule with a homogeneous committee results in the following expected utilities:*

$$Eu_\ell = -\frac{1}{12N^2} - \frac{x_c^2(N^2 - 1)}{3},$$

$$Eu_c = -\frac{1}{12N^2} - \frac{x_c^2(N^2 - 1)}{3} - x_c^2,$$

where N is the smallest positive integer greater than or equal to $(-\frac{1}{2} + \frac{1}{2}\sqrt{(1 + \frac{2}{x_c})})$.

As is the case under open rules with heterogeneous committees, no equilibrium entails distributional losses. The one that is most informationally efficient is also unanimously preferred by all parties.

Closed Rule

We now compare the information content under the open rule relative to the closed rule. Notice that proposition 2 holds as well for a homogeneous committee.

PROPOSITION 7. *Every equilibrium under the closed rule with a homogeneous committee is informationally inefficient.*

Because both open and closed rules lead to informational inefficiencies, we must examine the informativeness of equilibria under the closed rule as compared to the open rule.

PROPOSITION 8. *Suppose $3x_c \le -p_0 \le 1 - x_c$. With a homogeneous committee, a legislative equilibrium under the closed rule is:*

$$b^*(\omega) =$$

$$\begin{cases} -\omega + x_c & \text{if } 0 \le \omega \le -3x_c - p_0 \\ 4x_c + p_0 & \text{if } -3x_c - p_0 \le \omega \le -2x_c - p_0 \\ 2x_c + p_0 & \text{if } -2x_c - p_0 < \omega < -p_0 \\ p_0 & \text{if } -p_0 \le \omega \le x_c - p_0 \\ -\omega + x_c & \text{if } x_c - p_0 \le \omega \le 1 \end{cases}$$

$$p^*(b) =$$

$$\begin{cases} b & \text{if } b \notin (p_0, 2x_c + p_0) \cup (2x_c + p_0, 4x_c + p_0) \\ p_0 & \text{otherwise} \end{cases}$$

The beliefs of the legislature are:

$$g^*(b) =$$

$$\begin{cases} x_c - b & \text{if } 4x_c + p_0 < b \le x_c \\ U([-3x_c - p_0, -2x_c - p_0]) & \text{if } b = 4x_c + p_0 \\ U([-2x_c - p_0, -p_0]) & \text{if } b = 2x_c + p_0 \\ U([-p_0, -p_0 + x_c]) & \text{if } b = p_0 \\ x_c - b & \text{if } -1 + x_c \le b < p_0 \\ -p_0 & \text{otherwise} \end{cases}$$

The expected utilities are

$$Eu_\ell = -\frac{4}{3}x_c^3 - x_c^2,$$

$$Eu_c = -\frac{4}{3}x_c^3.$$

Figure 3 illustrates the closed rule equilibrium identified in proposition 8.

FIGURE 3. An Equilibrium under the Closed Rule with a Homogeneous Committee

Note: The horizontal axis measures the state of nature (ω)—known to the committee but not to the legislature. The vertical axis simultaneously measures committee bills (b), policies adopted (p), and outcomes (x). The status quo policy (p_0) is also depicted on the vertical axis. The legislature's ideal outcome is zero while the committee's ideal outcome is x_c. The line segments in the figure denoted with asterisks are the bills, policies, and outcomes occurring in equilibrium. The figure depicts an equilibrium under the closed rule where, in extreme states (close to zero or close to one), the committee proposes its ideal policy. In intermediate states, the committee proposes one of three bills, one of which is the status quo. The discontinuous line with three steps, labeled b^*, depicts the committee proposals, which are always adopted. In the figure, the first discontinuity occurs in state $\omega = -2x_c - p_0$, depicted on the horizontal axis, where the committee is indifferent between the policy, $p = p_0 + 4x_c$, and the legislature's ideal policy, $p_0 + 2x_c$, both depicted on the vertical axis. The second discontinuity occurs in state $\omega = -p_0$, depicted on the horizontal axis, where the committee is indifferent between the policy, $p = p_0 + 2x_c$, and the legislature's ideal policy, which happens to be the status quo p_0, both depicted on the vertical axis. The dark line, labeled x^*, depicts the resulting outcomes.

The intuition for this result is that, when the legislature's ideal policy diverges substantially from the status quo, the legislature is willing to cede power to the committee and exchange distributional losses for informational gains. In these circumstances, the committee proposes its own ideal policy; confronted with the choice between this and the status quo, the legislature adopts the proposed bill. When the legislature's ideal policy is close to the status quo, the committee cannot force the legislature to accede to its demands. The situation becomes much more complex, and in equilibrium the committee proposes one of three policies. In particular, when the status quo lies between the legislature's ideal policy and the committee's ideal

policy, no compromise can be reached, so no new policy is adopted.

This equilibrium is similar to the one constructed by Gilligan and Krehbiel (1987, proposition 5). Their construction differs from ours in that for states between $-p_0 - 3x_c$ and $-p_0 + x_c$ only two policies are chosen. As a consequence, the informational efficiency of the equilibrium in our proposition 8 is higher than in the Gilligan and Krehbiel equilibrium. At the same time the distributional losses in the two equilibria are the same. As a result, the equilibrium derived here is unanimously preferred to the Gilligan and Krehbiel equilibrium. More concretely, consider the case in which $x_c = \frac{1}{8}$ and $p_0 = -\frac{1}{2}$. The distributional losses

TABLE 2. Legislature's Preferences over Rules with a Homogeneous Committee			
Rank	Informational	Distributional	Overall
1.	Closed	Open	Closed
2.	Open	Closed	Open

in both the Gilligan and Krehbiel equilibrium and the equilibrium in proposition 8 amount to $\frac{1}{64}$. The latter is superior on informational grounds, however, with informational losses of $\frac{1}{384}$ compared to losses of $\frac{1}{96}$ in the Gilligan and Krehbiel equilibrium.

Comparison of Rules

It appears to be difficult to characterize explicitly the most informative legislative equilibrium under the closed rule with a homogeneous committee. In particular, we do not know whether the equilibrium in proposition 8 is the most informative. Nevertheless, a comparison shows that for all values of $x_c < \frac{1}{4}$, this closed rule equilibrium is more informative than any equilibrium under the open rule. Furthermore, the following proposition shows that this is preferred by the legislature to any equilibrium under the open rule.

PROPOSITION 9. *Suppose $x_c - 1 \leq -p_0 \leq 3x_c$. With a homogeneous committee, the equilibrium in proposition 8 is informationally superior to all open rule equilibria. Furthermore, all open rule equilibria are worse for the legislature than the equilibrium in proposition 8.*

The conclusion of proposition 9 alters the results of Gilligan and Krehbiel (1987), who find the closed rule superior to the open rule if and only if the committee is not composed of "preference outliers," that is, x_c is not too large. With preference outliers, the distributional losses to the legislature exceed any informational gains in selecting the closed rule over the open rule. We find that the potential informational gains under the closed rule are, in fact, greater than in the Gilligan and Krehbiel (1987) equilibrium. As a consequence, informational gains always more than offset distributional losses, and the closed rule is superior even with preference outliers. We summarize these findings in Table 2.[7]

SPECIALIZATION

Thus far we have compared the informational efficiency of different legislative rules under the assumption that committee members are informed. An additional basis for comparing rules is the incentives that each rule provides for the committee to become informed about the implications of policies on outcomes. Information gathering is expensive: The committee staff does research, cost-benefit studies are conducted,

hearings are held, and so on. Legislative rules differ in the efficiency and distributional gains that accrue to the committee, so it may well be that under one rule a committee chooses to become informed, but under another rule it does not. Clearly, the efficiency losses due to remaining uninformed are likely to be severe. To study these effects, we examine the efficiency and distributional gains from specialization under each rule for heterogeneous and homogeneous committees.[8] In this section, we suppose that the status quo reflects the fact that the legislature is completely uninformed, that is, $p_0 = -\frac{1}{2}$. We further suppose that the conditions required for proposition 8 hold; in particular, $3x_c \leq -p_0$, so $x_c \leq \frac{1}{6}$.

Homogeneous Committees

With specialized committees, we showed that closed rules lead to greater efficiency as well as distributional gains to the committee as compared to the open rule. When the committee is unspecialized, the best the legislature can do under each rule is simply maintain the status quo; thus, there are no differences, either in efficiency or distributional effects, between the two rules. As a consequence, the closed rule provides a strictly greater incentive to specialize than does the open rule. This is not to say that the committee will specialize whenever it is socially desirable to do so. Because efficiency gains accrue to both the committee and the legislature, the committee's specialization decision does not take into account all the net social gains from specialization. In other words, in some circumstances the committee will not choose to specialize even though it is socially desirable to do so. Moreover, the committee's decision to specialize takes into account distributional gains to itself but not distributional losses to the legislature. In other words, in some circumstances a committee will choose to specialize even though it is socially undesirable to do so. As a result, although a closed rule provides the best incentives to specialize, it does not align private and social incentives.

Heterogeneous Committees

In heterogeneous committees, the exact nature of the process by which committee members become informed affects the incentives to specialize. It is useful to distinguish two cases. If the information is acquired publicly as a result of committee hearings or general staff reports, then all committee members are informed. If the information is acquired privately as a result of staff working exclusively for either the majority or minority, then it is possible that one side of the committee is informed but the other is not.

Public Information. When information is publicly acquired, the decision to become specialized is committee-wide, that is, either every member or none become

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[7] Because the distributional losses under the open rule are zero, the losses under the best legislative equilibrium will always be (weakly) larger.

[8] We omit the formal analysis underlying the results reported in this section, but it may be obtained from the authors.

specialized. As we showed previously, equilibria with specialized committees under the open, modified, and closed rules do not differ in their distributional characteristics (indeed, all three rules lead to no distributional gains to either the majority or minority), but the open and modified rules lead to greater efficiency gains compared to the closed rule. In contrast, when the committee is unspecialized, the best the legislature can do under each rule is simply maintain the status quo; hence there are no differences, either in efficiency or distributional effects, among the rules. As a consequence, the open and modified rules provide strictly greater incentives to specialize than does the closed rule. This happens because there are greater efficiency gains under the open or modified rule, and the committee suffers no reductions in distributional gains relative to the unspecialized case. Because distributional effects are absent, it is never the case that a committee operating under these rules will choose to specialize, even when it is socially undesirable to do so. Of course, since the committee's specialization decision does not account for the full efficiency gains accruing to society, it is still the case that it may be socially desirable to specialize, but the committee will choose not to do so.

Private Information. When information is privately acquired, the decision to specialize is taken separately by the majority and the minority.

First, consider the majority's specialization decision. If the majority anticipates that the minority will remain unspecialized, the strategic situation is the same as for a homogeneous committee. This is obvious under the open and closed rules. Under the modified rule, the unspecialized minority could, in principle, actively participate in shaping the legislative outcome. Yet, one can show that such participation can only be detrimental on informational grounds. Thus, the incentives to specialize under the modified rule are identical to those under the closed rule in the homogeneous case. Therefore, the closed and modified rules provide the best incentives for the majority to specialize when the minority remains unspecialized.

Next, suppose the majority anticipates that the minority will become informed. If the majority remains unspecialized, then, under the open rule, the outcome is the same as in the homogeneous case. Under the modified rule, the unspecialized majority could, in principle, be an active participant. Once again, this can only have deleterious informational consequences, so the most informative outcome is the same as under the closed rule in the homogeneous case. Under the closed rule, however, such active participation by the unspecialized majority is beneficial. A comparison of these outcomes to the outcomes when both are specialized determines the majority's specialization incentives. We find that when preference divergence is large, the adverse distributional effects associated with giving power to the minority dominate; hence, the modified rule provides the best incentives to specialize. When preferences are less divergent, the relatively larger efficiency gains associated with the closed rule domi-

nate, and it provides the best incentives for the majority to specialize.

Second, consider the minority's specialization decision. If the minority anticipates that the majority will remain unspecialized, then its gains from specialization under the open rule are identical to those under the open rule in the homogeneous case. For reasons given above concerning the majority's incentives, the gains to the minority from specialization under the modified rule are identical to those under the closed rule in the homogeneous case. Under the closed rule, there are some limited gains from specialization to the minority—it can work with the unspecialized majority to overturn the status quo when that serves their mutual best interest. We find that the modified rule provides the best incentives for the minority to specialize when the majority is unspecialized.

Finally, suppose the minority anticipates that the majority will specialize. If the minority remains unspecialized, then, under the closed or modified rules, the outcome is the same as with a closed rule in the homogeneous case. Likewise, the open rule coincides with the homogeneous case. We find that when preference divergence is large, the distributional losses from the modified rule dominate, and it provides the best incentives to specialize. When preference divergence is relatively small, the large efficiency gains of the open rule dominate, and it provides the best incentives.

To recapitulate, the modified rule provides the best incentives for both sides to specialize when preferences are divergent. When preferences are less divergent, there is a trade-off among the rules. The closed rule provides the best incentives for the majority to specialize, whereas the open rule provides the minority with the best incentives. How this is resolved, of course, depends on the relative costs of the different sides in becoming specialized.

EMPIRICAL IMPLICATIONS

What are the empirical implications of the informational theory? Is the theory consistent with data on rules assignment in the House? There has been a lively debate regarding these questions (Dion and Huber 1996, 1997; Krehbiel 1991, 1997a, 1997b; Sinclair 1994). In this section we revisit some of the issues and examine the consequences of our results.

At the most general level, the theory postulates that restrictive rules are more likely to be observed when the legislature's expected utility in equilibrium is greater under, say, a closed rule as compared to an open rule. A weak postulate of the informational theory relates the probability of observing a restrictive rule to the difference in payoffs resulting from using such a rule. More precisely,

$$\text{Prob [closed]} = f(Eu_I^{\text{closed}} - Eu_I^{\text{open}}), \qquad (2)$$

where f is an increasing function. Expected utilities, of course, cannot be observed in the data, but the theory indicates relationships between other observable variables and the difference in expected utilities. One such

variable is the degree of preference divergence, x_c, and the other is the degree of heterogeneity in committee preferences.

Reduced form models in this area typically seek to explain the occurrence of restrictive rules by using, among others, explanatory variables that measure (or act as a proxy for) preference divergence and heterogeneity.

Different informational theory models lead to differing predictions about how x_c affects utilities, $\Delta_I \equiv Eu_I^{\text{closed}} - Eu_I^{\text{open}}$, and then, via equation 2, the probability that a restrictive rule will be used.[9] As we show below, heterogeneity plays an important role in the predictions of the effects of preference outliers.

In the case of a heterogeneous committee, our results predict that the relationship between the difference in utilities and the preference divergence x_c is unambiguously negative (more precisely, $(\partial/\partial x_c)(\Delta_I) < 0$, for all x_c). This, combined with equation 2, implies that one should expect to see restrictive rules more often when x_c is small. In other words, outliers should be given less restrictive rules. In the case of a homogeneous committee, $(\partial/\partial x_c)(\Delta_I) < 0$, if and only if x_c is small (precisely, if $x_c < \frac{1}{12}$).

In almost all the models estimated by Krehbiel (1991, 1997a) and Dion and Huber (1997), the preference divergence variable has a significant negative effect. This finding does not seem to be affected by alternative measures of preference divergence: continuous (x_c) or binary (outlier). The only exception is that in some models estimated by Dion and Huber (1996) the effect of the outlier variable is insignificant.

In addition to the effect of heterogeneity on the relation between preference divergence and restrictiveness, our results predict that the gain from using a restrictive closed rule versus an open rule is greater when committees are homogeneous rather than heterogeneous, that is, $\Delta_I^{\text{hom}} > \Delta_I^{\text{het}}$. This prediction should be treated with some care, however, since it involves holding the degree of preference divergence fixed. Thus, it involves a comparison between a homogeneous committee with preference parameter x_c and a heterogeneous committee in which the majority and minority have preference parameters of x_c and $-x_c$, respectively. The somewhat restrictive nature of this prediction means that it is rather difficult to submit this to a convincing empirical test.

Empirical evidence on how heterogeneity variables affect rule assignments is mixed. The signs of the effects seem to depend closely on how heterogeneity and preference divergence are measured. Dion and Huber (1997) interact binary outlier and heterogeneity measures and find that heterogeneity is generally associated with less restrictive rules. Krehbiel (1991, 1997a) uses continuous measures of both preference divergence and heterogeneity and finds the opposite effect,

although for several specifications the effect of heterogeneity is not statistically significant. For our purposes, an ideal empirical test would combine a continuous measure of divergence with a discrete heterogeneity measure.

Informational theory also suggests that the greater the degree of specialization of a committee, the more likely are restrictive rules. As our results show, however, this hypothesis is sensitive to whether the committee is heterogeneous. Homogeneous specialized committees should receive closed rules, whereas heterogeneous specialized committees should receive modified rules.

Overall, the empirical evidence is not inconsistent with these predictions. The proxy variables used for specialization, the number of laws cited by the committee, and the seniority of committee members are generally associated with more restrictive rules. Krehbiel (1991, 1997a) also finds that the specialization variables have generally positive effects, but these are not always statistically significant. According to Sinclair (1994), "laws cited" has a positive and significant effect. Dion and Huber (1997) also find that variable positive and significant, whereas the committee seniority variable has a negative but insignificant coefficient.

Our work highlights the importance of distinguishing between somewhat restrictive modified rules and more restrictive closed rules. We showed in proposition 5 that modified rules are fully informationally efficient in sharp contrast to closed rules. Moreover, when committees are heterogeneous, the modified rule provides the best overall incentives for them to specialize. All the empirical studies of which we are aware, however, give no special distinction to modified rules—a bill is coded as either restrictive (closed) or not. Indeed, in the coding convention followed by most researchers, modified rules (as studied here) and closed rules are lumped together. Our results suggest that intermediate degrees of restrictiveness can matter a great deal to informational efficiency. This suggests that the conclusions regarding the empirical support for informational theories may depend on whether modified rules are coded as restrictive. In our view, it would be useful to recode rule-bill observations in such a way as to distinguish between unrestrictive open rules, somewhat restrictive modified rules, and more restrictive closed rules.

CONCLUSION

In the so-called institutional design controversy, two rival theories, the distributive and informational, seek to explain the design of legislative institutions.

The *distributive* theory, as expounded by, for instance, Weingast and Marshall (1988), postulates that the committee system is set up to facilitate the exchange of influence over policy choices. Legislators effectively bid for membership in committees with jurisdiction over issues they perceive as most important for their reelection. Appointment to a particular committee endows the members with "rights" to influence policy in that area, and the seniority system preserves

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[9] In practice, it is unrealistic to expect an exact measurement of x_c. Rather, in many cases, a binary classification is developed, depending on whether x_c is above or below some threshold value. If x_c is large, which indicates substantial preference divergence, then the committee is classified as consisting of outliers.

those rights. Committee members appropriate rents in their areas of interest, and the outcomes may be socially inefficient. Restrictive rules that limit the power of the legislature to amend proposed bills allow committee members to appropriate the rents they are "due" by virtue of their appointments.

Whereas distributive theory views the committee system as a means of rent appropriation and exchange, informational theory views the system as a means of specialization and information transmission. Committee composition reflects this theoretical difference. In particular, distributive theory predicts that committees are more likely to be composed of preference outliers and receive restrictive rules. Our analysis indicates that preference divergence, per se, is not harmful to informational efficiency. In heterogeneous committees, full efficiency can be achieved under open or modified rules regardless of preference divergence. Thus, significant divergence, as predicted by distributive theory, need not have harmful informational effects, provided rules are not too restrictive.

Distributive theory also predicts that committees are likely to be composed of members with similar (homogeneous) preferences. Informational theory predicts that homogeneous committees will be associated with informational losses. Thus, there is a trade-off between a committee optimally composed for distributive purposes and one composed for informational purposes. In some instances, the legislature may be able to overcome this trade-off by making multiple referrals to separate committees. That is, two homogeneous committees with opposing preferences have jurisdiction over the bill. In that case, a system of multiple referrals to two homogeneous committees is strategically equivalent to a system of a single referral to one heterogeneous committee. As long as the rules permit the legislature the choice between the bills proposed by the two committees, mimicking the modified rule, full informational efficiency can be achieved.

Informational theory shows that full efficiency not only can be achieved but also can be achieved in a way that ensures no distributional costs. In other words, committee members cannot extract any rents from the legislative process; the policy outcomes are those most favored by the legislature. Under the open rule, because the legislature retains all power to amend bills, it is not too surprising that the committee is unable to appropriate any rents. Under the modified rule, however, the legislature cedes substantial power to the committee, yet the committee is still unable to appropriate any rents. As explained earlier, the equilibrium constructed in proposition 1 exploits the fact that committee preferences are opposed; the preferences of the majority and minority straddle those of the legislature.[10] This allows the legislature to extract all the information in such a way that the chosen bill is always the one that emerges from committee, and this is true

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[10] The fact that they are symmetrically opposed and exactly balance each other is not important. Propositions 1 and 5 continue to hold even if committee preferences are asymmetric. What is important is that they lie on either side of the legislature's preferences.

both in and out of equilibrium. Stated another way, the committee is unable to exploit its power under the modified rule because positive rents to one side mean negative rents to the other. Even though the modified rule is restrictive, the fact that it allows both sides to propose bills gives the legislature enough strategic flexibility to play off one against the other, so full efficiency can be achieved. In contrast, the restrictive closed rule allows only the majority to propose bills, which leaves the legislature with almost no room to maneuver. As a result, under the closed rule, full efficiency cannot be achieved.

APPENDIX

Proof of Proposition 1

We show that none of the players can gain by deviating from the prescribed strategies.

Note that the legislature is always optimizing, given its beliefs, and these beliefs are consistent with Bayes's rule along the equilibrium path. We only need to consider deviations by $c1$ and $c2$.

We will say that $c2$'s proposal is self-serving if $u_{c2}(b_2 + \omega_1) > u_{c2}(b_1 + \omega_1)$, where $\omega_1 = b_1^{*-1}(b_1) = -b_1$. In other words, $c2$'s proposal is self-serving if b_2 is more profitable for $c2$ than b_1, under the hypothesis that $c1$ is "telling the truth," and hence the state is $\omega_1 = -b_1$. It is easy to verify that b_2 is self-serving if and only if $b_1 > b_2 > b_1 - 2x_c$.

First, suppose $c2$ follows the prescribed strategy. We argue that $c1$ has no profitable deviations.

If $\omega \leq 1 - 2x_c$ but $c1$ deviates to a b_1 such that $b_1 > b_1^*(\omega)$, then $c2$'s proposal is not viewed as self-serving, and the resulting policy $b_2^*(\omega)$ is even lower than $-\omega$. This is disadvantageous for $c1$. If $c1$ deviates to a b_1 such that $b_1 < b_1^*(\omega)$, then whether $c2$'s proposal will be viewed as self-serving depends on b_1. If it is so viewed, then the chosen policy is $b_1 < b_1^*(\omega)$, which is worse for $c1$. If it is viewed as not self-serving, then the chosen policy is $b_2^*(\omega) = -\omega - 2x_c$, which is also lower than $-\omega$.

If $\omega > 1 - 2x_c$ but $c1$ deviates to a b_1 such that $b_1 > b_1^*(\omega)$, then the policy chosen is at least as high as $b_2^*(\omega)$; because $b_2^*(\omega)$ is already too high, this is not advantageous for $c1$. If $c1$ deviates to a b_1 such that $b_1 < b_1^*(\omega)$, then $c2$'s proposal is not viewed as self-serving, and $b_2^*(\omega) = -\omega + 2x_c$ is adopted. But $c1$ is exactly indifferent between this bill and the equilibrium bill $-\omega$, so such a deviation is not advantageous either.

Now suppose $c1$ follows the prescribed strategy but $c2$ deviates. The only circumstance in which this could be profitable—that is, when $u_{c2}(b_2, \omega) > u_{c2}(b_1^*(\omega), \omega)$—results in no change to the chosen policy. Thus, $c2$ has no profitable deviation.

Finally, it is unprofitable for $c1$ to deviate to a bill b_1 that is outside the range of equilibrium actions.   *Q.E.D.*

Proof of Proposition 2

Suppose the contrary, so that for every pair $(b^*(\omega), s^*(\omega))$, the legislature chooses the policy $b^*(\omega) = -\omega$.

Define $[\omega_0, \omega_1]$ to be the interval such that, for all $\omega \in [\omega_0, \omega_1]$, $-(x_c - (p_0 + \omega))^2 \geq -(x_c)^2$. It is routine to verify that $\omega_0 = -p_0$ and $\omega_1 = 2x_c - p_0$. Hence, in this interval, $c1$ prefers the status quo action to the equilibrium action. But for $\omega \in (\omega_0, \omega_1)$, if $c1$ chooses $b' = -\omega + \varepsilon$, where $\varepsilon > 0$ is small, then ℓ must choose either policy p_0 or b'. Because

both of these policies are preferred to the equilibrium action by $c1$, this is a profitable deviation. $\qquad$ *Q.E.D.*

Proof of Proposition 3

We show that no one can gain by deviating from the prescribed strategies.

Note that the legislature is always optimizing, given its beliefs, and the beliefs are consistent with Bayes's rule along the equilibrium path. We only need to consider deviations by $c1$ and $c2$.

A. $\omega \leq -2x_c - p_0$. If $c1$ deviates, this results in policy p_0, which is lower than $-\omega$ and hence worse from $c1$'s perspective.

If $c2$ deviates, the result is p_0. This is not profitable, since $x_c \leq -x_c - \omega - p_0$, and thus $-x_c^2 \geq -(-x_c - \omega - p_0)^2$.

B. $-2x_c - p_0 \leq \omega \leq -x_c - p_0$. If $c1$ deviates, this results in policy p_0, which is lower than $b^*(\omega)$ and worse from $c1$'s perspective.

If $c2$ deviates, this, by construction, leads to exactly the same utility for $c2$ as the equilibrium action. To see this, notice that $-2\omega + (-2x_c - p_0) - (-x_c - \omega) = (-x_c - \omega) - p_0$, and so $c2$'s utilities from the two outcomes are the same.

C. $-x_c - p_0 \leq \omega \leq x_c - p_0$. In this region a deviation by neither $c1$ nor $c2$ affects the policy chosen, p_0.

D. $x_c - p_0 \leq \omega \leq 2x_c - p_0$. If $c1$ deviates, then, by construction, this leads to exactly the same utility for $c1$ as the equilibrium action. To see this, notice that $(x_c - \omega) - (-2\omega + (2x_c - p_0)) = p_0 - (x_c - \omega)$, and so $c1$'s utilities from the two outcomes are the same.

If $c2$ deviates, this results in a policy p_0, which is higher than $b^*(\omega)$ and worse from $c2$'s perspective.

E. $\omega \geq 2x_c - p_0$. The proof is analogous to the case in which $\omega \leq -2x_c - p_0$.

The calculation of the expected utilities is routine. *Q.E.D.*

Proof of Proposition 4

The proof of proposition 4 is somewhat involved. Unlike the open or modified rules, the closed rule does not permit full informational efficiency (proposition 2), so a detailed examination of the set of equilibria is needed.

We begin by identifying a parametric class of closed rule equilibria that includes the equilibrium of proposition 3. This proves useful in determining the most informative equilibrium.

LEMMA 1. *For each* $\alpha \in [0, x_c]$, *the following constitutes a legislative equilibrium under the closed rule:*

$$b^\alpha(\omega) =$$

$$\begin{cases} -\omega & \text{if } \omega \leq -2x_c - p_0 \\ -2\omega + (-2x_c - p_0) & \text{if } -2x_c - p_0 \leq \omega \leq -x_c - p_0 - \alpha \\ p_0 + 2\alpha & \text{if } -x_c - p_0 - \alpha < \omega < x_c - p_0 - \alpha \\ p_0 & \text{if } x_c - p_0 - \alpha \leq \omega \leq x_c - p_0 \\ -2\omega + (2x_c - p_0) & \text{if } x_c - p_0 \leq \omega \leq 2x_c - p_0 \\ -\omega & \text{if } \omega \geq 2x_c - p_0 \end{cases};$$

$$s^\alpha(\omega) =$$

$$\begin{cases} \omega & \text{if } \omega \leq -x_c - p_0 - \alpha \\ \omega \in [-x_c - p_0 - \alpha, x_c - p_0 - \alpha] & \text{if } -x_c - p_0 - \alpha \leq \omega \leq x_c - p_0 - \alpha \\ -p_0 & \text{if } x_c - p_0 - \alpha < \omega < x_c - p_0 \\ \omega & \text{if } \omega \geq x_c - p_0 \end{cases}$$

If $c1$ *and* $c2$ *agree or* $b = p_0 + 2\alpha$, *then*

$$p^\alpha(b, s) = b.$$

If $c1$ *and* $c2$ *disagree and* $b \neq p_0 + 2\alpha$, *then*

$$p^\alpha(b, s) = p_0.$$

The beliefs of the legislature are

$$g^\alpha(b_1, b_2) =$$

$$\begin{cases} -p^\alpha(b, s) & \text{if } b \neq p_0 + 2\alpha \\ [-x_c - p_0 - \alpha, x_c - p_0 - \alpha] & \text{otherwise} \end{cases}$$

The expected utilities are

$$Eu_\ell = -\frac{4}{3}x_c^3 - 2\alpha^2,$$

$$Eu_{c1} = -\frac{4}{3}x_c^3 - x_c^2 + 4\alpha x_c(2x_c - \alpha),$$

$$Eu_{c2} = -\frac{4}{3}x_c^3 - x_c^2 - 4\alpha x_c(2x_c - \alpha).$$

See Figure 4 for an illustration of a semirevealing equilibrium of type α.

Proof of Lemma 1. The proof of lemma 1 is virtually identical to the proof of proposition 3, with two differences. First, it is now possible for $c1$ to deviate and induce the policy $p_0 + 2\alpha$. If $\omega \leq -x_c - p_0 - \alpha$, this deviation induces a lower policy than that called for in equilibrium; hence, the equilibrium policy is preferred by $c1$. For $\omega \geq x_c - p_0 - \alpha$, such a deviation induces a policy that is higher than p_0, and in this region the policy p_0 is preferred by $c1$ to all actions higher than p_0; hence, this is not a profitable deviation. Second, consider deviations in the region $[-x_c - p_0 - \alpha, x_c - p_0 - \alpha]$. No deviations are preferable in this region because, at the point $\omega = x_c - p_0 - \alpha$, committee member $c1$ is exactly indifferent between the policy $p_0 + 2\alpha$ and the status quo p_0. For states ω in this region satisfying $\omega < x_c - p_0 - \alpha$, $c1$ strictly prefers $p_0 + 2\alpha$ to p_0, and for states $\omega > x_c - p_0 - \alpha$, $c1$ strictly prefers p_0 to $p_0 + 2\alpha$. Thus, no deviations from the equilibrium strategies are profitable in this region also.

The calculation of the expected utilities is then routine. *Q.E.D.*

We will refer to the equilibria identified in lemma 1 as semirevealing equilibria of type α. Two observations are worth noting. First, a semirevealing equilibrium of type 0 is identical to the equilibrium in proposition 3 (Figure 4 reduces to Figure 2 when $\alpha = 0$). Second, the legislature's payoff is maximized when $\alpha = 0$, that is, the equilibrium in proposition 3 is at least as good for the legislature as any semirevealing equilibrium of type α.

We now show that, in fact, the equilibrium in proposition 3 is the best for the legislature among all equilibria. We do this by showing that the legislature views any other equilibrium as dominated by some semirevealing equilibrium of type α. As noted above, this in turn is dominated by the equilibrium in proposition 3. In order to establish this formally, some definitions are useful.

Suppose $(\bar{b}, \bar{s}, \bar{p})$ is some arbitrary legislative equilibrium under the closed rule. Let $\bar{P}$ denote the resulting *equilibrium policy function*; that is, for all ω, $\bar{P}(\omega) = \bar{p}(\bar{b}(\omega), \bar{s}(\omega))$.

It is also useful to define $q_1(\omega)$ to be the policy, such that in state ω, $c1$ is exactly indifferent between $q_1(\omega)$ and p_0. In other words, $q_1(\omega)$ satisfies $-(x_c - q_1(\omega) - \omega)^2 = -(x_c - p_0 - \omega)^2$, and from this it follows that $q_1(\omega) = -2\omega + 2x_c - p_0$. In any state ω, the set of policies that $c1$ considers to be at least as good as p_0 are those lying between $\min \{p_0, q_1(\omega)\}$ and $\max \{p_0, q_1(\omega)\}$.

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FIGURE 4. A Semirevealing Equilibrium of Type α under the Closed Rule

Note: The horizontal axis measures the state of nature (ω)—known to committee members but not to the legislature. The vertical axis simultaneously measures majority bills (b), policies adopted (p), and outcomes (x). Notice that under the closed rule, only the majority can propose bills. The status quo policy (p_0) is also depicted on the vertical axis and is set equal to negative one-half. The legislature's ideal outcome is zero. The line segments in the figure denoted with the symbol α are the bills, policies, and outcomes occurring in equilibrium, where α is a parameter. For each α between 0 and x_c, there is an equilibrium of "type α." The discontinuous line with two steps, labeled b^α, depicts the majority's equilibrium proposals, which are always adopted. In extreme states (close to zero or close to one), the majority proposes the legislature's ideal. In states close to one-half, a single policy that is higher than the status quo is proposed. This policy, $p_0 + 2\alpha$, depicted on the vertical axis, first gets adopted in state $\omega = -x_c - p_0 - \alpha$, depicted on the horizontal axis. In intermediate states, the majority proposes a "compromise" policy midway between the legislature's ideal and the status quo. The discontinuity in the bills proposed by the majority, b^α, occurs in state $\omega = x_c - p_0 - \alpha$, depicted on the horizontal axis, where the majority is indifferent between the policy, $p = p_0 + 2\alpha$, and the status quo policy, p_0, both depicted on the vertical axis. The dark line, labeled x^α, depicts the resulting outcomes. Notice that the equilibrium in Figure 2 is of this type when α is equal to zero, so there is no discontinuity.

LEMMA 2. *Suppose $\bar{P}$ is an equilibrium policy function under the closed rule.*

Then, for almost all ω,

$$\min \{p_0, q_1(\omega)\} \leq \bar{P}(\omega) \leq \max \{p_0, q_1(\omega)\}.$$

Proof. If there exists an open interval of states in which neither of the above conditions are satisfied, then $c1$ can profitably deviate by proposing $b = p_0$ in these states, which guarantees that p_0 will be adopted.   *Q.E.D.*

Lemma 2 sets some limits on equilibrium policy function $\bar{P}$ because $c1$'s incentive constraints must be respected. Similarly, $c2$'s incentive constraints must be respected, but $c2$ is in a much weaker position than $c1$ because he can only make

speeches and not offer proposals. Whereas proposals constrain the legislature's choices under the closed rule, speeches are only cheap talk and can be ignored by the legislature.

We will say that $c2$'s speech is relevant, given proposal b, if there exists an s' such that $p(b, s') = p_0$ and an s'' such that $p(b, s'') = b$. In other words, given $c1$'s proposal b, $c2$'s speech can affect the policy adopted. If $c2$ cannot affect the policy, that is, if $p(b, s)$ is a constant no matter what speech s $c2$ makes, we will say that $c2$'s speech is irrelevant.

Analogous to the definition above, define $q_2(\omega)$ to be the policy such that, in state ω, $c2$ is exactly indifferent between $q_2(\omega)$ and p_0. In other words, $q_2(\omega)$ satisfies $-(-x_c - q_1(\omega) - \omega)^2 = -(-x_c - p_0 - \omega)^2$, and from this it follows

that $q_2(\omega) = -2\omega - 2x_c - p_0$. In any state ω, the set of policies that $c2$ considers to be at least as good as p_0 are those lying between min $\{p_0, q_2(\omega)\}$ and max $\{p_0, q_2(\omega)\}$.

LEMMA 3. *Suppose $\bar{P}$ is an equilibrium policy function under the closed rule. Then, for almost all ω, if c2's speech is relevant, given proposal $\bar{b}(\omega)$,*

$$\min \{p_0, q_2(\omega)\} \le \bar{P}(\omega) \le \max \{p_0, q_2(\omega)\}.$$

Proof. If there exists an open interval O of states in which $c2$'s speech is relevant, given proposal $\bar{b}(\omega)$, then for any $\omega \in O$, there exists a speech $s'(\omega)$ such that $p(b(\omega), s'(\omega)) = p_0$. If p_0 is preferred to $\bar{P}(\omega)$ by $c2$, she can profitably deviate by making the speech $s'(\omega)$. *Q.E.D.*

LEMMA 4. *Suppose $\bar{P}$ is an equilibrium policy function under the closed rule. If there is an open interval of states O such that $\bar{P}(\cdot)$ is continuous and strictly decreasing (or strictly increasing) over O, then either*

1. c2's speech is relevant, given proposal $b(\omega)$, or
2. $\bar{P}(\omega) = -\omega + x_c$.

Proof. Suppose that the contrary is true. Then there exists some open interval O in which the policy function is strictly decreasing, say, speeches are irrelevant, and $\bar{P}(\omega) \neq -\omega + x_c$. In this case, in any state $\omega \in O$, $c1$ prefers either policy $\bar{b}(\omega - \varepsilon)$ or $\bar{b}(\omega + \varepsilon)$ for small enough ε to the equilibrium policy $\bar{b}(\omega)$, since $\bar{P}(\omega) \neq -\omega + x_c$. Moreover, because speeches are irrelevant, $c1$ can unilaterally induce either of these policies in state ω, and this is a profitable deviation. *Q.E.D.*

Suppose that neither of the conditions given in lemma 4 holds over some open interval of states $\omega \in O$. Then it must be the case that $\bar{P}(\omega)$ is almost everywhere a constant function. Moreover, at any point ω' where $\lim_{\omega \to \omega'}^{-} P(\omega) = p^- \neq p^+ = \lim_{\omega \to \omega'}^{+} P(\omega)$, then $c1$ must be indifferent between p^+ and p^- in state ω'. We shall refer to an equilibrium policy function with these properties as a step function over the interval O.

LEMMA 5. *Given any equilibrium under the closed rule, there exists a semirevealing equilibrium of type α that is no worse for the legislature.*

Proof. Suppose $\bar{P}$ is an equilibrium policy function under the closed rule. Let $(b^\alpha, s^\alpha, p^\alpha)$ be a semirevealing equilibrium of type α, and let P^α denote the resulting policy function; that is, for all ω, $P^\alpha(\omega) = p^\alpha(b^\alpha(\omega), s^\alpha(\omega))$.

We will argue that there exists an $\alpha \in [0, x_c]$ such that the legislature's payoff from P^α is at least as large as its payoff from $\bar{P}$. Again, it is useful to divide the state space into different regions.

A. $\omega \le -2x_c - p_0$, or $\omega \ge 2x_c - p_0$.

If $\omega \le -2x_c - p_0$, then $P^\alpha(\omega) = -\omega$, and this is the best possible policy for the legislature. Similarly, if $\omega \ge 2x_c - p_0$, then $P^\alpha(\omega) = -\omega$, which is the optimal policy for the legislature.

B. $x_c - p_0 \le \omega \le 2x_c - p_0$.

Now suppose $x_c - p_0 \le \omega \le 2x_c - p_0$. In this region, $P^\alpha(\omega) = q_1(\omega) = -2\omega + 2x_c - p_0 \ge -\omega$ and thus has the property that all policies the legislature prefers to $P^\alpha(\omega)$ are lower than $P^\alpha(\omega)$. Also, $q_1(\omega) \ge p_0$ and, by lemma 2, $\bar{P}(\omega) \ge P^\alpha(\omega)$. Hence, $P^\alpha(\omega)$ is no worse for the legislature than $\bar{P}(\omega)$.

C. $-x_c - p_0 \le \omega \le x_c - p_0$.

Lemma 2 implies that $\bar{P}(\omega) \ge p_0$ in this region. Combining this with lemma 3 implies that, in any legislative equilibrium, speeches are irrelevant in this region. By lemma 4 it then follows that any equilibrium $\bar{P}$ must either be a step function

or $\bar{P}(\omega) = -\omega + x_c$. Yet, $\bar{P}(\omega) = -\omega + x_c$ is not possible, because the legislature prefers p_0 to $-\omega + x_c$ for every ω in this region and can guarantee p_0. Thus, $\bar{P}$ is a step function in this region.

Suppose that the last discontinuity in the region $[-x_c - p_0, x_c - p_0]$ occurs at the point ω', and let $\lim_{\varepsilon > 0} \bar{P}(\omega' - \varepsilon) = p'$. Notice that at ω', $c1$ must be indifferent between p' and p_0 (because speeches by $c2$ must be irrelevant in this region). This is the same as saying that $p' = q_1(\omega) = -2\omega' + 2x_c - p_0$.

Next, we claim that if the region in which p' is the equilibrium action under $\bar{P}$ is $[\sigma', \omega']$, then it must be the case that $P^*(\sigma') = p'$. A necessary condition for this to be an equilibrium is that the legislature knows only that $\omega \in [\sigma', \omega']$ and weakly prefers to choose p' rather than p_0. Given its information, the legislature's optimal policy is $-\frac{1}{2}(\sigma' + \omega')$, and it chooses p' as long as

$$p' + \frac{1}{2}(\sigma' + \omega') \le -\frac{1}{2}(\sigma' + \omega') - p_0,$$

which is equivalent to

$$\sigma' \le \omega' - 2x_c.$$

This in turn is equivalent to saying that $\sigma' \le P^{*-1}(p')$.

Now notice that because $\omega' \in [-x_c - p_0, x_c - p_0]$ there exists an $\alpha \in [0, x_c]$ such that $\omega' = x_c - p_0 - \alpha$, and hence $\sigma' \le -x_c - p_0 - \alpha$. Finally, since $c1$ is indifferent between p' and p_0 at the point $\omega' = x_c - p_0 - \alpha$, it must be that $p' = p_0 + 2\alpha$, but this is identical to a P^α equilibrium in this region.

D. $-2x_c - p_0 \le \omega \le -x_c - p_0$.

In any subinterval of this region, if $\bar{P}(\omega) > P^*(\omega)$, then by lemmas 3 and 4, $\bar{P}$ either must consist of a step function or follow $c1$'s bliss line. From the argument given in region **C**, we know that for all $\omega \ge -x_c - p_0 - \alpha$ where $\alpha \in [0, x_c]$, $\bar{P}(\omega) = p_0 + 2\alpha$.

Next note that any equilibrium with the property that for all $\omega \ge -x_c - p_0 - \alpha$, $\bar{P}(\omega) \ge p_0 + 2\alpha$ or $\bar{P}(\omega) < -2\omega + (-2x_c - p_0)$ is worse than any P^α equilibrium from the perspective of the legislature. It is routine to verify that any equilibrium $\bar{P}$ consisting of either a step function or $\bar{P}(\omega) = -\omega + x_c$ or both has the property that $\bar{P}(\omega) \ge p_0 + 2\alpha$ or $\bar{P}(\omega) < -2\omega + -2x_c - p_0$. Hence, P^α is no worse than $\bar{P}$ in this region.

It is now easy to verify that $\bar{P}$ is no better for the legislature than some P^α. *Q.E.D.*

Combining lemmas 1 and 5 with the observation that from the perspective of the legislature the best semirevealing equilibrium of type α is one in which $\alpha = 0$, we complete the proof of proposition 4. *Q.E.D.*

Proof of Proposition 8

Observe that the beliefs are consistent with Bayes's rule wherever possible. We show that, given beliefs, the legislature cannot profitably deviate.

1. If $4x_c + p_0 < b \le x_c$, then by selecting policy b, the legislature earns $-x_c^2$. By deviating to p_0, the legislature earns $(b - p_0)(p_0 + 2x_c - b) - x_c^2$, and because $4x_c + p_0 < b$, this expression is less than $-x_c^2$.
2. If $b = 4x_c + p_0$, then the expected utility of the legislature from choosing b is $-\frac{7}{3}x_c^3$. By deviating to p_0, the legislature earns $-\frac{19}{3}x_c^3$, so deviating is clearly not profitable.
3. If $b = 2x_c + p_0$, then the legislature earns $-\frac{8}{3}x_c^3$ by

choosing b. By deviating to p_0, the legislature gains identical payoffs, so this is not a profitable deviation.

It is routine to verify that deviation is unprofitable to the legislature for the remaining cases.

We now turn to the incentives of the committee to deviate.

1. If $0 \leq \omega \leq -3x_c - p_0$, then the committee is obtaining its best possible outcome, so deviation is not profitable.
2. Suppose $-3x_c - p_0 \leq \omega \leq -2x_c - p_0$. The committee is just indifferent between the policy $4x_c + p_0$ and $2x_c + p_0$ if and only if $\omega = -2x_c - p_0$. Hence, for all $\omega \geq -2x_c - p_0$, the committee prefers the policy $4x_c + p_0$ to any policy $p \leq 2x_c + p_0$.

The remaining cases are analogous to one of those above.

$Q.E.D.$

Proof of Proposition 9

It is sufficient to show that the legislature's expected utility in the equilibrium under the closed rule constructed in proposition 8 is higher than the legislature's expected utility in the best equilibrium under the open rule, that is, in the equilibrium described in proposition 6.

Under the open rule, the payoff to the legislature in the best legislative equilibrium is (proposition 6):

$$-\frac{1}{12N(x_c)^2} - \frac{x_c^2(N(x_c)^2 - 1)}{3}, \qquad (3)$$

where $N(x_c)$ is the smallest positive integer greater than or equal to $(-\frac{1}{2} + \frac{1}{2}\sqrt{1 + 2/x_c})$ and, in fact, is the number of distinct policies chosen in equilibrium.

This can be equivalently stated as follows. For any positive integer N, define $x(N) = 1/(2N(N + 1))$. If $x(N) \leq x_c < x(N - 1)$, then $N(x_c) = N$.

Under the closed rule, the payoff to the legislature in the equilibrium constructed in proposition 8 is

$$-\frac{4}{3}x_c^3 - x_c^2. \qquad (4)$$

Because $x_c < \frac{1}{4}$, $N(x_c) \geq 2$, so

$$-x_c^2 \geq -\frac{x_c^2(N(x_c)^2 - 1)}{3}.$$

Next note that for any $N \geq 2$ and $x(N) \leq x_c < x(N - 1)$,

$$-\frac{4}{3}x_c^3 > -\frac{4}{3}x(N - 1)^3$$

$$= -\frac{4}{3}\left(\frac{1}{2N(N - 1)}\right)^3$$

$$= -\frac{1}{6N^3(N - 1)^3}$$

$$\geq -\frac{1}{12N^2}.$$

Thus, the second term in equation 4 is no less than the corresponding term in equation 3, and the first term is actually greater. $Q.E.D.$

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Plausibility of Signals by a Heterogeneous Committee

KEITH KREHBIEL *Stanford University*

*K*rishna and Morgan propose "amendments" to two of Gilligan and Krehbiel's theoretical studies of legislative signaling. The new results for homogeneous committees do not significantly change the empirical expectations of prior works, but the results for heterogeneous committees contradict earlier claims. This note gives primary attention to heterogeneous committees and compares and contrasts the new and old equilibria and their empirical implications. The notion of signaling is somewhat nebulous in all such games but seems distinctly less plausible in the key Krishna-Morgan proposition than in previous legislative signaling games. Furthermore, the empirical literature on choice of rules—specifically, the finding of a positive relationship between committee heterogeneity and restrictive rules—is inconsistent with the Krishna-Morgan analysis but consistent with Gilligan-Krehbiel analyses, even though the former is informationally efficient and the latter are not.

In "Asymmetric Information and Legislative Rules: Some Amendments," Vijay Krishna and John Morgan present nine propositions and two corollaries that extend the open, modified, and closed rule versions of the homogeneous and heterogeneous committees models introduced by Gilligan and Krehbiel (1987, 1988). The authors state: "Our results differ significantly from those of Gilligan and Krehbiel. Since our model and methods are identical, it is worth exploring the reasons for this discrepancy" (Krishna and Morgan 2001b, 436).

This comment is an attempt to assesses the substantive significance of the new findings. The issues that arise are due to multiple equilibria within this class of games. Short of applying explicit equilibrium refinements for signaling games, two factors can be considered when evaluating different sets of results: (1) the plausibility of equilibrium behavior in light of our knowledge of legislative politics, and (2) the empirical support for testable implications of the equilibria based on systematic data analysis. After briefly summarizing the new results, I will offer some remarks on plausibility and on empirical findings.

NEW RESULTS

Table 1 summarizes the results of Krishna and Morgan. Three observations are noteworthy. First, none of the findings allege that prior results are incorrect. Rather, the significance of the article rests on its identification of new equilibria. The key results are propositions 1, 3, and 8, which characterize equilibria that involve more information transmission (i.e., less residual uncertainty) than their respective structurally identical games in Gilligan and Krehbiel (1987, 1988).

Second, approximately half the new results pertain to homogeneous committees. For these committees, the implications for the choice of rules are not quali-

tatively different from those in previous studies.[1] In fact, the Krishna and Morgan closed rule equilibrium partitions the random variable space more finely than does that of Gilligan and Krehbiel, and the open rule equilibrium and expected utilities are unchanged. These two facts actually strengthen the original informational rationale for restrictive rules.

Third, the new results for heterogeneous committees have empirical implications that contradict those of earlier works. According to the Krishna-Morgan analysis, if committees are heterogeneous, the legislature will always choose to employ open or modified rules rather than closed rules. The reason is simply that, in the more recently identified equilibria, open or modified rules provide a more hospitable setting for credible information transmission than do closed rules. The remainder of this note focuses on this implication, which, as can easily be seen in Table 1, rests fundamentally on proposition 1.

PLAUSIBILITY OF SIGNALS

Krishna and Morgan's proposition 1 identifies an equilibrium in which the floor median voter can infer perfectly the committee members' private information. In the open rule case, the median voter is free to modify the bill so that the realized outcome equals her ideal point. Similarly, in the modified rule case, the median voter chooses the bill offered by the first committee member, and this bill results in an outcome identical to that in the open rule case. As such, the distributional and informational properties could not be better from the perspective of the legislature's median voter.[2]

The proposition 1 equilibrium is technically correct,

Keith Krehbiel is Edward B. Rust Professor of Political Science, Graduate School of Business, Stanford University, Stanford, CA 94305-5015 (krehbiel@Stanford.edu).

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[1] See Gilligan and Krehbiel 1987 for propositions and proofs and Krehbiel 1991, chapter 3, for a more general, nontechnical summary.

[2] An important caveat is that specialization is not endogenous in the Krishna and Morgan analysis, as it was, for instance, in Gilligan and Krehbiel (1987, 1990, 1997). A largely unexplored area of inquiry, therefore, is whether—or the conditions under which—members of a heterogeneous committee would specialize if there were no distributive benefit for doing so. Clearly, the cost of specialization would have to be exceeded by the benefits to the committee of uncertainty reduction. Krishna and Morgan (2001b) take some modest steps in assessing the incentives for heterogeneous commit-

TABLE 1.   Summary of the Krishna and Morgan Results

Number	Content	Rule, Committee	Signaling	Summary, Comments
P1	Characterization of equilibrium	Open, Heterogeneous	Separating	This equilibrium is maximally informationally efficient and, thus, yields greater utility than the corresponding Gilligan and Krehbiel equilibrium (1988, prop. 1).
C1	Preferences over equilibria	Open, Heterogeneous		The equilibrium in proposition 1 is unanimously preferred to all other open rule equilibria.
P2	Inescapable inefficiency	Closed, Heterogeneous	Pooling	All closed rule equilibria involve some pooling and, thus, involve some informational inefficiency.
P3	Characterization of equilibrium	Closed, Heterogeneous	Pooling	The equilibrium in proposition 3 is more efficient than the corresponding Gilligan and Krehbiel equilibrium (1988, prop. 3).
P4	Comparative informational efficiency	Closed, Heterogeneous		Krishna and Morgan's proposition 4 is the most efficient equilibrium of its type.
P5	Identical equilibria	Open and Modified, Heterogeneous	Separating	The proposition 1 equilibrium for the open rule is always an equilibrium for the corresponding modified rule game, too.
C2	Inescapable inefficiency	Open, Homogeneous	Pooling	All closed rule equilibria involve some pooling and, thus, are inefficient.
P6	Expected utilities in equilibrium	Open, Homogeneous	Pooling	This was also derived and noted in Crawford and Sobel (1982) and Gilligan and Krehbiel (1987).
P7	Inescapable inefficiency	Closed, Homogeneous	Pooling	All equilibria with homogeneous committees and a closed rule involve some pooling and are thus inefficient.
P8	Characterization of equilibrium	Closed, Homogeneous	Pooling	This equilibrium is more efficient than the corresponding Gilligan and Krehbiel equilibrium (1987, prop. 5).
P9	Comparison across rules	Open and Closed, Homogeneous	Pooling	Conditions are identified under which the closed rule is more efficient than the open rule.

but how well does its comport with actual legislative behavior? Some simple but substantive examples indicate that the combination of beliefs and behavior in the equilibrium are somewhat implausible.[3] Specifically, suppose a heterogeneous committee's jurisdiction includes authorizations for spending on military armaments and personnel. At the start of a budget cycle (or, analogously, at the beginning of the game), members of Congress are uncertain about the extent to which various areas in the world constitute threats to U.S. national security. Committee members and their staffs, therefore, engage in information gathering. For example, they hold hearings, attend secret briefings, and generally work to estimate the magnitude of the threat to national security in various hot spots throughout the world. The tangible product of these specialization efforts is a policy recommendation to the legislature as a whole. In this context, the random variable, ω, can be

interpreted an indicator of national security. The lower is ω, the greater is the need for defense spending (relative to other possibilities), and vice versa. Within this framework, and depending on the value of ω, two and only two cases emerge.

Case 1

Consider any play of the game in which the committee members learn that the sum total of international threats constitutes a security level such that ω lies on or between 0 and $1 - 2x_c$.[4] For any such state of the world, the equilibrium stipulates that the first committee member, a relative hawk,[5] proposes $b_1 = -\omega$. As noted

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tees to obtain information near the end of their article (see also Gilligan and Krehbiel 1997).

[3] Similar objections can be raised for nearly any signaling model, including those of Gilligan and Krehbiel, so "plausibility" is necessarily subjective and relative. Comparisons are introduced as the discussion proceeds.

[4] See Figure 1 in Krishna and Morgan for a graphic depiction of the equilibrium on which the following discussion is based.

[5] Following Epstein (1998), Krishna and Morgan refer to the first committee member as the "majority" and the second committee member as the "minority." This interpretation, which was not the intention of the original model, suggests that one committee faction is larger than (and therefore can outvote and be more influential than) the other. By extension, the partisan interpretation suggests that the legislature's median voter is a member of the majority party and that, given these partisan references, behavior itself may be

above, this proposal accommodates perfectly the median voter in the legislature because, when she accepts it, as she does in equilibrium, the resulting outcome is her ideal point.

Meanwhile, the second committee member, c_2, a relative dove, offers the proposal $b_2 = -2x_c - \omega$, which is exactly $2x_c$ lower than b_1. The magnitude of this difference in proposals is the same as in the Gilligan-Krehbiel "confirmatory signaling" equilibrium. To make this interval confirmatory, however, Krishna and Morgan's dove must make a proposal that, if it were to be adopted, would provide for considerably less spending than even the dove regards as ideal. So, although the committees are analytically symmetric in all exogenous respects, their derived behavior is not symmetric in the Krishna-Morgan equilibrium. Committee member 1 always caters to the chamber median (here and in case 2 below), whereas committee member 2 (in this case but not the next) oddly confirms the signal by, in effect, understating his preferences by a factor of 2.

Extension of the exercise to the broader political arena is also instructive. Obviously, the dove's proposal, if it were put to a vote on the floor, would be resoundingly defeated. The dove knows this, of course, but might rationalize it within a larger game of politics with reference to the value of position taking (Mayhew 1974).[6] Unfortunately, the requisite rationalization is flimsy because, regardless of whether the proposal is subjected to a knowingly futile vote, the dove's constituents will be unhappy on position-taking grounds. Upon learning the value of ω—as they will if, indeed, the Krishna-Morgan equilibrium is separating—constituents will view their elected representative as having squandered an opportunity to offer a proposal that may have been taken seriously. Instead, the dove crafted a futile proposal that even the somewhat outlying constituents regard as overly extreme on their own side of the spectrum. Accordingly, the dove will find it difficult to explain to constituents the logic of this distinctively asymmetric form of confirmatory signaling.

Confirmatory signaling in the Gilligan-Krehbiel open and modified rule equilibria is different. First, committee behavior is symmetric. Second, the signaling that occurs in equilibrium seems compatible with electoral realities. Specifically, under the modified rule,

"partisan," in the sense of cohesion independent or in spite of preferences. In other words, these partisan interpretations are slippery slopes to an interpretation of the results as involving cooperative behavior. Because these are unquestionably noncooperative theories, such interpretations should be resisted. I therefore refer to "committee members 1 and 2," who are identical in terms of moves within the game form, with the exception of the closed rule case, in which the first member proposes, and the second member only makes a speech.

[6] Technically, Mayhew-like position taking is inconsistent with the cheap talk assumption of the open rule model. If a player cares about positions apart from consequences, then signals are not costless or, more specifically, payoffs are not independent of signals. To reject a theory on the basis of the possible falsity of one of its many assumptions is harsh, however. All theories simplify reality at the level of assumptions, but many predict well in spite of assumptions that deviate from reality. This discussion should be viewed as subjective but suggestive about whether the Krishna-Morgan or Gilligan-Krehbiel equilibria are likely to predict well.

when the realization of ω is extreme relative to the committee's preference extremity, each committee member proposes the bill that results in the floor median voter's ideal point. This form of confirmatory compromise is common in politics and seems relatively easy to explain to constituents; for example: "We needed a compromise to pass a bill, so we met the opposition half way." Similarly, under the open rule, each committee member proposes the bill that, if passed, results in his or her respective ideal point. This form of confirmatory position taking also seems easy to explain to constituents: "We gave it our best shot, but the votes just weren't there for the proposal we liked best. Eventually, the House opted for a compromise amendment."[7] In total, these factors seem to confer a comparative advantage to the Gilligan-Krehbiel equilibrium over the Krishna-Morgan equilibrium in terms of plausibility.

Case 2

If the state of the world is above the threshold ($\omega > 1 - 2x_c$ and ≤ 1), the prescribed behavior in the Krishna-Morgan equilibrium is, if anything, more peculiar than in case 1. For substantive motivation, suppose that during secret briefings, the CIA reports an unexpectedly large reduction in anti-U.S. terrorist threats and activity throughout various hot spots, which indicates that the U.S. national security level is somewhat greater than all legislators' prior estimates.

The behavior of the first committee member is identical to that in case 1. He again proposes the bill whose realized outcome equals the floor median voter's ideal point, namely, $b_1 = -\omega$.

In contrast, the behavior of the second committee member is unusual, not only relative to that in case 1 but also and especially relative to the first committee member's behavior. In substantive terms, the equilibrium stipulates that the dove must now behave as if he were a superhawk: $b_2 = 2x_c - \omega$. Like the proposal strategies in case 1, this strategy and that of the other committee member are not symmetric, and, again, the latter is difficult to explain. The substance of the dove's proposal is best revealed with the hawk's proposal as a reference point. Although the hawk's bill results in the median voter's ideal point, which is x_c less than the hawk wants, the dove's proposal adds $2x_c$ to the hawk's proposal. In other words, the dove's proposal, if adopted, would result in a policy that is twice as great as the hawk's ideal point.

To suggest that this action puts the dove in an electorally awkward position is to understate the obvious. Yet, such behavior is technically rational because, in effect, the analyst is free to impose beliefs on the floor median voter, subject to Nash behavior in other

[7] For moderate values of ω, both committee members send noisy signals. See Gilligan and Krehbiel (1988, 472–3, n. 12) for several possible interpretations of "pooling." There is no firm consensus on this issue, but for present purposes it suffices to think of pooling as a statement such as: "We disagree with one another on what should be done." The median voter's reaction to noisy signals is discussed below.

respects. Given this freedom, a plausibility check on the inferential processes that might culminate in such beliefs seems in order. This, too, is subjective but nevertheless suggestive. The issue is: What would a reasonable floor median voter infer upon seeing various pairs of committee bills? (To address it, I take as given that the first committee member proposes $b_1 = -\omega$, as discussed above.)

If the median voter observes a dove's proposal that provides for much more defense spending than the hawk's proposal, common sense suggests that she might think: "The dove must have learned about a dreadful threat to our national security. He would never advocate such high spending otherwise, so I should pass it."[8] Such reasoning is inconsistent with the beliefs Krishna and Morgan incorporate, however. In their proposition, the floor median voter's thinking has to be something like this: "Proposing an outlandishly large amount of expenditures is just the dove's eccentric way of confirming that we have a high level of national security, so I'll go with the hawk's more dovish proposal." Clearly, the leverage of the Krishna-Morgan equilibrium comes from the stipulation that, however odd it may seem, the floor median voter, upon seeing two proposals in which the dove's is twice as hawkish as the hawk's ideal point,[9] will infer that the hawk's proposal is perfectly in the floor median voter's interest.[10]

EMPIRICAL FINDINGS

It is not inconceivable that legislators' actions and thoughts approximate those of the players in the Krishna and Morgan game, but evidence of this possibility is needed. The above illustrations sharpen the focus on three corresponding questions. Stated more generally than in the cases of defense policy, the questions are as follows.

- Case 1: How often do liberals make proposals much more liberal than even their liberal constituents want? Conversely, how often do conservatives make proposals much more conservative than even their conservative constituents want?
- Case 2: How often do leftist committee members generate proposals twice as rightist as those rightist members? Conversely, do rightists' proposals regularly outflank those of leftists?
- Both Cases: If and when the proposal behavior of high-specialization committees takes on the above properties, do uninformed moderate legislators know exactly how to make sense of committee members' ostensibly peculiar proposals?

If the empirically informed answers to these questions are approximately "often, often, and yes," then advocates of the Krishna-Morgan amendments to legislative signaling theories should be willing and able to come forth with corroborative empirical analysis. If the answers are approximately "rarely, rarely, and no," then, in spite of the superior informational properties of the Krishna and Morgan equilibrium, it seems unlikely that the amended results from the heterogeneous committees model will provide better predictions than the original set of models and results.

In either case, the questions underscore the fact that the suggestions about the comparative plausibility of behavior in Krishna-Morgan and Gilligan-Krehbiel equilibria are ultimately empirical issues. Fortunately, a comparative assessment can be found in studies that derive and test the comparative-statics implications of the equilibria. The key question is whether systematic data analysis reveals a positive or a negative relationship between the heterogeneity of committees and the likelihood their proposals will receive restrictive rules. Among previous studies, none consistently produces the negative relationship that the Krishna-Morgan analysis predicts.[11] In contrast, some studies consistently report empirical findings opposite those predicted by the Krishna-Morgan equilibria but consistent with the Gilligan-Krehbiel comparative equilibrium results (see Krehbiel 1991, 1997). An important qualification, however, is that the measures in these studies are weak. As such, the received wisdom about choice of rules is not as solidly grounded as one would like.

CONCLUSION

This brief comment cannot address all facets of the Krishna and Morgan extensions to informational theories of legislatures, but the focus on proposition 1 covers a large fraction of the authors' substantively significant results.[12] The main new result—corollary

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[8] Although this reasoning seems sensible in isolation, it begs the question of how the uninformed median voter can make sense of the hawk's seemingly soft proposal. In the Gilligan and Krehbiel equilibrium, when these kinds of puzzles arise for moderate ω, the answer is intuitive: She cannot make sense of the proposals, so she infers—with noise—that the national security threat is moderate, because, after all, hawks and doves are in disagreement. In the presence of the resulting uncertainty, the median voter simply and plausibly selects the policy that expectationally yields her ideal point. This is where and why the Gilligan-Krehbiel equilibrium entails informational inefficiency and the Krishna-Morgan equilibrium does not. In the former case, the median voter responds to noisy or implausible signals with a best guess; in the latter case, the median voter responds to peculiar pairs of proposals and knows exactly what they mean about the state of the world.

[9] "Hawkishness" here is implicitly parameterized as x_c, the degree to which the hawk's ideal point deviates from that of the floor median voter $x_f = 0$.

[10] In subsequent communications, Krishna and Morgan (2001a) amend their original amendment (proposition 1A), which under somewhat modified assumptions has the same outcome as proposition 1 for all states of the world but which has more plausible signals in terms of the above discussion. A detailed response is not possible under the circumstances, but it is easy to see that the floor median voter's belief structure and behavior off the equilibrium path are very complex. This comment confines attention to the published work (2001b), but I nevertheless note that Krishna and Morgan's position is that the plausibility objections raised here regarding signaling apply "with equal force" to both sets of models.

[11] In six equations estimated, Dion and Huber (1997) find a negative effect twice, but in both instances the coefficient is not statistically significant.

[12] For example, although I have not addressed the Krishna-Morgan statements about incentives, it suffices to note that such claims, like propositions 2–9 and corollaries 1 and 2, either do not deviate

2—follows directly from proposition 1, which suggests that distribution and efficiency are not in conflict after all. If this were true not only as a theoretical possibility but also as a practical matter, then many central problems of legislative organization—including committee composition, specialization, and choice of procedures—suddenly become trivial. Why, for instance, would we ever observe anything but heterogeneous committees and modified or open rules? In contrast, if the theoretically stipulated behavior in cases 1 and 2 above is not common in practice, then the existing view of legislative organization as one of managing the tension between distributive benefits and informational efficiency remains viable.

Aside from proposition 1 and the results that depend on it, the Krishna and Morgan results are aptly summarized as complementary extensions of the Gilligan-Krehbiel analysis that are based on more informative equilibria than previously identified. These theoretical contributions are interesting and useful, but they do not significantly change the qualitative predictions that were tested with some success in *Information and Legislative Organization* (Krehbiel 1991). Indeed, the Krishna-Morgan homogeneous committee equilibrium was corroborated before it was derived.

More generally, the amendments that Krishna and Morgan propose are of two distinct types: (1) complementary, reinforcing, and, thus, friendly, or (2) seemingly implausible and contrary to extant systematic data on legislative rules and heterogeneous committees. Although all the new results provide food for thought, the friendly amendments are distinctly more palatable than propositions with implausible signaling. I concede, however, that plausibility of signals is an eye-of-the-beholder standard and, therefore, reiterate a less objectionable claim: The new heterogeneous committee results do not account for congressional choice of rules as well as the old results, even though the new dominates the old in terms of informational efficiency.

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qualitatively from those of Gilligan and Krehbiel or do not survive without the implicit backing of proposition 1.

Additional food for thought follows. Given that multiple equilibria in signaling games are common, and assuming that the objective of theory is to predict and explain actual behavior, then perhaps plausibility of signals is a more promising concept on which to build equilibrium refinements than is informational efficiency. In any case, the surprising and thought-provoking results of Krishna and Morgan encourage us to continue to grapple with important issues in legislative organization, but to do so from empirical as well as theoretical perspectives.

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BOOK REVIEWS

Political Theory

Positive Political Theory I: Collective Preference. By David Austen-Smith and Jeffrey S. Banks. Ann Arbor: University of Michigan Press, 1999. 208p. $39.50.

Norman Schofield, *Washington University*

This study is intended as the first of a two-volume presentation of positive political theory. As the authors indicate in the preface, in this volume they develop what may be termed Arrovian direct preference aggregation, a theory that examines the properties of a direct aggregation rule, f, mapping from a domain of preference profiles on a set, X. For any profile, ρ, the image of f is a social preference relation denoted $f(\rho)$. The social relation may exhibit maximal or unbeaten outcomes, $mf(\rho)$, in X itself. Thus, the volume is concerned with the "classification" of all social rules that map from preference profiles into X itself.

An alternative way to study social preference is to incorporate strategic behavior by individuals. Such a theory constructs a behavioral map, b, say, from preference profiles to a strategy space, denoted S. In this game-theoretic context, the behavioral map will be induced by appropriate psychological theories of "rationality," by the nature of the strategy space S, and by the rules of the game, including f. Finally, the "outcome" function, g, aggregates strategies into outcomes, in X itself.

Typically, the behavioral map is assumed to select Nash equilibrium strategies $N(S,g,\rho)$ of the "game" (S,g,ρ) defined by preferences, strategies, and outcome functions. In actual political contexts the players will include not only an electorate but also more complex entities, such as parties, interest groups, and institutions. The "primitive" theory abstracts away from this complexity, however, by considering what may be called a simple committee. In such committees, complicated agenda rules, and so on, are ignored. In this context there may be an isomorphism between the direct aggregation theory and the indirect game-theoretic model, but only when a core exists. The core, $C_f(\rho)$, at the profile ρ and under the rule f is simply the set of unbeaten alternatives in X. When this set is nonempty, the Nash equilibrium of the induced game $(S,g\rho)$ should coincide with the core.

In essence, the present volume studies those conditions on f and ρ sufficient for core existence. Chapters 1 and 2 construct the fundamental building blocks of the primitive theory by carefully defining the properties of preference relations and by showing essentially that the social relation will itself be "rational" only when there are veto groups in the society. In particular, an elegant proof of Arrow's General Possibility Theorem is presented: Only if the social rule, f, is dictatorial can $f(\rho)$ be transitive for all transitive individual preferences.

Transitivity is an unnecessarily strong property for core existence, however. A weaker sufficient property is "acyclicity." As Kotaro Nakamura showed in 1979, if the set of alternatives X is finite, then f is "classified" by a number, denoted $s(f)$ and called the Nakamura number. If f is simple, in the sense that it is characterized by its winning coalitions, then $f(\rho)$ is "acyclic" for any ρ whenever the cardinality of X is bounded above by $s(f) - 1$. If the cardinality of X is at least $s(f)$, then a profile can be constructed such that $f(\rho)$ is both cyclic and has an empty core. The results of chapter 3 on restricting cardinality of X are then complemented by chapter 4, in which restrictions on profiles themselves are shown to be sufficient for existence of a core.

Whereas the first four chapters assume X is finite, chapters 5 and 6 develop the model when X is a subset of Euclidean space, $\mathfrak{R}^\kappa$. The main result of chapter 5 extends Nakamura's result by showing that the condition $\kappa \leq s(f) - 2$ is both necessary and sufficient for core existence (when individual preferences are assumed to be both "continuous" and "convex"). The final part of chapter 5 develops the theory when preferences are induced from smooth utilities. In 1967, Charles Plott (*The Probability of a Cyclical Majority*) essentially showed that a point was a core, under majority rule, if and only if the gradients of individual voters could be paired off. This result was extended by Richard McKelvey and Norman Schofield ("Generalized Symmetry Conditions at a Core Point," *Econometrica* 55 [July 1987]: 923–33) in 1987, who demonstrated that a particular gradient condition, for any voting rule, was both necessary and sufficient for core existence at a point. This technique leads to the main result, due to Jeffrey Banks and Don Saari.

The resulting "generic" classification or "chaos" theorem is based on Thom Transversality Theory. ("Generic" refers to an open-dense set in the topological space, U, of all smooth utility profiles. The topology involved is very fine, using information encoded in the gradients.) As an illustration, suppose f is induced from a simple rule, whereby any coalition of size q or above, out of n, is winning. Then f is generically classified by an integer $\sigma(q,n) \equiv 2q - n + \beta(q,n)$. Here $\beta(q,n)$ is a correction term that can be calculated explicitly. For majority rule $\beta = 0$, while $\beta(n - 1, n) = \frac{n-5}{2}$.

In particular, the core of f is generically (or almost always) empty whenever $\kappa > \sigma(q,n)$. For majority rule with n odd, this integer is 1; for n even it is 2. When the core is empty, voting cycles can "fill" the policy space X.

These results, first noted in a less general form about twenty-five years ago, led William Riker ("Implication from the Disequilibrium of Majority Rule for the Study of Institutions," *American Political Science Review* 74 [September 1980]: 432–46) to conclude that, "in the long run, nearly anything can happen in politics."

This core generic nonexistence result has led theorists in the last decade to focus on the indirect, game-theoretic model as the basis for positive political theory. As Austen-Smith and Banks observe, however, in the form of their last corollary, the theorem can be interpreted as follows. Consider a game form (S,g) for the social rule f on X, such that the Nash equilibrium $N(S,g,\rho)$ is always nonempty. If f has no vetoers, and if the dimension κ of X exceeds $2(n - 2)$, then for almost every profile, ρ, it is the case that, whenever $x \in N(S,g,\rho)$ there is a point $y \in X$ preferred to x by at least $(n - 1)$ individuals.

Although the game-theoretic models attempt to avoid the chaos theorem, it reappears as a generic nondemocratic result. Austen-Smith and Banks prefer to use the weak dimension restriction of $2(n - 2)$, valid for any f (without vetoers), but the result also could be stated for majority rule: If $\kappa \geq 3$, then for almost every profile ρ, if x is a Nash equilibrium of (S,g,ρ), a majority prefer some y to x.

Overall, the volume is an extremely lucid and self-contained presentation. It collects together every important result of the fifty years of research in this field, since Kenneth Arrow's book in 1951. The first four chapters use fairly well understood techniques and could readily be used in an introductory graduate class. Chapters 5 and 6 are increasingly more difficult but reward close study. In fact, readers will realize why Riker was moved to make his assertion about the fundamental feature of politics. Moreover, any enthusiasm

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for simple game-theoretic equilibrium models of political choice will necessarily be tempered by the realization, after reading this book, that such equilibria appear to float on a sea of chaos.

Austen-Smith and Banks, along with their colleagues John Duggan and Michel Le Breton, have made vigorous efforts in the last few years to understand the relationship between the contradictory results of social choice and game theory. The death of Banks just before Christmas 2000 saddened everyone who knew him. Without his deep intelligence and acute perception it will be that much more difficult to build on the work presented in this volume.

East Meets West: Human Rights and Democracy in East Asia. By Daniel A. Bell. Princeton, NJ: Princeton University Press, 2000. 396p. $65.00 cloth, $19.95 paper.

Fred Dallmayr, *University of Notre Dame*

"I continue to believe," states Brian Barry (*Justice as Impartiality*, 1995, p. 3), "in the possibility of putting forward a universally valid case in favor of liberal egalitarian principles." Barry's belief is not idiosyncratic but widely shared by Western intellectuals. In fact, a traditional trademark of Western philosophy has been to insist on the universal validity of its teachings, while relegating other cultures and ideas to a merely contingent or particularistic status. The belief tends to be shared by much of Western social science. Thus, political science as practiced in America is assumed to be a globally applicable discipline. As part of political science, comparative politics is said to offer a universally relevant scheme or grid, just as rational choice theory claims to provide a general explanation of social behavior irrespective of time and place. In this respect, recent philosophy and political theory show signs of a counterinsurgency aimed not at a simplistic relativism but at the cultivation of a stronger sense of the contestability of dominant frameworks and disciplinary paradigms. Contestation, of course, cannot be a one-way street but must allow for mutual questioning. *East Meets West* is an exemplar of this insurgency. Written in dialogue form, the book is liable to erode absolutist claims on all sides and thus to induce a greater readiness for mutual listening and learning.

Although more broadly relevant, *East Meets West* limits cross-cultural learning and contestation to the East Asian context, focusing specifically on debates around the notion of "Asian values." As Bell emphasizes, the point of his book is not simply to debunk Western liberal democracy and its tenet of universal rights but to make room and provide a hearing for East Asian challenges and arguments, particularly those that address "West-centric perspectives" (pp. 4–6). Attentiveness to such arguments, in his view, is required not only for theoretical reasons—their possibly innovative contributions—but also for practical political motives. If we fail to engage seriously with East Asian perspectives, we risk "widening misunderstanding and setting the stage for hostilities that could otherwise have been avoided" (p. 8). Bell's respect for cultural diversity does not prompt him to overvalue cultural traditions or to take "culture too seriously" (p. 9). After all, cultural traditions are always multiple, partially constructed, and part and parcel of evolving social-political and existential experiences. To avoid the temptation of mummifying traditions, Bell insists, it is important to distinguish "between traditional values that are still relevant today and others that have been relegated to the dustbin of history" (p. 10). To maintain contact with real-life issues, it is hence

desirable to limit one's focus to values "that continue to have widespread impact on people's political behavior in contemporary societies." (p. 10)

The dialogue of the book is divided into three parts and occurs in three different localities: Hong Kong, Singapore, and mainland China. It thus reflects a steadily deepening engagement with East Asian core beliefs. The lead character throughout is an individual named Sam Demo, a program officer for a fictitious American nongovernmental organization, National Endowment for Human Rights and Democracy (NEHRD). In Hong Kong, Demo engages in dialogue with a human rights activist and businessman; in Singapore, with a leading politician (Lee Kuan Yew); and in mainland China, with a political philosopher. Part 1, located almost accidentally in Hong Kong, offers general theoretical reflections on democracy and human rights, and these reverberate throughout the remainder of the book. In Bell's words (pp. 13–4), the discussion here seeks to throw into relief not the untenability of human rights but the importance of "substantial local knowledge" for the assessment of their range and limits; such knowledge may also reveal resources for buttressing human rights practices in indigenous ways while simultaneously leavening and complicating the agenda of Western rights activists. These points are fleshed out in greater detail in part 2, with reference to the specific political realities of Singapore. Countering the arguments of the elder statesman (Lee Kuan Yew) against democracy and human rights, Demo (or Bell) tries to show the compatibility of democracy with local "communitarian" values by emphasizing both the utility of the former for the latter and the usefulness of communitarianism for the pursuit of democratic goals. Adopting mainly a pragmatic or "consequentialist" line of reasoning, Bell notes that "strategic considerations of political relevance strongly speak in favor of communitarian justifications for democracy in Singapore, and perhaps in other East Asian societies as well" (pp. 16–7).

Part 3, devoted to mainland China, offers perhaps the most intriguing and also controversial reflections on the topic of the book. The dialogue is set in Beijing in June 2007, one day before a constitutional convention is to take place on political reform in China. Leaving behind merely pragmatic-consequentialist arguments, the discussion aims to construct a normative case for a distinctive Chinese approach to democracy, an approach that incorporates the traditional Confucian respect for rule by an intellectual elite. Tackling the question whether there are aspects of East Asian traditions that can provide a "moral foundation" for political practices and institutions different from Western-style liberal democracy, part 3 responds resolutely in the affirmative (pp. 18–9). At issue is a reform proposal, advanced by a professor at Beijing University, that would combine liberal democracy with elements of traditional Confuciansim, particularly the legacy of guidance by a group of competent and public-spirited intellectuals/scholars. More concretely, the proposal aims at the establishment of a bicameral legislature with a democratically elected lower house and an upper chamber composed of representatives selected on the basis of competitive examinations. In the course of the debate, Demo (or Bell) is "eventually persuaded by the proposal, though he presses the point that the 'House of Scholars' should be constitutionally subordinate to the democratically elected house."

For Western (especially American) readers, Bell's book provides ample food for thought as well as a lively reading experience. Given its aim of nurturing cross-cultural encoun-

ters, the dialogical structure of the book practices what it preaches, which vindicates the author's claim that "the medium is part of the message" (p. 11). Throughout its complex discussions, the book demonstrates Bell's subtle and dialogical bent of mind—that he is not merely a detached onlooker but a seriously engaged participant in contemporary East Asian debates. Given this bent, he would welcome (one surmises) some critical comments, especially when offered in the same dialogical spirit. One small qualm concerns the title of the book, which might have been reversed; over long stretches it is really a case of West meeting East. Still on the level of cross-cultural encounter one can question Bell's assertion that "the most widely publicized challenge to Western liberal democracy has emerged from the East-Asian region" (p. 7). Surely another and perhaps even more explosive challenge has been mounted by elements in Islamic civilization.

A more serious issue has to do with the mingling of pragmatic-consequentialist and normative-philosophical arguments. A book that invites Western readers to "meet the East" must take particular care to present arguments initially persuasive to such readers; appeals to locally pragmatic usefulness are insufficient in this context, as they prejudge questions of justification. As it happens, philosophical resources familiar to Western readers are not lacking and might have been marshaled. Thus, the emphasis on "substantial local knowledge" and the need to reconcile universalism and particularism could gain support from Aristotelian teachings regarding prudential judgment (as well as Hegel's notion of concrete universals).

The issue of normative justification is especially crucial in part 3, which deals with the possible emendation of Western liberal democracy by means of Chinese-Confucian legacies. For many Western liberals, the idea of such an emendation is liable to be anathema; however, outright rejection also means a refusal of reflective learning. Surely, contemporary liberal democracy is not without serious blemishes, ranging from rampant consumerism to voter apathy and public corruption; faced with these defects, friends of democracy should welcome, not simply dismiss, suggestions for improvement. After all, the proposal of the Beijing professor is not altogether alien to the history of Western thought. From Plato's philosopher-king and "nocturnal council," to Aristotle's distinction between corrupt and virtuous modes of popular rule, to Hegel's postulate of a *sittlich* class of civil servants, a long line of Western thinkers have sought to protect the common good against the ravages of factionalism and private self-indulgence.

The issue today is how such notions can be reconciled with the axiom of popular self-government. With specific reference to the "House of Scholars" one may ask: Who guards the guardians, or who examines the examiners? As it seems to me, in a democratic age the protection of the public good shifts from the state to society, which means that public virtue cannot be bestowed "top-down" from the state but must be nurtured among ordinary people (in their local settings). That is why in democracy civic education and voluntary civil associations are important, supplemented occasionally by inspiring *sittlich* leaders (such as Gandhi or Martin Luther King, Jr.). All democrats should worry about the peril of democracy sliding into demagoguery and self-indulgence, and they should vent this concern in public deliberations. Whether or not one agrees with the Beijing proposal, Bell is to be applauded for bringing it to the public forum and thus keeping the dialogue alive.

Oakeshott and His Contemporaries: Montaigne, St. Augustine, Hegel, et al. By Wendell John Coats, Jr. Selinsgrove, PA: Susquehanna University Press, and London: Associated University Presses, 2000. 138p. $31.50.
Sartre's Radicalism and Oakeshott's Conservatism: The Duplicity of Freedom. By Anthony Farr. London: Macmillan, and New York: St. Martin's, 1998. 266p. $65.00.
The Skeptic's Oakeshott. By Steven Anthony Gerencser. New York: St. Martin's, 2000. 214p. $49.95.

Timothy Fuller, *Colorado College*

Interest in Michael Oakeshott's political philosophy has been growing for a generation, since the publication of the original edition of his *Rationalism in Politics* (1962), a collection of essays dating from 1947 to 1960. Few then were aware of *Experience and Its Modes* (1933), a book that has since become the subject of much attention as the desire to understand Oakeshott's work as a whole has grown. With the publication of *On Human Conduct* (1975), Oakeshott gave us the work he hoped would confirm his place in the tradition of British political thought going back to Hobbes. Of course, his notable essays on Hobbes are now available in *Hobbes on Civil Association* (2000).

Oakeshott's reception in the United States was mixed. He was branded by liberal critics as a Burkean conservative, irrelevant to the American tradition. But American conservatives have often been cool to Oakeshott's skepticism and apparent lack of strong religious convictions (Irving Kristol and Russell Kirk, among others); Oakeshott is no "movement conservative." He has been called a man of the "right," an aesthete, an elitist, and yet also a postmodernist, a relativist, and an historicist. Yet, Oakeshott has been criticized for not taking politics (or economics) seriously, for seeing politics as a second-rate activity, a "necessary evil," and for insisting that the real strength of civilization lies in art, poetry, and philosophy, whose practitioners do best by remaining aloof from politics.

Oakeshott controversially held to the view that the practical life, although unavoidable and intrusive, is not the foundation of other activities, such as philosophic reflection, historical study, scientific research, or poetic expression. These are not, for him, peculiar ways to carry on the struggles of practical life by other means; rather, they are revelations of the multidimensional character of human experience in which play counters work, enjoyment ambition, conversation debate. Oakeshott thought of himself as a philosopher according to his view of philosophy. His interest in the policy debates that affect much philosophy in our time was modest, and he was as likely to criticize a conservative mixing politics and philosophy together as he was a liberal or a radical.

Philosophy, he thought, seeks to understand, not to prescribe, to reveal and describe the assumptions that people make in order to make sense of the world for themselves and to justify the conclusions they reach. Philosophy, for Oakeshott, is in the indicative mood; to philosophize is to disengage, not to intervene. To consider politics philosophically is, in Oakeshott's view, to describe the necessary character of political activity in its ceaseless efforts at preserving and changing; it is not to prescribe courses of action. In revealing the character of politics, philosophers have done what, as philosophers, they can do. They have no authority to direct politics derived from their philosophical understanding, because to act politically they must, like everyone else, accept uncritically some presuppositions for the purposes of action, thus leaving philosophy behind; they cannot unite theory and practice. Those who seek to do this may use philosophic ideas to lend support to their political dispositions, but they are

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using them in a nonphilosophic way. Oakeshott, in being consistent, does not say do not do this; rather, know what you are doing when you do it.

These three books offer well-informed, intelligent engagements with Oakeshott's thought, putting it into broad intellectual contexts and responding to Oakeshott on the issues aforementioned. Anyone interested in Oakeshott can learn much from these authors.

Anthony Farr seeks to clarify the question of political freedom by comparing Sartre and Oakeshott, whom he considers the most impressive twentieth-century interpreters of political freedom on the Left and Right, respectively, in a tradition going back to Rousseau, Kant, and Hegel. Sartre and Oakeshott share at least two assumptions. First, there is no "human nature"; second, there is no human teleology. But there are self-chosen projects, ideals, aspirations, goals. These are humanly made and manifest the self-understandings humans have as they move through history: Human beings are both the matter and the maker of themselves. Sartre insists on the human freedom to make and remake the world in continual liberation from what has already come to pass, in accord with self-chosen ideals and projects (the "Left view"). Oakeshott sees our embeddedness in a world of experience and habit that came before us, a world which does not determine what we do but which must constrain our choices in ways we cannot escape (the "Right view").

The Left accuses the Right of discouraging the effort to change the world. The Right accuses the Left of living in deluding abstraction from the "ultimate shapelessness of the world," with its "artificial and tenuous order" (p. 144) promoting optimism at the price of ultimate disillusionment. For Oakeshott, the practical life may seek complete satisfaction but will never attain it. This is the ordeal of freedom. And Oakeshott respects the freedom of the practical life in undermining the claims of the intellectuals to know how to shape and perfect it, especially in his critique of modern rationalism, of Sartrean emancipationism, and of the Heideggerian quest for "authenticity." For Oakeshott, freedom is at once more ordinary and yet more dignified. Freedom achieves a moral character in bringing into being civil association based on noninstrumental rules by means of which the "members are free because their performance is never actually directed; the state imposes nothing but conditional requirements," and they are as civil associates committed to "no substantial endeavour" (p. 194), a moral not a prudential relationship.

Farr admires these ideas and expounds them well, but he has reservations about their adequacy, for they ignore the view that "law is a vital component of who we are, a component of our psychology—the argument of the Sittlichkeit" (p. 206). Equally, he believes that, in On Human Conduct, Oakeshott permits an "atomic" and self-possessed "individual" to appear who is no longer the tenuous identity in the midst of a shifting experimental miscellany he finds in Rationalism in Politics. Oakeshott shifts the emphasis found in the earlier work, as Farr sees it, from our "having learned" to our "choosing" what we learn; selves, in the later Oakeshott, are independently deliberative. Thus Oakeshott moves in the reverse direction from Sartre, who started from such an individual and later contextualized him.

Farr sees Oakeshott as relinquishing "the courageous notion of the essays." "Each individual is made the fulcrum of determination as to what he learns and does not learn whereas before the emphasis was upon the resource that the community had to offer" (p. 222). This impregnable individual or agent Farr takes to be foreign to Oakeshott's original impulse because it implies something approaching a "human

nature." He prefers the earlier to this later Oakeshottian individual who, although still attuned to ordinary life (contra Heidegger) and willing to permit rather than to command (contra Sartre), defends an "audacious individual" with the potential to "lay waste the world" (p. 256).

Farr expounds his thesis powerfully. There is something to his view that the later Oakeshott hints of a "human nature" and an autonomous individual. This has to do with Oakeshott's defense of the reality of individuality against efforts to dissolve it, which is, I think, Oakeshott's corrective response to the interpretation of his earlier thought given by writers who, unlike Farr, disliked what Farr likes. But given Oakeshott's consistent rejection of rationalism and "theoreticians," his insistence on noninstrumentalism, his severity toward getting and spending, his quasi-Augustinian skepticism, it is more likely that he was adjusting indirectly to the point that earlier critics had made of his alleged irrationalism. It is doubtful that Oakeshott ever thought there was not a real individual.

Steven Gerencser approaches many of the same issues in a somewhat different way. Like Farr he gives an excellent account of the terrain of Oakeshott's thought and of some of the important intellectual background for his thought, especially regarding Hegelian and British idealism. In the end, he is concerned with the relationship of Oakeshott's thought to democracy, noting that his thought has attracted a number of both radical and democratic theorists. Gerencser explores the prospect of retaining the formality of civil association with a commitment to liberal democracy without Oakeshott's allegedly "conservative" civil association. Farr thinks that the rules of civil association require consensus on a common purpose in a manner not so far different from that required in an enterprise association, and he believes there will be significant conflict and antagonism over the rules of civil association that can neither be merely neutral nor purely noninstrumental.

According to Gerencser, "Oakeshott excludes from politics questions about the authority of the institutions and conditions of political association. Oakeshott's skepticism about the common good of community is arrested in the matter of authority" (p. 161). Gerencser must know that to say this is to vitiate the originality of Oakeshott's insight into the concept of authority. Rightly understood, Gerencser thinks, the import of Oakeshott's thought is to accept conflict over authority itself, in which case "the state will still exercise power with whatever authority it can cobble together, relying on the beliefs about authority that it can accommodate with some coherence" (p. 161).

To put it another way, Oakeshott may be seen as having described how modern European history is the story of movement from relations of command and obedience toward relations of authority and acknowledgment. But what is the character of acknowledgment of authority? There is no unanimity, and thus there are those who feel excluded and unrecognized even if there is a high degree of agreement. There must be a place for dissent if subscription to the civil association is to be based on real assent and not just on Hobbesian fear of the consequences of not submitting. The example of Martin Luther King shows that appeals can be made to the rules of association to establish the best understanding of what the rules are designed for. Skepticism about the current state of the rules is not merely negative but is an integral part of achieving greater coherence of civil self-understanding. According to Gerencser, for Oakeshott civil disobedience would be an "oxymoron" (p. 163) because he recognizes only subscription to authority or civil war. The intermediate condition of both subscribing to and yet criticizing the rules Oakeshott does not explore.

The point is that the movement from command and obedience to authority and (variable) acknowledgment is not complete and may be perpetually incomplete. The skeptic would preclude the prospect of a project to perfect legitimacy; such a project would rightly be exposed to all the arguments Oakeshott launches against rationalism, ideology, "the pursuit of perfection as the crow flies." But, if Gerencser is right, Oakeshottian skepticism should be launched against what he sees as Oakeshott's own rationalism in seeking to perfect the coherence of the idea of authority. Whereas Farr fears the deliberative and rationalist Oakeshottian individual who would transform the world, Gerencser fears the aloof rationalism of an Oakeshott who would find complaints about authority to be misunderstandings.

Possibly the distinction between civil and enterprise association should be all the more important if one were to follow Gerencser's view. Since many protests attack rules either because they fail to implement a collectivist dream or because they inhibit the imposition of commands the protesters desire, one would want to distinguish those from protests that address the specific issue of making civil association more true to itself. One fears, however, that this would also be too restrictive for those whose aspiration to turn politics to "right results" remains powerful even in what are largely civil associations.

Wendell John Coats presents us with a set of short essays on diverse intellectual relationships that illuminate Oakeshott's thought while trying to preserve him from a too systematic explanation. His first aim is to show how Oakeshott responded to the thinkers important to him, and his second aim is to respond to a number of the criticisms made of Oakeshott by commentators such as Farr and Gerencser. Coats shows, for instance, how Montaigne and Hobbes inspire Oakeshott's elaboration of civil association as an arrangement for individuals to live in proximity while pursuing unique purposes. Behind Montaigne and Hobbes stands St. Augustine. As Coats reads Augustine and Oakeshott, they share the view that there are two categorically distinct classes of actions: those done for their own sake and those done for some other sake. Politics is done for some other sake, it cannot be done for its own sake. This departs from Plato and Aristotle, who imagine we can do politics both for its own sake and for some other sake. Politics is thus seen as an interminable earthly activity in which the dissatisfied seek satisfactions with only evanescent success. A civil association would in practice be a plurality of individual pursuits in which it is no shame to care little for politics while acknowledging that politics is inescapable. We conduct politics in order to live, we do not live in order to conduct politics.

Coats considers also Oakeshott's relation to Hegel and Hume. In doing so, he addresses directly Farr's thesis about the advent in Oakeshott's late thought of an ontological individual. He agrees with Farr that there is a tension in Oakeshott's thought, but he thinks it has been present from the 1940s. The differences between the essays in *Rationalism in Politics* and *On Human Conduct* relate to the difference between emphasizing the constraints on the exercise of one's agency and emphasizing what the character of individuality is. This is a dialectical understanding that mediates the either/or.

Central to Oakeshott's thought, as Coats sees it, is his image of "the poetic character of all human activity" (p. 107). Amid the historic practices and arrangements that constitute the context in which we must operate, there is the possibility of momentary release from the deadliness of doing in the encounter with the poetic, allowing that delight in the present which is, for Oakeshott, the most fully human moment, a moment in which we do not dwell on what we have been or imagine we might become. Thus also Oakeshott's "conservatism" as enjoyment of present possibility is coupled to his "liberalism," with its emphasis on the individual seeking self-understanding and the satisfaction of imagined and wished-for outcomes (whether or not one thinks the world will be thereby ameliorated). Either of these tendencies may dominate in any given person, but both must be present in human beings, just as continually figuring out how we are to relate ourselves to one another combines accepting existing patterns with intimations that such patterns are not all we would wish them to be (or not to be). Oakeshott expresses this outlook both in the essays of the 1940s and in *On Human Conduct*. Coats prefers the tentativeness of Oakeshott's explorations, presenting them in the conversational mode that cannot have been far from Oakeshott's actual intent.

One can learn, in taking these books together, that what Oakeshott had to say can excite a conversation about the possibilities and limits of politics pertinent to our time and place, in an idiom that to some degree rescues us from the vulgarity that intrudes when we forget the price we pay when we think either that we can get rid of politics or that politics is the source of all meaning.

The Measurement of Voting Power: Theory and Practice, Problems and Paradoxes. By Dan S. Felsenthal and Moshé Machover. Cheltenham, UK: Edward Elgar, 1998. 322p. $90.00.

Simple Games: Desirability Relations, Trading, Pseudo-weightings. By Alan D. Taylor and William S. Zwicker. Princeton, NJ: Princeton University Press, 1999. 246p. $59.50.

D. Marc Kilgour, *Wilfred Laurier University*

In the 2000 U.S. presidential election, the Electoral College, for the first time in a long while, did not smoothly and seamlessly select the popular vote winner. This phenomenon did not figure in the immediate controversy over the election, but it may yet have an effect, and part of that effect may be long-overdue attention to the rules for aggregating individual votes into a group decision. Those who design institutions for group decision making, or who study such institutions, may be reminded that aggregation procedures can fail to reflect important principles, even principles as simple and fundamental as one person, one vote.

For those with an interest in the analysis and development of voting systems, these books are timely. Both are potentially valuable and in quite different ways. Both treat binary decisions, the most elementary and probably the most common of group decisions. Based on a "yes" or "no" from each individual, a binary decision rule, called a simple game or simple voting game, determines whether the group as a whole selects "yes" or "no." In other words, a simple game is specified by a rule determining which sets of "yes" voters are sufficient for a "yes" decision by the group. Some games are very simple (e.g., under a simple majority rule, a winning coalition is any set containing more than half the voters); others are so complicated that the easiest way to express them is an exhaustive list of winning coalitions. Practical examples include elections with two candidates; yes-no referenda; legislature or committee voting on resolutions, including amendments; jury decisions; and constitutional amendments.

Felsenthal and Machover systematically examine a priori voting power in simple games, evaluating the many techniques that have been proposed for measuring it. Taylor and

Zwicker analyze binary decision rules at a more abstract level, describing and relating some of their deeper properties. Both books are important and relevant because simple games are basic models of fundamental political processes: There may be much here for political scientists to learn and apply.

The idea of measuring a priori voting power, painstakingly explained by Felsenthal and Machover, is to assess and compare individuals' influences on group decisions. Note that in one-person, one-vote contexts, individual voting power is not an issue; but power is meaningful when differences invite comparisons. And differences occur often in practice: when there is weighted voting (as in a shareholders' meeting, or in bodies such as the European Union); when some participants have veto power and others do not; or when participants' roles are distinct (as in the U.S. Congress, with a president, vice-president, 100 senators, and 435 representatives). The voting powers associated with the various roles are products of the rules, which makes them a priori measures, that is, meaningful only prior to the assignment of specific individuals—with their different preferences, bargaining abilities, and alliance propensities—to specific roles.

Many have argued that such a priori power can be important, particularly in constitutional design, where it is axiomatic that the rules under which decisions will be reached should be selected before the issues are known. Felsenthal and Machover make this point well. But it has also been claimed that the more that is known about the institution, the less relevant are a priori measures of power, which are mere abstractions that say little or nothing about the actual game being played. (A recent debate can be found in "Symposium on Power Indices and the European Union," *Journal of Theoretical Politics*, vol. 11, no. 3, July 1999, and in two symposia in *Homo Oeconomicus*: Manfred J. Holler and Guillermo Owen, eds., "Symposium on Power Measures," vol. 16, no. 4, 2000, and Mika Widgérn, ed., "Symposium on Power Measures," vol. 17, nos. 1 and 2, 2000.)

As is often the case, both sides are partly right. All power measures are based on an abstraction, the simple game. Sometimes, in the design of constitutions, for example, the contents of this abstraction may be all or almost all that is known about the institution; in other cases, known institutional details may far exceed the modelling capability of the simple game. An easy test is available: If the simple game tells you most of what is interesting about the interaction of the individuals, then use a power measure; but if the features of interest, such as relative policy positions or strategies, cannot be represented in a simple game, then look elsewhere.

One problem with power measures is that there are many of them, so selection is a problem. Felsenthal and Machover sort out exactly what the measures do and provide a credible assessment of which are good for what purpose; this is the greatest strength of their book. They distinguish two different kinds of power, I-power (for influence, best evaluated by the Banzhaf measure), and P-power (for power as a prize, best evaluated using the Shapley-Shubik index). Much of the controversy over Banzhaf versus Shapley-Shubik is explained in terms of these different conceptions of power: Does more power mean greater influence on outcomes, or is it the ability to command a larger share of the spoils of victory? In the latter view, power is fundamentally relative (shares can be compared); in the former, it is closer to absolute (it measures the scope for individual influence). As *The Measurement of Voting Power* demonstrates, an inquiry into the comparative merits of power measures raises fundamental questions about the nature of power.

In studies of voting power, a *paradox* is an example demonstrating that a particular power measure does not exhibit some property that one might intuitively expect. Felsenthal and Machover follow this norm, using vulnerability to paradoxes to assess (i.e., criticize) measures. They dismiss the paradox of large size, which occurs when the power of a bloc is less than the sum of its members' individual powers. But they regard vulnerability to the bloc paradox—a player "annexes" another but finds its power reduced—as sufficient grounds to reject all but the Banzhaf measure and the Shapley-Shubik index. The greatest contributions of *The Measurement of Voting Power* are the systematic examination of paradoxes and how well power measures avoid them, and the clear and credible conclusions about which measures to use and when.

Although often lively, the writing style of Felsenthal and Machover is sometimes argumentative. They tend to take strong positions on issues, including side issues. For example, they pointlessly open the question of what is "game-theoretic" with their claim that, notwithstanding that both are defined for any simple game, the Shapley-Shubik index is game-theoretic and the Banzhaf is not. The only apparent ground for this claim is their evidence that the Banzhaf measure was first proposed by the statistician Penrose. (Later, Banzhaf and then Coleman proposed it independently.) Their language can be unnecessarily critical; they are quick to suggest that other authors "overplay" points, or are "misguided" or "circuitous." Not all of their criticism is undeserved, but some of it is. For example, they are correct that Straffin's independence assumption could be expressed much more compactly, but leaving it in a form that emphasizes the contrast with his homogeneity assumption makes an important point. Still, for most readers, any problems with style are a small price to pay for the systematic and comprehensive approach of *The Measurement of Voting Power*.

In contrast to all this controversy over meaning and interpretation, *Simple Games* is a math book. All definitions are precise, all examples are meant merely to illustrate, and all claims are proven rigorously. For major theorems, several independent proofs may be presented. This is a mathematical monograph, and those trained in other disciplines will find the level of mathematics demanding if not daunting. Still, the book contains a broad and deep analysis of simple games, which are fundamental building blocks for political structures.

Simple Games has a general theme, which might be described as the relation of game properties to trading. Note that many simple games are not weighted-voting games; real-world examples include the U.S. Congress and the 1982 Canadian constitutional amendment scheme. In fact, a major theorem states that weighted-voting games are characterized by the property of trade robustness: It is not possible to begin with two winning coalitions, exchange some players of one for players of the other, and end up with two losing coalitions.

Although weighted games are structurally simpler, some nonweighted games share properties with them. For instance, in a weighted game a player is more powerful than another with less weight, in the sense that replacing the second player by the first can make a losing coalition win but cannot make a winning coalition lose. There are nonweighted games in which this power ordering, called desirability, is well defined, and they also can be characterized in terms of trade robustness, but under a more restricted form of trading. Moreover, they can be described using so-called pseudoweightings. But note that there are some games in which no such power ordering is available, which gives a new perspective on the problem of finding cardinal measures of relative or absolute power. Further generalizations of the notion of desirability occupy most of the second half of the book.

Taylor and Zwicker provide in-depth mathematical theory of a kind not often found in political science. Their primary audience is game theorists, by which they do not mean economists. *Simple Games* is itself proof that there is great subtlety and complexity hidden in this straightforward model. But whether these mathematical results can ever be adapted to produce practical group decision processes, embodying principles appropriate to real-world institutions, is another question, one that Felsenthal and Machover, with *The Measurement of Voting Power*, help us answer.

Thucydides' Theory of International Relations: A Lasting Possession. Edited by Lowell S. Gustafson. Baton Rouge: Louisiana State University Press, 2000. 262p. $55.00 cloth, $24.95 paper.

Robert C. Bartlett, *Emory University*

Too often edited volumes are a farrago of barely related essays that amount to the academic equivalent of the proverbial camel: a horse built by committee. Lowell S. Gustafson is to be commended for compiling a coherent collection of essays that are united not only by their common conviction that Thucydides' great work does indeed possess a "theory of international relations" but also by their common desire to contrast this theory with current trends in the discipline. According to the contributors, Thucydides is not the father of "realism" or its variants because his understanding of international politics is essentially moral: Every approach to international politics that studies power to the neglect of justice, necessity to the neglect of freedom, will prove to be an inadequate tool with which to understand the political deeds of human beings. However much we may be impinged upon by necessity, we remain fundamentally free, and however much we may seek brute power, we nonetheless also strive to be just.

The book is divided into four sections ("Thucydides as Theorist of International Relations"; "International Politics and the Regime"; "Varieties and Refinements of Realism"; and "Ethics of International Relations"), and each contains helpful contributions. Laurie M. Johnson Bagby's excellent overview of the "fathers of international relations" culminates in the programmatic suggestion that Thucydides' work supplies a model for a political science that is genuinely scientific precisely because it is thoroughly political ("normative"). In part 2, W. Daniel Garst persuasively argues that neorealism yields "an overly simple and misleading account of the behavior and interaction of states" (p. 84) because it neglects the domestic sources of international conduct; by drawing attention to the care with which Thucydides details those domestic sources, Garst demonstrates that the neorealist reading distorts or neglects Thucydides' complexity. Jack Riley, in his sweeping account of Athenian imperialism, especially in its tension with the demands of sound domestic politics, makes the provocative argument in part 3 that "human reason and statesmanship" (p. 150), guided above all by a certain moderation, may yet succeed in doing what the Athenians only came close to doing: combining freedom and empire.

Yet, as important as it is to show the inadequacy of a realism that issues in either "an overly simple ... account ... of states" (p. 84) or a "superficial" reading of Thucydides (p. 78), we run the risk of misconstruing Thucydides' profound understanding of states if from the beginning we apply only our own concerns or categories to his work. Thucydides chronicled the Peloponnesian War because he believed it revealed something true about "the human way"

or "human nature" (*History of the Peloponnesian War* I.22.4 as well as III.82.2 and 84.2); by his own account he sought to understand the necessities at work in human nature, especially with respect to those claims that most presuppose our freedom from necessity, the claims of justice.

It is not quite accurate to suggest, then, that "what most interests Thucydides ... is the question of how this distribution of power [in fifth-century Greece] came about, particularly why Athens rather than Sparta acquired an empire" (p. 69). Indeed, several contributors put questions to Thucydides that are not his own, and it is not surprising that they prove dissatisfied with his answers to them. Thucydides is said to be burdened by a "limited methodology" (p. 62; also p. 233) and "appears unable to provide a definitive diagnosis [of Athens], let alone a prescription for a cure" (p. 214); Plato himself is made to say that Thucydides "cannot teach us anything" (p. 61). The ground of this last claim appears to be the assertion—as unsubstantiated as it is surprising—that Thucydides is not a philosopher but an historian (p. 61; also p. 180). Unlike Herodotus, Thucydides never speaks of "history," although he does speak repeatedly of "nature."

To begin to grapple with Thucydides the student of nature, the philosopher, one must set forth the correct premises and full implications of the "Athenian thesis." In its most consistent form, it calls into question the very existence of justice because it denies the prerequisite, what might be called the freedom of the will. Cities, like the individuals who make them up, are finally in the grip of a certain compulsion. To say, then, that the principle of Athenian foreign policy is that "the strong rule the weak regardless of justice" (p. 167) may not yet be to grasp fully its radical character: One cannot regard what does not exist.

Moreover, to speak of "the flaws of human nature" (p. 207) or of politics as "tragic" (pp. 22, 43, 138, 150, 152, 154, 163, 166, 171, 172, 233, 243) is not only to expect man and world to be fundamentally other than what they are but also to judge them in the light of this expectation. Does Thucydides himself, who never speaks of "tragedy," hold such an expectation? Although the present volume consistently deplores "realistic" arguments from necessity, often doing so in vivid language (e.g., "pathological thinking," p. 217), to deplore a position or its consequences is not yet to refute it. The greatest contribution of the book is to remind students of Thucydides how difficult that task is and how much depends upon it.

Thucydides' own view of justice and its fate in the world is admittedly controversial, but for that very reason it must remain an open question whether he "judges politics" by means of a "moral compass" (p. 154). In his "icily controlled prose" (p. 233), it is surely hard to see that he was "overwhelmed" or even "appalled" by the "tragedy" (pp. 233, 43) he witnessed. Could it be that in helping us to recover the forgotten ground of his remarkable equanimity, by compelling us to reflect on the central question he insists we see, Thucydides' account of the Peloponnesian War may prove to be for us, too, a "lasting possession"?

Poisoning the Minds of the Lower Orders. By Don Herzog. Princeton, NJ: Princeton University Press, 1998. 559p. $29.95.

Ian Harris, *University of Leicester*

Readers of Don Herzog's earlier volumes, *Happy Slaves* (1989) and *Without Foundations* (1985), will know that they should expect a bracing time when they open his books. They will not be disappointed. This is, without a doubt, a book

buoyant in tone and in content. It marks a new departure in the author's manner of writing, displays a wide historical learning, and shows a striking lack of cant in its attitude toward political thought.

Herzog has written a book about conservatism and democracy, or more precisely about the conservative thought he finds in England of the late eighteenth and early nineteenth centuries. He complains of the balkanization of learning, in which people write for a limited circle of those who share their views, and part of the point of the book is to pursue another method. Herzog writes from a point of view he describes as that of an "unreconstructed liberal," who hopes "to strike a modest blow for the rationality of free speech by scrutinizing conservative ideas" (p. ix).

The structure of the book is tripartite. The first part, "Enlightenment," which contrasts conservatism and enlightenment, suggests that British conservatives thought that the minds of the lower orders were being poisoned by the spread of democratic ideas. Its conclusion is altogether more subtle, which is that the conflict was over epistemic authority. The second part, "Contempt," devotes less time to exploring that thought than to unpleasant aspects of the social hierarchy that was then at stake. Herzog suggests there is much here that should disquiet conservatives ("I mean the real ones, the ones with the infamous social agenda" [p. xiii]) and democrats (p. xiii) alike. The third part, "Standing," looks at what it was like to be regarded as a social inferior in Georgian Britain, in the interests of getting away from abstract treatments of equality in order to see what the receiving end of inequality was like.

This is a vigorous book. The author's independence of thrust, his energy of research, and his engaging style are admirable. I feel, at the same time, that there is a subtle, slim book trying to get out of this direct, large one, and that the two would complement each other. Take the case of Burke. He assumes a new appearance in these pages against the canvas of his society, but Burke was also a resourceful figure. He appears here, not least, as one spokesman for anti-Semitism among many (pp. 311, 319, 516–7). There can be no doubt that there were rashes of anti-Semitism in British society in this period, and indeed later. But Burke is not obviously its spokesman as much as someone who alluded to the beliefs he knew around him, as all politicians do, then as now. His disparaging references to Jews sometimes touched on opposition to Christianity, sometimes on finance, and sometimes on straightforward criminality: These cannot have been exclusively Jewish traits in Burke's mind. They are coupled with references to "very respectable persons of the Jewish nation" and to their "antient religion.". They are complemented not only with public concern for an individual, Samuel Cohen, but also with the view that Jews are "the people, whom of all others it ought to be the care and wish of humane nations to protect" (pp. 516–7). In short, there is the broad canvas so vigorously colored by Herzog, but also there are subtle tones yet to be painted into the picture.

If one adopts an historical method of writing about political thought, then one is obliged, for part of the time at least, to adopt the manner of an historian. Herzog certainly does this, in that his erudition is as striking as it is painstaking. Yet, there is another portion of time he needs to give to his material. He needs to ask this question: Did these people think thus because they were conservatives, or because they were people of their time? This question is not posed here, and we cannot exclude the possibility that the latter is the more important point. It would not be difficult to show— Arthur Hertzberg has shown (*The French Enlightenment and the Jews*, 1968)—that nastiness about Jews, explicit or im-

plicit, was a feature of much enlightened thought in the eighteenth century. It is otiose nowadays to observe that Thomas Jefferson was a slaveowner, among other things. In a like way it requires little emphasis that in the mind of the eighteenth century the Rights of Man were just that—women were excluded from the franchise in revolutionary France. This sort of point is adumbrated by Herzog when he discusses Cobbett, but it remains less developed than it might be.

Indeed, this is a point of general reflective relevance. There are in every society crucial indicators of general opinion—turns of phrase, habits of manner, single words— that are consensual or leading features. Anyone with a political or social axe to grind will try to appropriate these for their own. One need only spell "democracy" and "liberty" to be reminded of the contested meaning this produces. So it was here. If "improvement" denoted that intellectual progress would sweep away aristocracy, church, and perhaps monarchy, it also meant that these very institutions might be the preconditions of progress. If "reform" meant widening the franchise in order to curtail aristocratic influence, it also meant reduction of royal power for the benefit of the aristocracy. If "equality" meant a redistribution of private property, it also meant that all conditions were supposed to be equally happy. The point does not need to be elaborated in general, but it needs to be elaborated here. A text entitled "Influencing the Minds of the Lower Orders (Among Others)" would be broader and more nuanced. But it would be wrong to be critical. Indeed, that we have been given much here makes us ask for more.

Plato's Democratic Entanglements: Athenian Politics and the Practice of Philosophy. By S. Sara Monoson. Princeton, NJ: Princeton University Press, 2000. 252p. $39.50.

Larry Arnhart, *Northern Illinois University*

Sara Monoson challenges the common view of Plato as a strong opponent of democracy. Although she acknowledges his severe criticisms of democracy, she argues that his response to Athenian democracy shows ambivalence rather than complete hostility. Not only does Plato offer some qualified endorsements of democratic politics, she contends, but also he presents the practice of the philosophic life as rooted in Athenian democratic culture. Karl Popper's critique of Plato as a proto-totalitarian enemy of the "open society" is not as influential as it once was, but the assumption that Plato and Platonic philosophy are incompatible with democracy persists. Monoson wants to overturn that view and thus convince modern democratic readers that they may have something to learn from Plato.

In part 1 Monoson studies four elements of Athenian democratic life. The first is the story of Harmodius and Aristogeiton as an Athenian myth that celebrates the unity of the city in opposing tyranny. The second is the idea of the citizen as a "frank speaker" (*parrhesiastes*) who will contribute to free democratic debate in the pursuit of what is true and right. The third is the idea of the citizen as a lover of the city, particularly as developed in Pericles' funeral oration. The final element is the idea of the citizen as a theater-goer who experiences the ritual unity of the city and reflects on critical moral and political issues through the theatrical festivals at Athens.

In part 2 Monoson shows how Plato uses these four traditions of Athenian democracy in presenting his view of politics and philosophy. He appropriates the tradition of the tyrant-slayers Harmodius and Aristogeiton by presenting the philosopher as the best opponent of tyranny. He appropriates

the tradition of free speech by depicting philosophy as an exercise in frank speaking. He appropriates the tradition of Periclean funeral oratory by offering his own funeral oration in the *Menexenus*. Finally, he appropriates the tradition of Athenian theater by using theatrical imagery to describe the experience of philosophic inquiry.

Monoson develops her position in a clear and engaging manner. Anyone who wants to think about Plato's relationship to Athenian democracy will learn much from this book, but I suspect that some readers will not be persuaded by her reasoning. For example, the logic of her argument concerning democracy and theatricality seems to go like this: Theater is associated with Athenian democracy. Plato uses theatrical metaphors for philosophy (as in the analogy of the cave in the *Republic*). Therefore, Plato sees democracy as valuable. Such loose logic is not very compelling.

Although Monoson does not develop the thought very far, the most theoretically interesting idea in her book is the suggestion that the life of Socratic philosophy is promoted by the multiplicity—the openness to diverse ways of life—found in a democracy. Although Plato mocks this multiplicity, he also sees it as an intellectual resource for philosophy. The lovely variety of Athenian democracy, the many "patterns" (*paradeigmata*) of life that it displays, gives any human being with a theoretical temperament plenty of material for thought. It may not be just an accident that Socrates was an Athenian. If philosophy is the free study of all human things, and if democracy allows, or even promotes, the freedom of thought and the openness to human variety that make such a life possible, then philosophy may be seen as a product of democracy.

Political Theory and Partisan Politics. Edited by Edward Bryan Portis, Adolf G. Gunderson, and Ruth Lessl Shively. Albany: State University of New York Press, 2000. 226p. $59.50 cloth, $19.50 paper.

James S. Fishkin, *University of Texas, Austin*

What is the role of political theory in a world of partisan politics? Various approaches to this long-standing problem are raised in this stimulating collection of essays. Arlene Saxonhouse begins the volume by usefully reminding us of Plato's metaphor of the ship in book 6 of the *Republic*, in which self-interested sailors fight over the boat's direction "while the one who knows how to guide the boat, who can read the stars, stands aft staring upward and is considered useless" (p. 19). Similarly in the Assembly, self-interested rhetoricians may sway the crowd, without any concern for the pursuit of truth.

The book ends with an equally provocative metaphor. John Gunnell compares the political theorist who may get involved in politics to the anthropologist studying cannibalism who ends up getting eaten. Is there a role for the political theorist between irrelevance and absorption, between the image of the one who knows but is ignored on the ship and the one who gets eaten by becoming a participant?

Donald Lutz suggests one way the theorist can have relevance to real decisions is through teaching. He calculates that out of about 8,000 students he has taught over the years, 49 have held major elective office (p. 37). In addition to teaching, the political theorist can be part of what Edward Portis calls a "constitutional elite." Even if the people are not knowledgeable about a polity's basic principles, a restricted group of the more knowledgeable can be vigilant in supporting their application.

But what kind of knowledge does the political theorist

really have to offer that might be of use in politics and public policy? Most of the contributors embrace some kind of proposition about inconclusiveness, indeterminacy, pluralism, or the essentially contested character of first principles and key political concepts. The question that engages several of the contributors is how far this critique goes. Can we characterize room for reasonable disagreement without any notion of the "reasonable" just dropping out and leaving us with mere disagreement?

For example, in the area of distributive justice, Thomas Spragens argues that "the fundamental unfairness of life and the presence of gratuitous elements in the moral universe make it impossible to settle rationally upon a single set of distributive principles as demonstrably fair" (p. 81). His argument is that when we try to compensate for "natural injustices" we only introduce other forms of unfairness or moral conflict. We can do this, it would appear, but only at the cost of undermining any systematic solution to the question of first principles.

William Connolly adds the dynamic consideration of identity to the case for moral ambiguity and indeterminacy. He asks us to consider "the new movements that changed the register of identities eligible for inclusion in the American practice of justice" (p. 163). He mentions slaves, Indians, atheists, "homosexuals," and even "post-modernists" as those who have been held, at one point or another, to lack the requisites for personhood required for one's moral claims to be considered. Viewing the dialogue about justice as a dynamic process, Connolly provocatively asserts: "It is extremely probable that all of us today are unattuned to modes of suffering and exclusion that will have become ethically important tomorrow after they have been moved by politics across the threshold of cultural attentiveness" (p. 165). He suggests that any attempt to settle conclusively the question of justice would have to confront the fact that our continuing moral dialogue, including what would be admissible to a Rawlsian "original position," would be different at different times. Connolly's conclusion is not, however, to scrap liberal theory. What is needed is "a rewriting of liberal ideals, not their elimination" (p. 169).

Spragens offers probably the most forthright effort to defend the rationality of political discourse, despite the inevitability of moral conflict and ambiguity. "Rational discourse is essential to the creation of a popular will that can pass muster—that is, to the formation of a popular will that can claim to be rational consent rather than aggregate whimsy" (p. 91). Yet, here is an area where a great deal of political science would tell us that public will formation has only limited claims to rationality. Perhaps elites can be informed and rational, but much mass political thinking may be closer to what Spragens fears—"aggregate whimsy."

Adolph Gunderson's essay suggests a road to reform. He charts strategies and institutions that might increase deliberation, not by policy elites but by the mass public. He recommends proposals ranging from reforms in the media, to encouraging deliberative skills in public schools, to increasing the use of deliberative opinion polls (p. 110). His worry is that direct responsibility for policy decisions may encourage partisanship, so he wishes to create public spaces in which the public can deliberate in advisory ways, or in ways that improve the public dialogue, without being too close to actual decision making. He charts a thoughtful strategy that attempts to foster institutions that can engage the public, treat them as citizens rather than as consumers, as deliberators rather than as targets of persuasion. But the task is tricky. He describes it as mid-way "between Athens and Philadelphia." But Athens was not just the home of the mass politics of the

Assembly. It was also the home of deliberative microcosms chosen by lot, and some of them offered notorious examples of defective deliberation—notably symbolized by the fate of Socrates.

Clearly, specifying the conditions for constructive citizen deliberation, a middle ground between Athens (the mass politics of the Assembly) and Philadelphia (the elite deliberations of the Constitutional Convention), requires an entire agenda of research and institutional experimentation. There is now no clear middle ground between Athens and Philadelphia. But perhaps we can create one if we nurture collaborations between theory and practice of the sort suggested by Gunderson, Spragens, and others in this volume.

Truth v. Justice: The Morality of Truth Commissions. Edited by Robert I. Rotberg and Dennis Thompson. Princeton, NJ: Princeton University Press, 2000. 309p. $55.00 cloth, $18.05 paper.

Melissa Nobles, *Massachusetts Institute of Technology*

Scholarship is substantial and growing on "transitional justice," that is, the legal and political decisions devised by incoming democratizing regimes to address the excesses of outgoing repressive regimes and the harms endured by their victims. Truth commissions are perhaps the most significant, if controversial, innovations in a democratizing regime's toolbox. Their significance is largely derived from their peculiarity. Since the early 1970s, approximately 21 commissions have been established in various countries. They have been defined as "bodies set up to investigate a past history of violations of human rights in a particular country—which can include violations by the military or other government forces or by armed opposition forces" (Priscilla Hayner, "Fifteen Truth Commissions—1974 to 1994: A Comparative Study," *Human Rights Quarterly* 16 [November 1994]: 597–655).

That definition usefully captures the basic purpose of truth commissions but does not include their other functions. They are quasi-judicial bodies designed to uncover truths, if not a singular or comprehensive truth. They are often modeled after courts of law and often employ evidentiary standards similar to legal standards, but they are not courts of law. Commission recommendations are not in themselves legally binding, although information revealed by such proceedings has been used in subsequent criminal trials, as in Argentina. Truth commissions turn criminal prosecution on its head by seeming to make truth seeking an end in itself. No less important, they are designed to provide a psychological catharsis, for both individuals and nations, and to lead to reconciliation. Yet, a tension emerges immediately from these two purposes. How can truth, without prosecution, lead to justice? And without justice, can there be true reconciliation?

An answer to these questions is the central objective of *Truth v. Justice.* Although the book takes as its subject the morality of truth commissions in general, it is really about the South African Truth and Reconciliation Commission (TRC). It is the product of a conference held in South Africa in May 1998 and attended by officials of the TRC; South African academics, jurists, and physicians; and foreign scholars. Presentations at the conference comprise the book's 14 chapters. Like most edited volumes, particularly those that result from conferences, the essays are sometimes redundant and of uneven quality.

The tension between truth and justice that the book addresses is rooted in the political circumstances of the TRC's creation. In negotiating the transfer of power from the

white minority National Party (NP) to the black majority African National Congress (ANC), a compromise was reached. A blanket amnesty would not be granted to former government officials (including military and police forces), as desired by the NP, and there would not be wholesale criminal prosecutions of former apartheid leaders, as desired by the ANC. Instead, parliament established the TRC, and persons who committed "gross human rights violations" for political reasons were to be granted amnesty if they fully disclosed their crimes before the Amnesty Committee. Victims were allowed to speak publicly of their experiences before the Human Rights Committee. This compromise has been justified as the most desirable politically, given that the feared alternative was violent war.

Yet, the morality of the TRC has been questioned. If justice—that is, criminal prosecution—is traded for truth and future reconciliation, then a defense of and justification for this trade are required. The chapter by Amy Gutmann and Dennis Thompson analyzes this central issue and thus serves as the conceptual anchor. As they see it, truth commissions come with a "heavy moral burden," since these and other such transitional institutions "sacrifice the pursuit of justice as usually understood for promoting other social purposes, such as historical truth and social reconciliation" (p. 22). If new democracies are to bear this moral burden, then the justification for truth commissions should meet three criteria, which constitute the "moral foundations" of truth commissions. First, the justification must be moral in principle, in that it "should explicitly appeal to rights or goods that are moral" and are thus "comparable to the justice being sacrificed" (p. 23). Second, the justification should be moral in perspective, in that it seeks to be broadly accessible to and inclusive of all persons seeking "moral terms of social cooperation" (p. 23). Third, it should be moral in practice, meaning that the justification can draw on the actual proceedings of the truth commission as an example.

In separate chapters, Elizabeth Kiss and André du Toit also provide thoughtful and persuasive justifications for the TRC, and du Toit directly responds to the argument of Gutmann and Thompson. Neither Kiss nor du Toit is content to have the TRC understood solely in terms of political compromise and to have its brand of justice judged insufficient because it was not retributive. Indeed, both argue that the TRC has helped create another kind of justice—"restorative" justice—that is distinct from and better than retributive justice. They do not deny the political compromise that created the TRC, but they assess the commission in ways seemingly independent of its origins.

According to Kiss, truth commissions are morally ambitious, and therein lies their significance and potential to advance restorative justice. In her view, restorative justice is a coherent and legitimate but risky framework for addressing past abuses, and truth commissions are ideally suited to deliver such justice. Restorative justice is defined by the TRC itself as "concerned not so much with punishment as with correcting imbalances, restoring broken relationships—with healing, harmony and reconciliation" (p. 69).

André du Toit also provides an alternative to retributive justice against which the TRC's morality should be judged. He argues that the kind of justice required during periods of political transition differs from that required in established democracies, and he judges the Gutmann and Thompson moral criteria inappropriate for the TRC. Instead, du Toit maintains that the TRC's constitutive moral conceptions of "truth as acknowledgment" and "justice as recognition" meet the demands of justice required during political transitions

(p. 123). Justice, for du Toit, has neither universal nor ahistorical meanings.

Other chapters usefully examine other aspects of truth commissions, although nearly all focus on the TRC. Three essays tackle different aspects of amnesty and other legal procedures. Two works critically analyze written reports produced by truth commissions, and historian Charles Maier distinguishes between the craft of serving history versus that of serving justice. As actual participants in the preparation of the TRC's final report, Charles Villa-Vicencio and Wilhelm Verwoerd provide interesting insights into the production of official truth.

Overall, the book makes an important contribution to the growing transitional justice literature in general and to the TRC literature in particular. The focus on the TRC, however, with only occasional references to other truth commissions, makes it difficult to draw general lessons. As nearly all the authors state, the TRC was extraordinary. It may well serve as a model for present and future commissions, but it is not representative. Nonetheless, inquiry into whether decisions that pass political tests can also pass moral tests is worthy of sustained analysis, along the lines provided in this volume.

Democracy, Justice, and the Welfare State: Reconstructing Public Care. By Julie Anne White. University Park: Pennsylvania State University Press, 2000. 173p. $35.00, cloth, $17.95 paper.

Russell L. Hanson, *Indiana University Bloomington*

Modern welfare states offer cash payments, public housing, medical assistance, food subsidies, and a wide range of social services to needy individuals. The assistance is rendered by professional social workers and administrators who have the authority (and responsibility) to determine who is needy, what their needs are, and how those needs can be met equitably and efficiently. This top-down approach is both ineffective and paternalistic, according to Julie Anne White, who advocates a system of care based on democratic deliberation rather than bureaucratic rationality.

White's critique of welfare paternalism draws on her case study of New Futures, a philanthropically inspired effort to improve service delivery in five American cities by coordinating the actions of various social service agencies. Although cooperation increased, the experiment failed to achieve its stated objectives of reducing school dropout rates, the rate of teen pregnancies, and teenage unemployment. Moreover, "the project left unchallenged the institutionalization of a class of caregivers...and a class of recipients of care, a division of labor that reinforced a paternalistic model of care" (p. 19). This is because the problems of the poor were defined in technical terms, and problem solving was therefore dominated by experts who seldom consulted with the intended beneficiaries. Not surprisingly, the latter came to view the experiment with great suspicion, which undermined its chances of success.

The reform failed not because of arrogance or bureaucratic bungling; it reflected the very notion of dependency upon which welfare policy is predicated, or so White argues. She notes that a housewife who stays home with her children while her husband works is no less financially dependent than the single mother on welfare. Yet, there is no groundswell of calls for the husband to determine his wife's needs and decide whether or how they will be met within the family. That form of subjection is reserved for the single mother on welfare,

whose dependency is socially or, as White insists, politically constructed along paternalistic lines.

Precisely because it is a political construction, the relation between client and state should be democratized, according to White. The alternative is to end welfare, which some say is incompatible with ideas of individual autonomy that are central to liberalism and democratic citizenship. For White, considerations of justice augur against this move, and she aims to reconcile justice and welfare in a democratic society. This requires a reconceptualization of justice, which is the core of this reflective book.

White rejects philosophies of justice as fairness, conceived in terms of impartiality and a substantive commitment to rights, as paternalistic. She likewise resists arguments that place a parochial ethic of care over and above distributional considerations. Instead, White advances a conception of "justice as fairness of caring," stressing the importance of consulting care recipients, not just caregivers, in social programs. Thus, she advocates a "more participatory process of defining needs, where the discussion privileges the voice of those presently 'in need' in the course of defining 'need' and determining arrangements of resources to meet those needs" (p. 136).

As an example, White cites the Beacons programs, which are collaborations between communities and schools in metropolitan New York. These projects have successfully engaged young students in extracurricular affairs and expanded the opportunities for adult education in poor communities. Unlike New Futures, Beacons projects are grassroots operations, and clients play an important role in defining community problems and designing solutions. This participation is the key to success, according to White, and it constitutes a model of democratic welfare provision or caring.

White assumes that welfare dependency is a political construct and, as such, should be democratized. It can be argued, however, that not all politically constructed relations can or should be democratized. The diagnosis and treatment of medical ailments, for example, are unavoidably technical, and although we might agree that patients should be consulted about options, would we want to "privilege" the choice of a person carrying an infectious disease if that choice increases others' risk? Do we think it is wise, just, or caring to "privilege" the voices of children, the emotionally distressed, or the mentally incompetent in deciding which of their needs to meet and how to meet them? In short, are not there instances in which paternalism is warranted, even in a democratic society?

White's argument overreaches in another respect, as well. Theories of deliberative democracy are invoked to buttress arguments against a mode of provision in which welfare professionals are presumed to know what is best for their clients. But the author's own scrupulous review of these theories shows that there is only a presumption of equality among the parties to a discussion or decision. Any notion of "privileging" is out of place in deliberative democracy, although it is a key feature of "justice as fairness of care."

Sensing the tension, White recalls John Stuart Mill's assertion that individuals are ordinarily (although not invariably) the best judges of their own interests, but this does not resolve the contradiction for two reasons. First, the competence of a significant number of welfare recipients is in doubt; children and those suffering from emotional or mental illness may not be the best judges of what is in their interest. Second, under conditions of scarcity a fair distribution of welfare services cannot be achieved by privileging each person's sense of needs. Financial limitations require us to set collective priorities on care, and that cannot be done without invoking

considerations other than self-definitions of need. Hence the notion of "privileging," at least in any strong sense of that term, does not work in this context.

White could qualify her argument by retreating from "privilege" to some assurance of participation by clients, or at least those who are capable of assessing their needs. But even this assumes that democracy is to be understood in deliber-

ative terms. Other models of democracy exist, but White does not consider them, even though they point to different conclusions about welfare provision. Ironically, democracy is the only term in the title of the book to escape philosophical scrutiny. That is a noticeable shortcoming in an otherwise thoughtful and constructive criticism of the conceptual underpinnings of contemporary social welfare provision.

American Politics

Stations of the Cross: Adorno and Christian Right Radio. By Paul Apostolidis. Durham, NC: Duke University Press, 2000. 273p. $54.95 cloth, $18.95 paper.

Laura R. Olson, *Clemson University*

By now most readers have a good deal of familiarity with the Christian Right, the political movement of conservative Protestants that began in the late 1970s and continues to thrive today. The movement has undergone significant changes over the past two decades. Interest groups have risen and fallen, and presidential candidacies (namely, that of Pat Robertson in 1988) have failed. Yet, the Christian Right is still seen by scholars, pundits, and electioneers alike as a political force with which to be reckoned.

In this important book, Paul Apostolidis puts forth a compelling argument that helps explain the cultural persistence of the Christian Right. A key to its success and staying power has been the fact that an identifiable and unique subculture exists within conservative Protestant circles. It features its own music, business guides, educational institutions, television networks, and radio programming. This subculture is not explicitly political; in fact, relatively few of its structures are steeped in politics. Yet, the existence of this subculture has profound political meaning not only for its denizens but also for the post-Fordist American society and polity at large.

Apostolidis centers his analysis on James Dobson, a well-known evangelical psychologist-cum-radio show host, and his daily broadcast, "Focus on the Family." Unlike such Christian Right leaders as Pat Robertson and Jerry Falwell, who have garnered substantial attention from the media for their deep involvement in all things political, Dobson flies below the political radar. His popular program does not often address explicitly political issues, and he has for the most part declined to fashion himself as a political spokesperson. Yet, "Focus on the Family" carries great political weight in a broader cultural sense, argues Apostolidis. Because Dobson strengthens in his evangelical listeners feelings of group identity and shared social concerns and mores, he plows and fertilizes the field that will later be harvested by more explicitly political Christian Right leaders.

Apostolidis is a critical social theorist, and his application and reformulation of T. W. Adorno's mode of cultural criticism are the theoretical keys to his analysis of "Focus on the Family." I, for one, very much appreciate his careful and nuanced treatment of the notion of culture, and I find his application of the Frankfurt School's dialectical critical technique to the Christian Right highly innovative. This book would be as useful to readers who are looking for a practical application of critical theory, and indeed a primer on Adorno and cultural criticism, as it is to readers who desire a more empirical lesson about Christian Right radio. Apostolidis effectively prepares the reader for his own analysis by re-

counting and rethinking Adorno's analyses of such cultural phenomena as American jazz, the radical twelve-tone music of German composer Arnold Schoenberg, and above all, early (New Deal era) conservative Protestant radio.

In his analysis of Dobson's program, Apostolidis examines the relevance of Christian Right radio to the current post-Fordist political economy. The analysis comes in three parts, each of which examines a different narrative inherent in Dobson's broadcasts.

The first narrative is that of the Christian professional. According to Apostolidis, "Focus on the Family" contains an ongoing narrative that offers an explanation for the post-Fordist demise of the welfare state. This explanation hinges not on government's relinquishing of responsibility for all its citizens, but on the personal moral shortcomings of private-sector professional service providers. The American health care system, for example, is not inadequate because of lack of government funding and control, but because the professionals who work within it, and the broader communities where they live, are morally bankrupt. Dobson, says Apostolidis, not only "expresses this historical situation, but furthermore actively reproduces it by encouraging certain subjective attitudes on the part of listeners" (p. 128). These subjective attitudes make evangelical listeners cynical about, and prevent them from supporting, radical reform of the welfare and health care systems in the United States. Such support would contradict their very understanding of how people can and should relate to one another.

The second narrative Apostolidis explores is that of the Christian politician as humble leader. Post-Fordism, he argues, has ironically brought about both a decline in electoral and legislative accountability and an increase in media portrayals of politics as accessible to the ordinary citizen. Apostolidis focuses on Dobson's on-air conversations with Oliver North and Charles Colson, both well-known players in major presidential scandals (Iran-Contra and Watergate, respectively). Both men are now heroes of the Christian Right because of their professed religious beliefs and, particularly in the case of Colson, for their outreach to the "unchurched." (Colson runs Prison Fellowship, an evangelical group that works with—and proselytizes—inmates and then helps them adjust to life once they are released.) Both men say their accountability is to God alone, not the American citizenry. On the radio show, North and Colson are portrayed as "ordinary guys," which for Apostolidis is an indication that Dobson is perpetuating the troubling irony of post-Fordist politics in the United States, by reflecting not only the decline in real accountability but also the plastic media portrayals of political figures as accessible and ordinary.

The third and final narrative is about Christian victimhood. Dobson's message to those who feel wronged by society is to encourage them to forgive whoever wronged them. A woman who had been kidnapped by a rapist-murderer told Dobson and his audience how she evangelized her captor and convinced him to accept Jesus Christ as his personal savior; she

then forgave the man for kidnapping her and "ministered to him" while he was awaiting the death penalty. An African American man who had been subjected to a lifetime of racial slurs and insults explained that he had come to rely on the goodness of white people in his life. Two "abortion survivors" told of their forgiveness of their biological mothers who attempted to terminate their pregnancies. All three of these testimonies, says Apostolidis, highlight the post-Fordist acquiescence demanded of women, minorities, and children. Members of these groups should not complain about, or even name, the wrongs done against them. Instead, they should essentially forgive and forget—and Dobson reinforces that message.

Apostolidis, who clearly is no fan of the Christian Right but does an admirable job of treating it with critical respect, concludes by suggesting that the Left could counteract the cultural effects of Christian Right culture by organizing through religious, rather than secular, institutions. Although I find this argument underdeveloped, overall the book is a fascinating and dense exploration of the inner workings of one institution of the conservative Protestant subculture. Apostolidis could have taken more seriously the variations within conservative American Protestantism; for example, the term "fundamentalism" cannot correctly be applied to all, or even most, American evangelicals. I should also caution the casual reader that this book is extremely dense, packed with theoretical insight. Those who do not agree with the Adornian point of departure and the author's insistence on taking seriously the concept of culture will probably not enjoy this book. That will be their loss, as there is much here to be learned about one of the more significant demipolitical movements in the United States today: the conservative Protestant subculture.

Congress and the Foreign Policy Process: Modes of Legislative Behavior. By Cecil V. Crabb, Jr., Glenn J. Antizzo, and Leila E. Sarieddine. Baton Rouge: Louisiana State University Press, 2000. 280p. $39.95 cloth, $24.95 paper.
Foreign Policy and Congress: An International Relations Perspective. By Marie T. Henehan. Ann Arbor: University of Michigan Press, 2000. 232p. $49.50.

Melissa R. Michelson, *California State University, Fresno*

Marie Henehan presents a systematic study of every roll call vote on foreign policy taken by the Senate between 1897 and 1984. She predicts an increase in Senate activity and disagreement on foreign policy after "the emergence of imperialism as a critical issue in 1897, the crisis over involvement in Europe in 1917, the heightened importance of the response to communism in 1947, and the perceived failure of the containment policy in Vietnam between the Tet Offensive in 1968 and the bombing of Cambodia in 1970" (p. 83). She also predicts a gradual decline in activity as the issue is resolved, but increases following major policy failures related to each critical issue.

Henehan uses line graphs to demonstrate that there is a critical issues pattern to Senate activity on foreign policy. With the exception of imperialism, for which there is insufficient data, the graphs are supported by a rigorous statistical analysis that shows Senate activity is affected not only by the rise of critical issues but also by failed policies. One time-series analysis is conducted for 1948 because "there are actually very few roll calls in 1947" (p. 104), but the author argues that this does not disprove her theory, since (1) the lull was only temporary, and (2) roll call votes are only one indicator of Senate activity. These are both very true, but the second statement calls into question the entire project.

Henehan concludes: "The findings show that the critical-issue theory can predict when these spurts in activity will begin" (p. 107). But it does not. In one of three cases, the burst of activity is late.

Henehan also examines levels of disagreement, measured as the number of close votes, amendments, and procedural votes. Close votes are defined as those for which one side receives 50–55% of the vote. Henehan expects the pattern of disagreement to follow the critical issues pattern and uses line graphs to illustrate that it generally does. But this analysis is problematic for two reasons. First, it disregards other variables that might influence the amount of disagreement in the Senate, most notably the balance of power between the two major parties. Contentiousness is also probably related to whether the same party that has the presidency also controls the Senate.

Second, the 50–55% criterion seems to miscategorize votes on treaties, which need a two-thirds majority. For example, the Panama Canal treaties in 1978 were both approved by 68–32 decisions (with 67 needed to pass), which by Henehan's coding were not close votes. The author notes that there were not many close votes in 1978 and argues that this is because the debate on the Panama Canal treaties was not about a new critical issue or the failure of a major policy. In other words, she sees 1978 as confirmation of her thesis. Instead, it suggests a coding problem. In another instance, Henehan's line graph appears to show a large increase in Senate contentiousness in the late 1970s, not in the early 1970s as predicted. Henehan does not seem to notice the spike.

The limitations of using roll call data are duly noted and discussed. Henehan further recognizes that measures of activity are limited, as "there is no way to assess the significance of the activity.... Naturally, one vote to cut off funds for Cambodia is more important than 10 votes on general foreign aid bills" (p. 61). Yet, she seems to forget this limitation later, when she claims that her critical issue theory explains Senate assertiveness and predicts when the Senate will become "vitally" active (p. 147).

In the conclusion Henehan addresses alternative theories of Senate activity, such as party control, honeymoons, and presidential election years. In stark contrast to the sophisticated trend analysis used to support her critical issues theory, these competing explanations are dismissed on the basis of highly aggregated data, simple crosstabulations, and phi statistics. She uses as her measure of party whether the same party controlled the Senate and the presidency, not the amount of control within the Senate. The analysis is far from complete or convincing.

Henehan is so eager to prove her point that she gives short shrift to alternative theories, glosses over inconsistencies in the results, and draws conclusions that go beyond what the data support. Nevertheless, she has created a rich data set for examining Senate activity, and her critical issues theory is well supported, if at times overstated. The book is a valuable addition to the literature and will be interesting reading for congressional scholars.

Crabb, Antizzo, and Sarieddine examine four patterns of legislative activity in the foreign policy arena: (1) congressional assertiveness and activism, (2) congressional acquiescence to presidential leadership and initiatives, (3) close collaboration between the executive and legislative branches (bipartisanship), and (4) significant but essentially different roles played by the president and Congress (division of labor). Four chapters are devoted to case studies that support each pattern, and a fifth describes the influence of lobbyists.

Patterns one and two are well substantiated. The Revolutionary War, the War of 1812, diplomacy preceding World

War II, and the post-Vietnam period are used as examples of congressional dynamism. This final example of assertiveness is questionable. The authors point to the War Powers Resolution as the best example of congressional assertiveness in this period: "If the resolution's provisions turned out to be ambiguous and proved in the end to be unenforceable, the intention of Congress was nonetheless unmistakable" (p. 41). But that intention is a matter of some debate in the literature; many see the resolution as a huge delegation of authority, giving the president the power to make short wars without congressional involvement. To support the congressional acquiescence pattern, the authors discuss the Louisiana Purchase, the acquisition of the Panama Canal site, the Lend-Lease Program, and the Tonkin Gulf Resolution.

The third and fourth patterns are less distinct and less satisfying. Bipartisanship is illustrated exclusively by early episodes after World War II (creation of the United Nations, the Marshall Plan, and the Taiwan Relations Act) that were driven by concern about the Soviet threat. The division-of-labor pattern consists solely of the Persian Gulf War. These instances were unique and seem unlikely to be repeated or to help predict future congressional behavior. And in many respects the Persian Gulf episode as presented seems to exemplify congressional acquiescence to presidential leadership, not a new pattern. The authors claim that Congress "made a significant, and at times essential contribution" (p. 124) by providing legitimacy to Bush's actions, demanding that other nations and the United Nations share part of the burden, and insisting that the administration clarify its goal. But they also note that Bush did not consult with Congress before making decisive diplomatic and military moves, and "the impulse toward congressional assertiveness in foreign affairs was remarkably restrained, to the point most of the time of being altogether absent, throughout the Persian Gulf crisis" (p. 125).

The case studies are not summarized in a way that provides for a theory of when Congress will choose to follow any of the four patterns. When Congress will be assertive or will defer to the president is left unanswered. In the conclusion, the authors note that "the phenomenon of legislative dynamism in dealing with external policy questions is quite clearly associated with periods of weak presidential leadership in foreign relations," and "forceful legislative intrusions into the sphere of external policy are also associated with periods of what might be called public-opinion groundswells regarding America's international role" (p. 160). But information on these variables is never presented in any systematic way.

The chapter on interest groups and lobbyists provides valuable information about congressional decision making in the foreign policy arena, but it is not placed within the framework of the four patterns. The section seems tacked on and detracts from the main discussion.

Despite these problems, the book is not without merit. It gives good overviews of most of the case studies chosen. Chapters 1–3 and 6 (the conclusion) in particular are good reading for an advanced undergraduate class on congressional-presidential relations.

Congress at the Grassroots: Representational Change in the South, 1970–1998. By Richard F. Fenno, Jr. Chapel Hill: University of North Carolina Press, 2000. 170p. $34.95 cloth, $16.95 paper.

Burdett A. Loomis, *University of Kansas*

Not long ago, Richard Fenno was at an American Political Science Association convention, wondering aloud whether anyone might want to publish a case study of a single congressional district over almost three decades. The University of North Carolina Press did, and congressional scholars and students of representation are indebted to the editors there. Just when we suspected that Fenno could not wring one more set of insights from his "soaking and poking" political anthropology, he produces a book that tells a profound tale of political change in the South (and in suburbia), gives us a grounded study of what it means to represent a constituency, and offers an understanding of both the Rayburn and Gingrich eras in the House of Representatives. In addition, students of Congress can enjoy this book in its nuanced referencing of *Home Style*, Fenno's still-relevant study of House members in their constituencies, published in 1978.

Congress at the Grassroots is about the ways in which two members of Congress (MCs), Democrat Jack Flynt and Republican Mac Collins, represented roughly the same Georgia congressional district from the 1950s through 1998. Flynt, who is easily recognizable as one of the congressmen in *Home Style*, practiced a folksy, person-to-person style as a conservative southern Democrat, first elected in 1954. Fenno traveled with him in the early and mid-1970s, after his district lines had been redrawn twice, following the "one-person, one-vote" rulings of the 1960s and then the 1970 Census.

Drawing upon (and often quoting) 25-year-old notes, Fenno delineates Flynt's *"issueless"* (p. 37, emphasis in original) representational style. This portrait takes us back to the heyday of conservative southern Democrats, who prospered in a setting of no serious Republican opposition. Even in the face of a significant environmental controversy, Flynt depended on his highly personal representational routines. All that would change in the 1970s, and Fenno's descriptions produce a clear baseline for comparing the following pair of sketches: Jack Flynt struggling in a new district in a changing South, and Mac Collins developing a conservative style notable for its absence of personal connection. Based on an examination of this one district, both author and reader must address the terms of representation, which every two years, in 435 seats, can be renegotiated.

Flynt inherited a new, more suburban district in 1972, and Fenno's conversations with him in 1972 and 1976, his final reelection campaign, are the most riveting narratives in the book. The congressman understands the general political challenge presented by his new district and yet will not, or cannot, change his behavior to increase his chances of winning. After exploring the new territory in the Atlanta suburbs with Flynt, Fenno writes: "It was the first time I had ever seen the congressman without any idea of where to go or what to do" (p. 57). Following an easy Flynt victory in 1972 and a very narrow win in 1974 over Newt Gingrich, Fenno returned to travel with the congressman during the 1976 campaign. Drawing extensively on his contemporaneous notes, Fenno expertly lays out a legislator's behavior in the terminal stage of his career. Fenno has characterized electoral careers as first "expansionist" and then "protectionist." With Flynt, he finds an MC in the final stages of protectionism—hanging on for one more term, in part because he might gain a bit more power in Washington. Fenno quotes from his notes: "It's a little like watching the last train run through town" (p. 74).

Returning to the district in the 1990s, Fenno finds a completely different representational style being practiced by Mac Collins, first elected in 1992, but a spiritual brother to the Class of 1974 Republican "revolutionaries." Much as Flynt could not change his person-to-person style, Collins frames most contacts in terms of policies. Fenno argues that

changes in context and communications have made it easier to develop a policy-oriented representational style that does not depend on personal linkages. Rather, the message is the message, expressed repeatedly in sophisticated ways. Whereas Jack Flynt reflected the old South's Democratic past, Mac Collins reflects the new South's conservative Republicanism. In itself, such an observation is scarcely earth shattering, but it gains weight from Fenno's unique perspective of thirty years' work in the district.

Congress at the Grassroots is a valuable addition to the literature because of the continuing quality of Fenno's observations, especially as they are tied to a clear-headed view of an extremely complex process. The book adds to our understanding of Congress by looking through the prism of constituency relations and the evolution of districts, but its greatest value is to allow a shrewd scholar to take one more look at the subject that has fascinated him for so long. Fenno was there to watch country stores become convenience outlets, power brokers become supernumeraries, and conservatives register as Republicans, not Democrats.

At first glance, this might seem a slight book on a slight subject. The district is merely one of 435, and the MCs, while competent and successful, are obscure. Yet, as always, Fenno has the wit to understand the broader significance of this extended case. In the end, he touches such important and well-researched subjects as role of party, the change in campaign styles, the coming of ideological partisanship (especially from the Right), and the complexities of representation. All in all, that is not bad for a slender volume and a set of old notebooks.

Uneasy Alliances: Race and Party Competition in America.
By Paul Frymer. Princeton, NJ: Princeton University Press, 1999. 214p. $16.95 paper.

Howard L. Reiter, *University of Connecticut*

The multiracial extravaganza staged at the 2000 Republican convention reminds us of the centrality of race in our national politics. In a work that effectively challenges cherished notions of how the political system functions, Paul Frymer argues that through most of U.S. history the major parties have lacked incentives to promote the interests of African Americans.

Central to Frymer's analysis is the concept of capture. A group is captive when it is so small and politically homogeneous that one party can afford to take it for granted and the other can afford to write it off. This is normally the case for black Americans, who were overwhelmingly Republican from the Civil War until the New Deal and then became almost monolithically Democratic in the 1960s. In three chapters that trace this history, Frymer argues that the modern party system was created in the 1830s precisely in order to remove slavery from the agenda; after that goal failed in the late 1840s and 1850s, African Americans entered their first period of party captivity. From 1865 to the 1930s, Republicans enjoyed their allegiance and perceived that there was little they had to do to keep it. The Democrats were the party of white supremacy, and Republicans, who hoped to build a base among white southerners and feared to antagonize racially prejudiced northern whites, downplayed their own racial progressivism.

After the 1940s and 1950s, when the black vote seemed to hold the balance of power, the parties switched roles. Since the 1960s, the Democrats have been the beneficiaries of black voters. Taking that vote for granted and hoping to woo white backlash voters, Democrats in a variety of ways have tried to

deemphasize their dependence on African Americans. These efforts have included ending "welfare as we know it" and symbolic gestures, such as the Sister Souljah incident. Meanwhile, Republicans have gone, in Barry Goldwater's words, "hunting where the ducks are," in ponds where there are few black faces.

After reviewing this history and placing it in the context of his capture theory, Frymer discusses several other aspects of the political system. He argues that in mobilization drives Democrats focus more on perceived swing groups than on blacks, who are therefore deprived of the efficacy-enhancing effects of being courted. He maintains that it is particularly ironic that African-American Democrats in Congress owe their clout to such nondemocratic norms as seniority and constituency service rather than to party responsibility. Finally, Frymer prudently draws parallels and differences between blacks and other potentially captured groups, mainly gays and lesbians and the religious Right.

The importance of this book is its major theoretical contributions. Frymer's chief targets are big ones: Anthony Downs, V. O. Key, Jr., E. E. Schattschneider, and other proponents of the responsible parties model, and pluralists. In *An Economic Theory of Democracy* (1957), Downs presented a model of partisan behavior based on rational choice assumptions, including the argument that parties are promiscuous pursuers of voters. Frymer points out that some voting blocs may be undesirable, as they would drive away other parts of the coalition. Therefore, even if one accepts Downs's assumption of single-minded electoral incentives, some groups will continue to be pariahs. Key argued in *Southern Politics* (1950) that the lack of two-party competition kept southern blacks down, and such competition would motivate both parties to woo them, but Frymer maintains that two-party competition may be a necessary but not sufficient condition for democratization. Key did not foresee that the post-Goldwater GOP would simply write off southern blacks in its pursuit of white voters. Frymer's portrait of the Congressional Black Caucus contradicts what the responsible parties advocates have argued: It is not plebiscitary processes that best promote the interests of African Americans but the contramajoritarian procedures of Congress, which have enabled blacks to secure beachheads on Capitol Hill. Finally, Frymer's depiction of "a long-term, white-based majority interest in the United States" (p. 20) belies the pluralist promise of shifting coalitions.

The book is not flawless. There are more factual errors than a volume this short should boast. Senator Thomas Hendricks was not a Republican (p. 58); farm workers were not "a majority of the population in the United States until the 1920s" (p. 184); George Wallace did not run for the Senate in 1958 (p. 98); the McGovern-Fraser Commission did not mandate primaries (p. 106); Ronald Reagan did not win in 1980 by a "close" margin (p. 108); Kay Bailey Hutchison was never governor (p. 195); and at least seven names are misspelled. Although Frymer rightly faults the Kennedy administration for its dilatory record on civil rights (p. 97), John Kennedy should be given credit for running on the issue in 1960. Fortunately, these errors and omissions do not undermine the main arguments of the book.

Along with such works as Michael Goldfield's *The Color of Politics* (1997), Robert C. Lieberman's *Shifting the Color Line* (1998), and Michael Omi and Howard Winant's *Racial Formation in the United States* (1986, 1994), Frymer shows the centrality of race in the American political process. In addition, he makes a strong theoretical contribution to our analysis of the functioning of political parties in democratic regimes. *Uneasy Alliances* will be a valuable resource for

scholars and students alike, for both its substantive argu-
ments and its theoretical achievements.

Acceptable Risks: Politics, Policy, and Risky Technologies.
By C. F. Larry Heimann. Ann Arbor: University of Mich-
igan Press, 1998. 188p. $44.50.

James A. Desveaux, *University of California, Los Angeles*

After a promising start by Martin Landau more than 30 years
ago, and advances by a small coterie of mostly Berkeley and
Stanford political scientists and organization theorists, the
study of organizational reliability has largely been orphaned
by American political science. One reason such an important
area of research has been given short shrift is that many
political scientists do not think of reliability as a subject based
in politics. It has been deemed more worthy of the attention
of other disciplines, especially engineering and operations
research. Another reason is that we have become even more
obsessed with transactional efficiencies than was the case
when Landau issued his warnings in 1969. Larry Heimann
presents a nuanced study on the problem of reliability that
incorporates politics, organization, and technology and offers
a series of convincing arguments about some reliability
paradoxes in policymaking. His book centers on the connec-
tion between bureaucratic structure, reliable decision mak-
ing, and the incentives public agencies have for responding to
risk in one way or another.

In earlier work, Heimann focused on the National Aero-
nautics and Space Administration (NASA) and the Chal-
lenger space shuttle disaster in 1986. Building on his past
scholarship, the author offers here a richer and more inclu-
sive structural analysis of NASA's successes and failures, and
he adds another contrasting case study, the Food and Drug
Administration (FDA). He argues that agencies fail in one of
two ways: Either they act when caution and forbearance
should prevail (Type I error), or they delay or fail to act when
action is clearly warranted (Type II error). Much of *Accept-
able Risks* explores the logic behind an organization's strategy
to focus on reducing either type of error. In NASA's case, the
decision to launch Challenger on January 28, 1986, was a
classic Type I error. Conversely, the FDA avoided a Type I
error when it refused to license thalidomide for sale in the
United States in the early 1960s. Were it not for limited
resources and political pressures, agencies would more likely
err on the side of caution, commit more Type II errors and
move more deliberately, but with fewer Challenger-type
disasters. When agencies repeatedly perform tasks without
incidence, however, pressures mount for them to produce
results more efficiently. Legislators turn their attention away
from reliability concerns and more toward whether tasks can
be accomplished more cheaply or, if not, whether they are
necessary. The political message is translated into an organi-
zational one: Administrators begin to worry more about
cheaper, faster, better than about failure avoidance.

A substantial literature explains how and why agencies
respond to political pressures, but most of it is decidedly thin
on the subject of how administrators interpret political
signals and translate them into organizational norms of
behavior. *Acceptable Risks* fills that gap and carries the
problem to the next stage, carefully analyzing the structural
choices that face public agencies charged with managing
different kinds of risky technologies. Heimann's contribution
is in the best tradition of public administration as the author
methodically explores how and why administrators follow one
path or another in coping with constraints imposed by both
political superiors and technological uncertainties. He does

so at two levels. First, after roughing out the general prob-
lematic, Heimann lays out a set of reasonable assumptions
regarding structural alternatives for increasing reliability at
agencies like NASA and the FDA. He then formally models
how structure affects organizational reliability. In the process
he gives some persuasive arguments for how administrators
respond to the reliability problem structurally. Heimann
maintains that, depending on resource constraints and
whether most decisions tend toward the programmed or
nonprogrammed variety, administrators will choose systemic
change (typical of reorganizations) versus tactical adjust-
ment, which involves changing components of the organiza-
tion by, say, augmenting expertise. Second, he connects this
more abstract analysis to decisions made by NASA and the
FDA, explaining the logic behind their choices. He offers the
reader a brief but useful history of both agencies that helps in
understanding the context in which key decisions are made.

In exploring paths of failure, the author puts forth a
reasonably detailed analysis of the variable utility of serial
versus parallel redundancy, demonstrating the reliability con-
sequences of each type. For organization theorists, this may
be the most interesting contribution of the book, especially
since Heimann clearly applies the logic of the modeling
exercises to his NASA and FDA case studies. Landau's
pioneering work on organizational reliability argued for the
introduction of redundant mechanisms into decision-making
processes, but his emphasis, and that of most of his followers,
centered on parallel systems. Heimann demonstrates, both
formally and from his case studies, that in the real world of
administration, problems of moral hazard and adverse selec-
tion among experts can persist, even with parallel redun-
dancy. Indeed, he argues for the superiority of serial redun-
dancy in reducing these agency pathologies and therefore
Type I errors, under select conditions.

Beyond the specifics of NASA and the FDA, many of us
wonder whether public agencies—or any organization—can
be designed to manage highly risky technologies in a failure-
proof manner. Heimann locates his argument somewhere
between two very different perspectives on the subject, the
high reliability proponents and the normal accident theorists.
The former, led by Todd LaPorte, Karlene Roberts, and
Gene Rochlin, argue that complex systems can be made
highly reliable, given an unobstructed commitment to that
pursuit. The latter group, named after Charles Perrow's book
on risky technologies, *Normal Accidents* (1984), argues a
different point. For them, certain technologies and the sys-
tems designed to run them entail a significantly high proba-
bility of failure as part of the bargain.

Heimann seems to come down more on the side of the
normal accident proponents. Agencies like NASA and the
FDA are forced to oscillate between avoidance of Type I and
Type II failures, depending on political pressures to avoid
disasters (usually after the occurrence of one), or to cut costs.
The author terms this the "cycle of failure" for public
agencies. As Mary Douglas and Aaron Wildavsky pointed out
in *Risk and Culture* (1983), interpretations of risk are socially
and politically constructed. The level of risk that politicians
and the population at large are willing to tolerate depends on
a lot of factors. Even for public agencies that manage
dangerous technologies, trying to operationalize the concept
of risk is often an imprecise exercise.

Heimann answers the question of whether there is some-
thing wrong with agencies responding to political pressures
with an "emphatic *no*" (p. 166, emphasis in original). I do not
take issue with his belief in norms of democratic accountabil-
ity, but his answer on this score is decidedly incomplete and
unsatisfactory. Granted, his book is not intended as an

analysis of legislative or executive oversight. Yet, in view of the challenges faced by agencies like the FDA and NASA, some nod might be given to the idea that pathologies exist in how institutions of political accountability are designed in the United States. This problem is particularly troubling as it applies to whether and how agencies might be buffered from some of the cruder, short-term demands of politicians. This is a relatively minor shortcoming, however, in an otherwise sensible and important contribution to our understanding of the design of reliable administrative structures.

Heimann pulls together threads from a number of important traditions in public administration and political science and weaves them into a compelling analysis. There are important insights here for the followers of Gulick, Simon, and Landau. *Acceptable Risks* is a welcome addition to political science and administrative theory. Let us hope it signals more quality work on organizational reliability within political science.

Lobbying Together: Interest Group Coalitions in Legislative Politics. By Kevin W. Hula. Washington, DC: Georgetown University Press, 2000. 208p. $55.00 cloth, $23.95 paper.

Scott Ainsworth, *University of Georgia*

Many political scientists like institutions, in particular exogenous institutions, which guide and constrain actions and allow scholars to concentrate more narrowly on behaviors within well-defined settings. For the interest groups subfield, institutions tend to be more mercurial than those in other areas of American politics. For instance, fundamental aspects of Congress may be institutionalized, but groups and lobbyists come and go. The environment of interests is ever changing. Characterizing the interactions between legislators and lobbyists is made more difficult because of the lack of clear institutional structures that guide or constrain behaviors. The iron triangle concept was powerful and meaningful because it provided at the least a loose framework for the analysis of legislator-lobbyist interactions. Kevin Hula's new book follows the reasoning of Hugh Heclo and William Browne, who argue that the iron triangle concept is outdated and inappropriate. That convenient metaphor suggested an informal institutional structure that is simply no longer appropriate. Without the iron triangle, what can fill the void? For Hula and a growing number of scholars, group coalitions play an increasingly important role in the structuring of legislator-group interactions.

Numerous scholars have noted the increase in the sheer number of organized interests operating in Washington. The growing tendency for groups to work within coalitions expands the potential number of players exponentially. Given these facts, it is particularly important to develop some general characterizations of the interest group environment. How and when do coalitions form? How and when do they act? To address these and other questions, Hula interviewed 130 group representatives connected to the transportation, civil rights, or education policy areas. Citing the work of Burdett Loomis, he characterizes coalitions by their breadth of membership and longevity. Not surprisingly, his interviews suggest that some members are more equal than others.

Hula neatly defines three types of coalition members: core members, players, and tag-alongs. They vary considerably depending on their long- and short-term goals. Core members seek a bill or a key element of legislation. Players are satisfied if they can alter a paragraph or two in a bill. Tag-alongs seek a photo opportunity for their own narrow goals. Hula finds the tag-alongs the most intriguing, and I

agree. They lend their support, even though everyone recognizes that they will not marshal their membership or be particularly active (p. 47). In the words of one lobbyist: "All right, as a favor, use our name" (p. 47). Two possibilities immediately come to mind. The first is that groups may arrange logrolls as they form various coalitions. The second is that the breadth of coalitions should be discounted by legislators and others as they come to recognize that coalitions are less comprehensive than their masthead might lead one to believe.

Hula's attention to interlocks is particularly commendable. Formal or informal links across organizations reduce the coordination costs associated with the formation of coalitions. Interlocks at the board of director level are common and sometimes formally instituted. For instance, the American Council of Education reserves seats on its board for representatives of other education associations. More generally, Hula finds that interlocks are most important for short-term coalitions. Long-term coalitions have fewer, even though they have more time to reinforce their relationships with interlocks. When establishing coalitions, Hula finds that interlocks work in one direction only. Individuals call on their former employers, but organizations virtually never work to track down their former employees.

After reading this book, my appetite was whetted for more. For instance, it is unfortunate that no legislators were interviewed for this project. Hula's group representatives frequently note that they are sensitive to the legislative environment. Education lobbyists differentiate between their authorizing and appropriating environments (p. 163). More than once a representative suggested that a coalition was organized or encouraged by legislators themselves. " 'They have oftentimes told us . . . get together . . . work out what you want . . . [then] let us know' " (p. 103). Or " 'Dingell was on the . . . Committee. He says, 'you guys get together . . . see if you can't find some common ground and come back to us' " (pp. 28–9). One committee highlighted is Transportation, and Chairman Shuster was known to be particularly active in the coordination of group activities. In sum, Hula's interviewees suggest that committee structures and legislators are key aspects of the access game, but Hula did not pursue these issues in the present work.

A greater emphasis on committee structures would complement the increasing focus of interest group scholars, including Hula, on policy areas. For instance, transportation policy is a fairly narrow area and primarily affected by only one committee. In contrast, civil rights and education policy are much broader, and numerous committees have a potential stake. Although Hula asks respondents about their breadth of interests, he does not ask about the number of committees they monitor or lobby directly. Indeed, a strong and compelling result in Hula's work is that groups concerned with multiple issue domains are more likely to work with coalitions. Breadth of interests leads to coalitional efforts. Even within single domains, group representatives may work with a small, medium, or large number of committees. The simple point is that access is affected by structures within Congress.

Although I wanted more information about the legislative issues involved, the importance of what was explored is not diminished. By highlighting the strategic concerns of group representatives as they contemplate forming coalitions, Hula makes a valuable contribution to an area of increasingly importance.

Making Americans: Immigration, Race, and the Origins of a Diverse Democracy. By Desmond King. Cambridge, MA: Harvard University Press, 2000. 320p. $45.00.

Edwina Barvosa-Carter, *University of California, Santa Barbara*

Desmond King offers a richly researched, skillfully written account of the development of a race-based U.S. immigration policy during the 1920s. The shift to a "national origins" immigration policy is linked to domestic racial and ethnic politics and especially to racial segregation. Moreover, King contends that a grasp of the immigration debates and policies of the 1920s is instrumental to understanding contemporary racial and ethnic politics, including the controversies surrounding multiculturalism.

Broadly similar treatments of the social construction of race in the context of U.S. racial and ethnic subordination have been written before (e.g., Michael Omi and Howard Winant, *Racial Formation in the United States*, 1986). The main contribution of this book lies in its detailed historical account of the political factors and debates that culminated in the 1924 legislation which shifted the basis of U.S. immigration policy from individual merit to "race" (equated with national origin). This legislation, which remained in effect until 1965, restricted immigration by southern Europeans on the explicit premise that they were genetically inferior to Anglo-Saxons and therefore unassimilable to American life (p. 194). Drawing upon congressional records, political correspondence, and period scholarship, King spotlights the individuals and organizations—such as the Harvard-affiliated members of the Immigration Restriction League—that in the first three decades of the twentieth century tirelessly advanced antiimmigration and eugenic claims to lawmakers.

Sensitive to the historical context, *Making Americans* contributes greatly to our understanding of the role that specific political actors played in implementing assimilationist "Americanization" programs (chap. 4) and installing eugenicist's claims of the genetic inferiority of southern Europeans, African Americans, and Asians as the basis for U.S. immigration policy (chaps. 5 and 6). The book also includes the voices of those who resisted the definition of "Americanization" as Anglo-Saxon conformity and who recognized eugenic claims as ill-founded pseudo-science that masked politicized racial bias. Overall, King delineates in rich and extensively researched detail the methods and arguments used to change the trajectory of U.S. immigration policy, along with the names and interests of those who successfully deployed those methods.

Although *Making Americans* succeeds admirably as an account of the formation and consequences of 1920s immigration politics, there are some important omissions. First, the author is careful to include the voices of those who resisted the race-based policy, but readers may find too little analysis of how and why their efforts failed. Did resistance simply succumb to the persistent efforts and incremental political successes of opponents? Did positions of political privilege or strategic errors prove decisive? Conclusionary statements that liberal views did not prevail (pp. 64, 222) leave open to conjecture the causes of this failure. Thus, the author suggests that we should learn from the mistakes of the 1920s, but exactly which mistakes caused advocates of liberal and egalitarian policies to lose their political battle against immigration restrictionists remain somewhat obscure.

Second, by emphasizing the 1920s (and the two preceding decades) the book gives little attention to other periods in which conceptions of race and racial hierarchies were also formed, especially in relation to non-Europeans. For exam-

ple, King repeatedly notes the exclusion of Chinese immigrants (which began in 1882), but he offers no analysis of how the dominant discourse on race in the 1920s compares either to the late-nineteenth-century political dynamics of Chinese exclusion or to later developments involving Chinese immigration. Readers are thus left to reconcile for themselves 1920s rhetoric describing Asians as "unassimilable" with the repeal of Chinese exclusion laws in 1943 and current political discourses that characterize Asians as the "model minority." Citing the work of Claire Jean Kim, King acknowledges the latter discourse as a component of "racial triangulation" implicated in the ongoing denigration of African Americans (pp. 289–90). Yet, this dimension can be but little understood through his analysis without greater reference to pre- and post-1920s racial dynamics.

Likewise, the author's emphasis on the 1920s neglects Mexican immigration issues that are today so significant to contemporary racial and ethnic politics. *Making Americans* characterizes Mexican immigration as limited and relatively uncontroversial until the 1950s (p. 235). Yet, the 1910 Mexican Revolution and recurrent economic pressures produced earlier surges in immigration, followed by depression-era repatriation. Political contestation over Mexican presence in the U.S. territories took place both before and after the 1920s. As early as 1846, for example, representatives to the constitutional convention in Texas and, later, in California debated the suffrage rights and citizenship status of Mexicans in racialized and hierarchical terms similar to those that reverberated through Congress in the late 1910s and the 1920s (Juan Gómez-Quiñonez, *Roots of Chicano Politics 1600–1940*, 1994, pp. 221–35). Did these earlier political exchanges set the stage for similar debates regarding southern Europeans after the turn of the century?

The neglect of such possibilities occasionally weakens the book's analysis of recent ethnic and immigration-related politics. For example, King labels the 1998 passage of California's Proposition 227, which banned bilingual education, as a reasonable voter response to the type of poor public policy commonly associated with multicultural politics (pp. 272–3). Yet, attention to the broader history of Latin immigration aside from the 1920s suggests that useful connections can be drawn between Proposition 227 and the resurgence of California nativist politics (e.g., Proposition 187), renewed assertions of English-language dominance, and recurrent claims that political unity depends on homogeneity—all currents akin to if not arising directly from the race and immigration politics of the 1920s. Rather than demonstrate the weakness of multiculturalism, Proposition 227 may point instead to political dynamics that (like the 1924 "national origins" policy) introduce legal structures designed to keep immigrants from the opportunities and resources necessary for first-class citizenship, a denial rendered through continuing cycles of ethnic-racial subordination, and still justified today in the name of "making Americans."

Despite these omissions, *Making Americans* is a truly excellent scholarly work that provides a wealth of insights into the immigration and racial politics of the 1920s. It is a book well worth attention.

Outside Lobbying: Public Opinion and Interest Group Strategies. By Ken Kollman. Princeton, NJ: Princeton University Press, 1998. 215p. $55.00 cloth, $19.95 paper.

Gary McKissick, *Emory University*

In an age when commercials bombard us with plain folk (from central casting) ruminating about the latest proposal in

Congress and when "astroturf" more often refers to synthetically manufactured grassroots activity than to stadium playing fields, evidence that groups frequently "go public" is easy to come by. Nevertheless, these efforts to reach and use the public have received little systematic attention from political scientists. Ken Kollman aims to fill this substantial gap in interest group scholarship. *Outside Lobbying* is an impressive effort, one that should invigorate further inquiry into this important aspect of interest group advocacy.

Kollman builds off a crucial distinction he draws between a policy issue's popularity and its salience. What people think about some issue is likely to be relatively clear to policymakers, Kollman argues, but how much they care about it at any given time typically will be more uncertain and variable (p. 9). Because salience, as a cousin to intensity, helps policymakers gauge the likelihood of eventual (electoral) political action, groups' information about a policy's salience is of value to policymakers.

From this basic insight, Kollman articulates a model of outside lobbying, which signals policymakers about the salience of some issue to their constituents. Although grounded in a formal signaling game (the technical details of which are confined to Appendix D in the book), Kollman's analysis requires no great familiarity with formal models. The accessibility of this book, in fact, is one of its strengths.

Underlying Kollman's account is a fundamental duality that the author does his best to address. In the formal logic of his signaling model, salience must be treated as exogenous to any interest group's lobbying activity. But clearly, as Kollman acknowledges, that is an unrealistic assumption; the lobbying may very well influence salience, at least among some relevant public. Indeed, that may often be the point. He thus distinguishes between the "signaling" role of outside lobbying and its "conflict expansion" role, with the latter meant to capture the essential endogeneity of a policy's salience. Kollman deserves credit for addressing this duality and for doing his best to stretch his conceptual account to accommodate it. But the tension it introduces is not easily navigated, a point that I will revisit shortly.

The main argument is straightforward: Groups that advocate popular positions on salient issues will tend to use outside lobbying strategies. In the lexicon of Kollman's signaling model, groups are divided into high and low types based on the salience of their issues to supporters. On occasion, low-salience types will try to fool policymakers through orchestrated—and, given their type, especially costly—outside lobbying campaigns. But, as Kollman makes clear, the difficulty in making such signals credible can be a powerful disincentive to expend scarce resources on outside lobbying.

To examine the implications of this conceptual account, Kollman takes an empirical approach that is thoughtfully designed and carefully executed. The data, which are derived from systematic interviews with interest group representatives, combine measures of group characteristics with information about policy-specific activities and choices. A particular strength of this approach is the opportunity it affords to match, as best as one might hope, the (public) popularity of a given issue with a group's analogous preferences. Kollman measures salience with similar creativity: the percentage of National Election Study respondents who (1) agree with a given group's position and (2) cite the particular policy issue as one of the most important issues in the survey's open-ended question. One might quibble with these choices, but it is hard to imagine better alternatives for Kollman's purposes. Distinct measures of popularity and salience are crucial to

the author's analytic task, and his research design and measurement strategy meet this challenge nicely.

The decision to collect data at the group-issue level proves especially useful. Because he can still aggregate across groups, Kollman can compare his findings on the prevalence of different lobbying tactics to earlier studies (e.g., pp. 17–8). More important, this level of analysis makes clear that generalizations about group tendencies, which were the main product of earlier surveys of lobbying, overlook the often substantial variation in a group's lobbying across issues. That is, whatever the differences one might observe in a group's central tendencies to employ specific lobbying tactics, Kollman's data clearly reveal that choices about tactics and strategy depend crucially on the particular issue.

Despite the many strengths of *Outside Lobbying*, there are several limitations. The most problematic arises from the duality of salience noted above and the consequent difficulty in fitting outside lobbying into the conceptual straitjacket of a signaling model. One need only recall the two most visible outside lobbying campaigns of the 1990s—opposition to Clinton's health care reforms in 1994 and the tobacco industry's efforts to kill settlement legislation in 1998—to see how misguided the assumption of exogenous salience may be when trying to understand outside lobbying. Both campaigns were at odds with public opinion, at least on the putative question at hand (improve health coverage, restrict/punish tobacco). But the whole point of those efforts, one could argue, was to refocus public attention on other issues that had greater public support (the specter of big government and tax-and-spend approaches, respectively).

Anecdotes do not a theory disprove, of course. But these are two vivid examples of the problem with treating salience as exogenous for the purposes of the signaling model. Kollman is clearly right to remind us that salience matters, but in focusing on public opinion as the crucial conditioner of groups' efforts to signal salience, his approach begs the questions of public opinion with respect to what and salience with respect to what. Many (most?) policy battles, including those Kollman examines, seem easily interpretable as involving several distinct "issues." The North American Free Trade Agreement (NAFTA), for instance, serves as the basis for an entire chapter (chap. 6). In measuring "public opinion" relevant to NAFTA, should we look at opinion about the environment, free trade, the economy, or what? Certainly, different groups threw themselves into the NAFTA fight over these different issues, or at least portrayed the battle in these terms. How such complexity factors into Kollman's analysis, however, is left frustratingly unclear.

I am inclined to think the importance of issue-specific variation will require focusing on somewhat different characteristics and consequences of it. *Outside Lobbying* emphasizes the roles played by the status quo reversion point and different "stages" of the legislative process (see chap. 5, esp. pp. 118–31). These emphases, meant to explicate the conflict expansion exceptions to the basic signaling story, seem at best incomplete explanations of the use of outside lobbying to raise the salience of certain issues. One might instead consider that both the salience of issues and the outcome of the proposal(s) at hand vary in terms of their uncertainty. These dimensions of uncertainty, rather than any intrinsic characteristics of the different stages of the legislative process, seem the more likely influences on groups' decisions about how and when to devote their resources to outside lobbying. Finally, as Kollman admits, *Outside Lobbying* is notably silent as to the actions and choices of other actors. Yet, it seems likely that variation across issues depends crucially on the behavior of other groups and partisan actors (a complication

that Kollman justifiably elects to leave outside the purview of this book).

These are, I think, important limitations on the effectiveness of the book's central arguments. That they emerge as concerns, however, is largely testimony to Kollman's achievement. It is only in the context of his careful attention to the varied purposes and manifestations of this class of lobbying strategies that the need to address these nagging issues becomes so clear. Kollman's study moves us well forward in understanding the logic of outside lobbying, even though the inevitable trade-offs of his approach leave ample room for future scholars to improve on it. This is an important book, one that should be part of any graduate seminar on interest groups and one that scholars of public opinion and policy-making should find stimulating as well.

Reform in the Making: The Implementation of Social Policy in Prison. By Ann Chih Lin. Princeton, NJ: Princeton University Press, 2000. 213p. $39.50.

William A. Taggart, *New Mexico State University*

During the last quarter of the twentieth century there was an explosion in the use of institutional corrections unparalleled in the annals of American penology. The numbers tell the story well: Just before the new millennium almost 1.3 million adults were confined in state and federal correctional facilities, which represents a tripling of the population in just under 20 years. One consequence of such a policy, and there are many, is that eventually a large proportion of these inmates will be released back into society. Their prospects do not look promising in terms of our current knowledge about recidivism rates, and we can expect a large and, most likely, increasing number of these individuals to rejoin the ranks of the incarcerated. The reasons for this policy failure are undoubtedly traceable to a number of forces; although one probably need not look beyond the prison walls as a place to start searching for answers.

Ann Chih Lin's book is a timely and valuable addition to the literature for those seeking ways to break the recidivism cycle. By no means is her work offered as a panacea. Rather, it focuses on one critical component of every prisoner's experience that is generally overlooked, the implementation of correctional rehabilitation programs. To be sure, rehabilitation has been studied extensively, and the consensus, Lin notes, is that it does not work. Indeed, it was this conclusion that discredited the rehabilitation model of corrections and resulted in the more punitive approaches that partially account for the swelling prison population. It is therefore somewhat ironic, a point not lost on the author, that the field of corrections is once again looking seriously at rehabilitation.

Lin's thesis is that rehabilitative programs are implemented in an organizational context that has a profound effect on how they are delivered. Lin argues that asking "what works" with respect to program design, the question that has guided many evaluation studies in the past, overlooks the independent role implementation plays in shaping desirable policy outcomes. In fact, Lin points out, there are examples of all types of prison programs that work, but they are outweighed considerably by virtually identical programs at other institutions that do not work. This suggests it may be the prisons and not the programs that require closer inspection. It is to this arduous task that Lin devotes her attention through a detailed examination of rehabilitative programming at five medium-security, male prisons in the United States. Through the use of hundreds of interviews, site visits,

and archival research, she sets out to discover what works in terms of program implementation.

The book is organized into five main chapters, plus an introduction, conclusion, and methodological appendix. The introduction and first two chapters develop foundational materials needed to understand the implementation processes discovered in each facility, the subject of the next two chapters. The foundation is complex, and Lin does a skillful job of linking the various pieces. The central argument is that implementation is a product of two dimensions: institutional needs and institutional values. Successful implementation, Lin posits, is a function of values that encourage an open, communicative prison environment and needs that are directly or indirectly supportive of rehabilitative goals (p. 56).

These opening chapters also introduce the five medium-security prisons studied by Lin, four federal facilities and one state institution. These are not identified and were selected as typical of the norm. The case study approach, which obviously lacks generalizability, is an important and necessary mainstay of implementation research. Lin employs her two explanatory dimensions to create a typology that organizes the varieties of implementation she unearths, both successful and unsuccessful.

Chapters 3 and 4 examine the five prisons. The first of these chapters focuses on the three cases of implementation failure and the next chapter on the two cases of success. The case studies are rich in detail and reveal that the implementation of prison programs is anything but "one size fits all." Even the two successes work for different reasons.

Chapter 5 is cast in comparatively narrower terms, although it tackles one of the toughest issues: whether program participation should be mandatory or voluntary. The concluding chapter touches on a number of themes, including a call for a greater emphasis on process evaluations that focus on program management and delivery. As Lin correctly points out, successful implementation does not guarantee declining recidivism rates, but implementation failures have little, if any, chance of helping.

Lin's book is a very useful addition to the literature. It suggests that program content is only part of the equation; what also matters is the institutional context in which programs are offered. Policymakers and high-ranking administrators should recognize that their visions of policy goals and priorities are not necessarily shared by either prisoners or prison personnel. Appropriate guidance, incentives, and flexibility are required to allow institutions to mold implementation processes to meet their particular, localized needs. For those interested in public policy generally, Lin offers an adaptable theoretical framework that appears to have other possible applications, such as in the field of education. For all readers it is a solid reminder that implementation does indeed matter.

Social Cleavages and Political Change: Voter Alignments and U.S. Party Coalitions. By Jeff Manza and Clem Brooks. Oxford: Oxford University Press, 1999. 342p. $55.00.

Carole Kennedy, *San Diego State University*

This work is touted as the only book-length examination of the sociological model of vote choice in American politics since David Knoke's *The Social Bases of Political Parties* (1976), and it is, indeed, a well-researched examination of the role that race, class, religion, and gender play in our understanding of voter alignments in the United States. At the same time, I have concerns about some of the methodolog-

ical decisions made by the authors and the effect of these choices on their conclusions.

The text presents a systematic analysis of social cleavages in the American political system, how they have persisted and changed over the past decades, and their effect on voter alignments with the Republican and Democratic parties. Manza and Brooks do a particularly good job describing the evolutionary effect of race, class, religion, and gender on political alignments, using data from the National Election Studies starting in 1952 and continuing through 1996. A great contribution is the precise and illuminating operationalization of the concepts they study, especially with regards to the measurement of class and religious divisions in contemporary America. Their analysis is less convincing, however, in their multivariate models, which attempt to explain why the trends they identify have occurred.

Manza and Brooks suggest that some political scientists have denied the importance of social cleavages as a factor in American politics. I am not convinced that the couple of review articles cited as evidence of this disdain for the sociological perspective actually represent the reality of its place in research done in the political science and political sociology disciplines. The Michigan Model, for example, has endured as a robust model of vote choice and has always admitted the importance of sociological variables as the first element in the funnel of causality. Much contemporary research into partisan alignment and vote choice incorporates a respect for race, class, gender, and religious preferences as important variables to examine. The beauty of the Michigan Model is that it recognizes the power of these cleavages in predicting partisan identification but also identifies candidate-specific aspects that may cause people to deviate from their avowed partisan identification and vote for a candidate from a party other than their own. Although rational choice modelers of vote choice rely less on sociological variables, there is still an implicit acknowledgment of the role of social cleavages in helping to determine where specific and distinctive preferences originate.

Among the authors' strongest scholarly contributions are the refined and improved measures of class and religious identification. Manza and Brooks compose a multicategory class scheme based upon respondents' occupation and employment situation. This results in a sevenfold classification of class location that includes professionals (salaried and self-employed, such as lawyers, physicians, engineers, teachers, scientists, writers, editors, and social workers) and non-full-time labor force participants (homemakers, retirees, students, the disabled, and part-time workers).

Given that the authors have devised new measures of the class variable, it is not surprising that they find trends in class cleavages that have persisted over time rather than declined, as suggested by other scholars who use different and more simplistic operationalizations (i.e., dichotomous blue-collar versus white-collar classifications). Class cleavage, as Manza and Brooks measure it, has fluctuated since 1952, with a peak in the 1964 election, but has not diminished or become irrelevant. Similar refinement of measures is done for religion. Once again, a multicategory scheme is adopted for delimiting various Protestant denominations, which also results in a sevenfold classification: liberal Protestants, moderate Protestants, conservative Protestants, Catholics, Jews, other, and none.

The finding that class and religion are still important variables in understanding political alignments reminds me of the intellectual furor caused by The Changing American Voter (Norman Nie, Sidney Verba, and John Petrocik, 1979). It purported to find hitherto unrevealed capacities of American

voters to engage in issue-based voting, only to discover later that the "changes" in voter behavior were attributable, at least partially, to altered wording in the National Election Study questionnaires. This is not to dismiss completely the findings of either that study or this. Indeed, Manza and Brooks operationalize class and religion in a more sophisticated way than previous attempts and hence advance our understanding of class and religious cleavages over time. The critical reader will also note, however, that the distinctive findings with regard to the persistence of these cleavages in American politics have some basis in the authors' unique conceptualization and operationalization of those variables.

Among the major findings of the study is a renewed appreciation for the influence of social cleavages on political alignments. These have not declined in importance, they suggest, but have consistently been determinative factors from the 1950s until the 1990s. In relative terms, the authors identify the largest social cleavage to center on race, which is twice as large as the religious cleavage. The class cleavage is third in magnitude, and the gender cleavage is fourth. With regard to the class cleavage, professionals (as uniquely defined by the authors) moved from being the most Republican class in the 1950s to being the most Democratic class in 1996. Also, using their sevenfold classification of religion, liberal Protestants moved from being the most Republican religious group in the 1960s to a more centrist position in the 1990s. The great strength of the authors' approach is to control for alternative explanations for shifts in social cleavages and to present findings on how each cleavage affects partisan alignment relative to changes that affect the electorate as a whole. This gives a rich and illuminating portrait of trends over time, which supports the authors' contention that these cleavages are important determinants of shifting partisan alignments.

The authors' attempts to explain why changes have occurred are less convincing. They use a Bayesian approach to model selection (which is inadequately explained or justified in the text or footnotes) and purport to refute conventional wisdom and existing research into the causes of the gender gap in American politics. They choose a model based primarily upon methodological concerns rather than a theory-driven rationale. The models for explaining the effect of religion and gender cleavage on vote choice are selected according to their Bayesian information criterion (BIC), which is based upon Adrian Raftery, "Bayesian Model Selection in Social Research" (Sociological Methodology 25 [1995]: 111–93). Raftery promotes the use of Bayesian analytic techniques that purport to distinguish between nonnested multivariate models and to identify the model that best "fits" the data. This approach is explicitly rejected by Andrew Gelman and Donald Rubin in "Avoiding Model Selection in Bayesian Social Research" (Sociological Methodology 25 [1995]: 165–73). There is a legitimate and defensible difference of scholarly opinion regarding the utility of BIC measures in model selection, but the authors do not address this concern anywhere in the text. A simple illustration, however, shows how this omission mitigates the contribution made by the authors.

In the chapter on the gender gap, Manza and Brooks select among competing models of the emerging gender gap in presidential elections between 1952 and 1992. They select what they claim is the preferred model because it has the best fit using Raftery's index, with a BIC improvement of −2. This model is then used as the basis of a framework to explain the gap as a function of labor force participation. This second model is selected only after discarding an alternative, to which the authors fail to assign a BIC score. In the first instance, the choice is based on a −2 improvement in BIC; in

the second instance, a rejection is made without explicit discussion of BIC.

My own research into the meaning of a BIC score found, on page 139 of the 1995 Raftery article, that a BIC difference of 0–2 indicates only weak evidence of any improvement in fit. A difference of 2–6 is considered positive evidence of an improvement, and a difference of 6–10 is considered strong evidence. Hence, the authors implicitly reject theoretical concerns in model selections, which directly contravenes the admonition in Raftery's 1995 article: "Statistical methods for model selection and accounting for model selection should be used only to address issues left unresolved by theory. Bayesian model selection is not an all-purpose panacea: strong theory, clear conceptualization and careful measurement remain vital for successful social research" (p. 157).

America's Congress: Actions in the Public Sphere, James Madison through Newt Gingrich. By David R. Mayhew. New Haven, CT: Yale University Press, 2000. 257p. $30.00.

Sarah A. Binder, *George Washington University and The Brookings Institution*

America's Congress is a deceptively simple work. At its most basic, it is an exploration of the public moves of members of Congress over the course of American history. With a newly built database of 2,304 observations of members' publicly noted moves stretching back to 1789, Mayhew offers an innovative portrait of how and when American legislators have made their mark on the public record, as recorded by eminent historians of the middle to late twentieth century.

What makes this a deceptively simple work? Mayhew's aim and effect in writing *America's Congress* go far beyond his perceptive reading of the fascinating patterns uncovered. Instead, the book is really a call for a new way of studying Congress and legislative politics more generally. It is a commentary, Mayhew says, on political scientists' treatment of Congress, and it is an appeal to legislative scholars to rethink the dominant modes and methods by which they typically approach the task of explaining legislative behavior and outcomes. To understand how *America's Congress* makes this contribution, a more detailed exploration of Mayhew's mode and methods of inquiry is in order.

Mayhew explores the sorts of actions by members of Congress that "register in the collective public consciousness" (p. 10). He argues that by definition such actions should be considered consequential or at least potentially so, given their notice by politically aware citizens at the time. For Mayhew, these bits of publicly noticed action make up the stuff of public affairs, the central bits of political life in a democratic system. This approach to studying politics is akin, Mayhew states, to studying economics by exploring the public moves of Bill Gates and George Soros, a decidedly "supply-side account" (p. 25) of legislative politics not usually found in studies of Congress. It is an approach that encourages an historical perspective on Congress and its members, as Mayhew asks not only what sorts of public actions are undertaken by members but also in what mix and with what consequence for national politics and institutions over time. As he points out, scholars more typically rely on roll call votes to characterize members' historical modes of behavior, but such data fail to capture the richer mix of legislative behavior that occurs over time and that has a claim to being politically consequential.

The method for recording members' public moves bears a family resemblance to the well-known and pioneering method Mayhew used in *Divided We Govern* (1991) to uncover landmark legislation enacted in the last half of the twentieth century. In *America's Congress*, he combs nearly forty general and era-specific histories of the United States written since 1950 to cull from their indices and texts which legislators have been noticed, when in their careers such notice takes place, and for what sorts of actions they are noticed. Mayhew appropriately showers readers with the richness of the mix of actions recorded, including Henry Clay's maneuvering to craft the Compromise of 1850, Preston Brooks's caning of Charles Sumner on the Senate floor in 1856, and Edward Kennedy's leadership of the fight against Robert Bork's nomination to the Supreme Court in 1987.

This is a methodological undertaking that will inspire awe in even the most hardy coder of legislative politics, and the result is an incredibly rich database that records in intricate detail the precursor moves to critical outcomes and junctures in America's national political life. Mayhew explicitly and honestly recognizes some of the drawbacks to his method, noting, for instance, some examples of conspicuously missing actions from the historical texts. The method may still generate some controversy, however. One might argue that legislators can make politically consequential moves without public or historical notice, and not all publicly noticed actions are necessarily consequential; such concerns could raise questions about the sorts of biases that may have worked their way into the data.

Any such weakness aside, Mayhew uncovers a treasure-trove of patterns in legislative life. One of the most striking observations is that less than half of members' public moves are directly related to passing legislation. "Taking stands" is an equally prominent mode of behavior and more often than not does not explicitly entail legislative action, such as the caning of Sumner for his antislavery views or former House Speaker Tip O'Neill's emergence as the chief Democratic spokesman against President Reagan and congressional Republicans in the 1980s. Such "oppositional" behavior is, Mayhew detects, a central mode of legislative life, a mode that comes predominantly from within the congressional majority party and that has been central to the formation of new parties throughout the course of American political life.

Mayhew's skillful reading of the data also leads him to important conclusions about the institutions of Congress and the presidency. The author draws a convincing portrait in chapter 4 that suggests the House, Senate, and presidency have become democratized over American history and at the same time have grown distinct and equal in influence and legitimacy. Mayhew's ability to detect and explain broad patterns in the evolution of the separation of powers is among the central and innovative contributions of this work.

Mayhew notes at the outset that the book is more an "exploration of the territory rather than a causal analysis" (p. 28). I think he understates the contribution of his work. By focusing on legislators' moves rather than, say, roll call votes, Mayhew makes a real theoretical innovation: He treats members of Congress as cue givers rather than cue takers. Taking gentle aim at legions of legislative scholars who have viewed legislators' policy preferences as exogenously given, Mayhew argues that legislative life is as much about opinion formation as it is about opinion expression (p. 18). That is why focusing on America's public sphere is such an important move theoretically: It forces students of American politics to look explicitly at the realm in which public opinion is molded and cast by legislators and political leaders. If members were simply cue takers, the public sphere would be unimportant. But if politics is also, as Mayhew argues, about the formation of public and elite preferences, then ignoring the sphere of

opinion formation comes at significant theoretical and analytical cost.

Each of Mayhew's books sets an incredibly high standard for the ones that follow. *Congress: The Electoral Connection* (1974) reoriented the study of Congress, and *Divided We Govern* (1991) transformed debates about the effect of divided government. Mayhew's theoretical ambitions in *America's Congress*, matched with his methodological and empirical contributions, have the potential to redefine once again how students of Congress go about the business of exploring and explaining legislative politics.

Dividing Citizens: Gender and Federalism in New Deal Public Policy. By Suzanne Mettler. Ithaca, NY: Cornell University Press, 1998. 239p. $52.50 cloth, $18.95 paper.

Virginia Sapiro, *University of Wisconsin, Madison*

Dividing Citizens is an important contribution both to the burgeoning literature on the historical development of the American state and citizenship and to the lively field of work on gender within that corpus. Like the best studies of this sort, Mettler's book ably demonstrates not only how the transforming institutions and practices affected women but also how gender norms and practices were built into the new structures, making gender a basic element of their architecture.

The author's main task is to explore how governmental institutions create and affect citizenship, especially social citizenship, and the degree to which this citizenship is gender based. Starting with the premise that citizenship "is fundamentally a relationship between citizens and government" (p. 8), Mettler believes that it is imperative to explore "how public policies and institutional arrangements organize the citizenry and shape the meaning and character of citizenship" (p. 9). She emphasizes three means by which institutions and policy do this: defining membership of the citizenry, for example, through naturalization and immigration; shaping participation, for example, by underscoring their existence as either rights bearing or dependent; and incorporation, "the manner and extent to which people are included, consolidated, and organized as members of the political community" (p. 9).

Mettler takes as her framework the period of dramatic state reorganization that characterized the New Deal. Following in part the urging of Judith Shklar that we look at incorporation and standing in the polity, especially of those who are marginal to it, Mettler considers the effect of policy and institutional change on "social citizenship." She focuses on the formation and implementation of three policies: old age insurance and assistance, unemployment insurance and aid to dependent children, and the Fair Labor Standards Act of 1938.

The author's conclusion, in its briefest form, is that through these New Deal policies as they were both developed and implemented, men—white men in particular—were increasingly incorporated into a national citizenship, whereas men of marginalized races and women were linked to the national polity in a much more fragmented and contingent relationship because they were the subjects of public policies that bolstered administrative autonomy for individual states in the federal system. Mettler's policy history recounts a story of the New Deal struggle against the national allergy to substantive rights and its expansion of social citizenship. It demonstrates the degree to which white men were the major beneficiaries of social citizenship at the national level; women

and nonwhite men were substantially left behind in the world of the sick chicken.

Mettler argues that these gender and race differences resulted only partly and occasionally from the conscious will of the primary policy agents to treat people differently on the basis of gender or race. Indeed, the differences were only partly and occasionally caused by nonconscious sexist and racist assumptions embedded in policy deliberation. Instead, she points to the unanticipated consequences of institutional arrangements and political imperatives that shaped policymaking. Of course, although racist or sexist outcomes may not have been anticipated or intended, policy agents certainly tended to tolerate them once they appeared.

Mettler works with two contingent conceptions of citizenship incorporation. The first might be called citizenship categorization, the result of such policies as inheritance, immigration, and naturalization. The second, social citizenship, leads us to ask, assuming one is categorically defined as a citizen, about the nature of the relationship between the citizen and the polity in terms of rights, duties, and welfare. Social citizenship defines the basic quality of life guaranteed to members of the polity. Clearly, polities differ among themselves and across time in the degree to which any notion of social citizenship is embedded in their political culture or public policy. T. H. Marshall's work likely would not have been written by an American.

The book's focus on the effect of these New Deal policy struggles and decisions on the gender- and race-based development of social citizenship is a fascinating and important story that goes beyond the notion of the "two tracks" of welfare so ubiquitous in the gender and state literature. The particular federalism of these policies did not just place different requirements and thus provision outcomes on women and men; it created different versions of social citizenship.

A third and crucial conception of citizenship is touched on by Mettler with tantalizing brevity. This is the more classic question of political citizenship, the active, deliberative component whereby citizens stand as the ultimate source of authority and will in a democracy. She is, of course, primarily interested in the effect of the New Deal on social citizenship, but she also recognizes the possible influence of categorical and social citizenship on the political.

It is conventional to consider the consequences of political citizenship for social citizenship, for example, by asking how citizens use their political rights to pursue policies in their interest. But we can explore the consequences of how people are socially incorporated into the polity for how they are constructed as political citizens. Indeed, some New Deal institutional arrangements explicitly created active and deliberative roles for political citizens, notably in agricultural policymaking and implementation. Social citizenship must affect the political interests on which we might act; it must shape our relationship to the political system, our orientations toward it, our perceptions of it, our assessment of what options for activity and influence are open to us. Surely, it must in some way shape those aspects of our political subjectivity—identity, consciousness, attitudes, perceptions—that help determine whether and how we are active as citizens and whether and how others will accept our activity as citizens.

Suzanne Mettler's attention to the consequences of state development for citizenship marks a unique contribution. It also points to a very slight weakness, but one that renders this book no less valuable in understanding American political development. Lucky for us: *Dividing Citizens* is a benchmark

book in its accomplishments and also points to an important agenda for future research.

Policy Entrepreneurs and School Choice. By Michael Mintrom. Washington, DC: Georgetown University Press, 2000. 324p. $60.00 cloth, $24.95 paper.

Karen Mossberger, *Kent State University*

Policy entrepreneurs or issue advocates figure prominently in major theories of the policy process (e.g., Frank R. Baumgartner and Bryan D. Jones, *Agendas and Instability in American Politics*, 1993; John W. Kingdon, *Agendas, Alternatives, and Public Policies*, 1995; Paul A. Sabatier and Hank Jenkins-Smith, *Policy Change and Learning: An Advocacy Coalition Approach*, 1993). This book explores in depth the phenomenon of policy entrepreneurs, the individuals who invest their time and resources in trying to bring an idea to fruition. The controversial issue of school choice is a particularly apt example of a policy that has spread through the efforts of entrepreneurs laboring in a number of states. A good overview sets the scene in the first chapter and describes such variants as public school choice (within and across districts), charter schools, private voucher plans, and publicly funded vouchers. But the book emphasizes building and testing a theory of policy entrepreneurship. This carefully crafted study presents a number of significant arguments and findings that will be of interest for scholars (or graduate students) concerned with policy diffusion, policy change, and agenda setting as well as education policy.

The author's primary concern is to explain how entrepreneurs promote policy change. Arguing that the term policy "entrepreneur" is essentially a metaphor for an economic concept, Mintrom reviews economic theories of entrepreneurship as a prelude to constructing a political model. The social context of entrepreneurship is common to both market-based and political entrepreneurs, according to Mintrom (pp. 126–9). Immersion in relevant social networks allows entrepreneurs to anticipate needs and demands as well as develop credibility and trust with potential backers. The dilemma is that entrepreneurs also need to maintain sufficient distance to view problems in a fresh way. Policy entrepreneurs face additional challenges in part because they market ideas rather than tangible products (p. 227). Substantial uncertainty often exists over whether or how a policy idea will work. Mintrom identifies a set of attributes policy entrepreneurs need in order to act as effective change agents: creativity and insight, social perception, the ability to mix in a variety of social and political settings, persuasive argumentation, team building, and leadership by example (pp. 152–3).

The author uses two types of evidence to explore his theory: a 1993 mail survey of selected members of the educational policy communities in 48 states and interviews with a purposive sample of policy entrepreneurs. Event history analysis based on the survey data establishes some parameters for the policy significance of entrepreneurs and networks. Mintrom shows that the likelihood of consideration or adoption increases with rising activity on the part of policy entrepreneurs, controlling for a variety of other factors (p. 201). A strength of this analysis is its differentiation between factors that influence agenda setting and adoption. For example, the probability of legislative consideration increases with the number of neighboring states that adopt the policy and with indicators of participation in national policy networks. Neither factor influences adoption, however, which is related to coalition building within states, the level of opposition from teacher unions, and state performance on

standardized tests (pp. 217–9). Together, these findings indicate that momentum from diffusion in the policy stream can affect agendas, but state-specific factors in the political and problem streams are more likely to determine the fate of choice proposals.

The network activity measured in the event history analysis demonstrates the social embeddedness of entrepreneurs but not whether Mintrom's list of attributes matters. At the core of the evidence concerning these hypotheses are case studies of coalition-building strategies in three states: Nebraska, Oregon, and Michigan. Political networks within the state were successfully used when some variant of school choice was adopted. Entrepreneurial activities differed, however, based on whether the entrepreneur was a legislator who could exploit an inside strategy or an "outside" entrepreneur who faced the task of erecting a political coalition from the ground up.

Mintrom uses these cases, and material from interviews in other states, to show how entrepreneurs coped with the challenge of marketing ideas. They accepted incremental accomplishments, such as public school choice, and created demonstration projects, such as privately funded vouchers. Both incrementalism and pilot programs enhanced the potential for further change by demonstrating feasibility and stimulating demand. The marketing of abstract ideas also encouraged entrepreneurs to emphasize different aspects when pitching the "product" to different constituencies. This strategy can expand political support but also introduces the potential for transforming the idea in the process, which contributes to what I have called the diffusion of a policy label.

Overall, this is an excellent study that features careful conceptualization of entrepreneurship and solid research. Creative use of event history analysis allows for generalization beyond discrete cases and explains more about the process of policy diffusion than previous studies that have used this method. Interviews with entrepreneurs bolster findings from the survey of nonentrepreneurs that might otherwise be doubtful, and they also allow for more detailed observation. Yet, the author could have, at times, made even better use of his interviews with policy entrepreneurs. Quotations from entrepreneurs in a number of states appear within the text, but there is no discussion of how strategies in these states related to those in the three case studies. Was one approach more common than the others, for example, or was the limited coalition building apparent in Oregon problematic in other states as well? This would have strengthened Mintrom's evidence on the attributes of entrepreneurs, given their significance for his theory.

What Mintrom offers as a theoretical framework is actually a set of ideal standards against which to measure entrepreneurialism. It will take further research on other policy issues, of course, to establish which of these are most important and under what circumstances. His findings about the importance of incrementalism and demonstration projects may be most relevant to hotly contested issues that face organized opposition, such as teacher unions. It also remains to be seen whether policy ideas are usually accepted only after a considerable period of softening up through debate and compromise, as indicated here, or whether emotional appeals by entrepreneurs sometimes short-circuit deliberation.

By examining this particular piece of the policy puzzle, the author contributes a useful conceptual framework and builds on existing theories of policy change. His work also advances our understanding of the process of policy diffusion, which often has been neglected in favor of research on patterns or rates of diffusion. Mintrom's study offers a much-needed view

of the role of ideas and information from the vantage point of
the policy entrepreneur.

The Politics of Ideas and the Spread of Enterprise Zones. By
Karen Mossberger. Washington DC: Georgetown Univer-
sity Press, 2000. 288p. $65.00 cloth, $24.95 paper.

Richard C. Feiock, *Florida State University*

This book is the latest product of Georgetown University
Press's outstanding American Governance and Public Policy
series. Karen Mossberger embarks upon a substantial intel-
lectual effort and seeks to accomplish several tasks. First, she
provides a detailed account of the evolution of the enterprise
zone concept and chronicles its implementation in five states.
This case analysis contributes to the study of policy diffusion
by focusing on "polydiffusion" though multiple channels and
distinguishing the diffusion of policy instruments and policy
labels. Second, she evaluates the spread of the enterprise
zone concept using a criterion of "informed decision mak-
ing." Third, she assesses whether the diffusion decision-
making process was characterized by a rational comprehen-
sive, bounded rationality, or organized anarchy decision
model. This effort has much to commend it but in the end is
uneven. Mossberger is very successful in the first task but less
so in the last two.

The introduction of the enterprise zone concept and its
consideration by officials at the federal and state level are
systematically examined. In-depth case analyses were con-
ducted in five states that adopted enterprise zone legislation:
Indiana, Massachusetts, Michigan, New York, and Virginia.
The studies are unique because they provide a detailed view
of state decision processes, focusing on what information was
diffused to the state and how it affected decision making.

The analysis describes in detail the process by which
enterprise zones diffused to the states. The literature has
emphasized either vertical channels, from the national level
to the states, or horizontal diffusion among states. Instead,
Mossberger argues convincingly that enterprise zones were
spread by a polydiffusion through both horizontal and vertical
channels. One important contribution of her account is the
concept of a policy label. Mossberger provides evidence that
in many states the presentation of enterprise zones revolved
around a constellation of words, such as "enterprise, innova-
tion, technology, independent, small business, venture, and
risk," not around an economic process. Studies of public
policy based in the welfare economics tradition see policies as
tools, as specific methods to correct market or government
failure. Although Mossberger does not explicitly make the
distinction between tools and labels, she adds an important
new term to the policy diffusion vocabulary.

Mossberger examines the intensity of analysis and the
decision model followed in the policy adoption process. She
first evaluates whether policy diffusion represented informed
decision making, that is, whether diffused information was
evaluated in light of the state's own circumstances. Moss-
berger concedes that assessment may be based on political
debate, rational calculation, or policy experience, but her
application focuses primarily on analytical assessment rather
than political assessment.

The decision-making model that characterized the diffu-
sion process also is examined. By identifying what knowledge
was diffused and how it was used, the author seeks to
categorize the decision-making process in each state as the
rational comprehensive model, the bounded rationality
model, or the organized anarchy model. There are several
problems with this attempt. First, the rational comprehensive

model becomes a straw man. It will always be an empty
category because its assumptions cannot be met. The decision
processes in the five states examined here fell into the other
two categories. Bounded rationality, although less broad than
a rational comprehensive approach, relies on technical, log-
ical, or analytical criteria for choice. In contrast, organized
anarchy is based more in temporal, political, and opportunis-
tic choice criteria; decisions are shaped by the timing of
events rather than the willful choice of rational models and
respond to political opportunities as well as problems.

One disappointment is that the relationship between in-
formed decision making and the decision models is not more
fully developed, either conceptually or empirically. The use
of diffused policy ideas by state officials might then be defined
by two dimensions that underlie the decision models and the
concept of informed decision making. The first is the inten-
sity of analysis given to alternatives, and the second is
whether analytical criteria are program based or political and
opportunistic. It might be informative to explore the cross-
classifications of these two dimensions and their implications
for understanding the patterns of debate over enterprise
zones in these five states. By not doing so, the author missed
opportunities to integrate more fully the idea of policy labels
into the larger theoretical framework and to strengthen the
link between this work and the comparative state politics
literature.

From the case analysis Mossberger seeks to identify factors
that account for the employment of the different decision-
making models within these states. Again, separating out the
two dimension of decision making, rather than relying on the
three classifications, might have provided a stronger theoret-
ical framework for prediction. The patterns of decision
making are expected to be linked to legislative staff, profes-
sionalization, partisan competition, liberalism, and divided
government. All the usual suspects are included, but what is
missing is strong theoretical arguments that link them to a
particular decision model.

Although the limitations of the conceptual framework
developed here are not unimportant, they must be evaluated
in light of the scope of the effort and the specific contribu-
tions this work makes to the literature on policy diffusion.
The comparative case studies provide a unique and detailed
view of state decision making and the policy diffusion process
that will generate much discussion and debate. This work
needs to be read by any serious student of state and local
policy.

**Founding the Criminal Law: Punishment and Political
Thought in the Origins of America.** By Ronald J. Pestritto.
Dekalb: Northern Illinois University Press, 2000. 191p.
$36.00.

Amy Bunger, *Florida State University*

Political theory reminds us that punishment is a fundamen-
tally political action, an exercise of political power. This book
is about penal reform and the philosophy of punishment as
both were debated in postrevolutionary America. Pestritto
combs through original writings of the founders and state
constitutions in an effort to elucidate leading philosophies
about the purpose of the criminal law and punishment. At a
macro level, the book provides a window into how the
American system, in Pestritto's venues of Pennsylvania, New
York, and Virginia, mediates between the tensions of the
preservation of individual liberty and maintenance of public
order. The book attempts to bridge the historical gap from
our founding to current issues in sentencing, such as the

three-strikes rule, determinate or mandatory sentencing laws, and state experimentation in marrying sentences to available prison space, or the cost of incapacitation. Pestritto's greatest contribution is to mine new material in these historical conversations on punishment, which allows them to be heard in our contemporary debates.

This work underscores the value of historical inquiry by demonstrating that ideas transcend historical circumstance. As Pestritto correctly notes, political theory accommodates the simultaneous pursuit of both crime prevention and just desserts, but this debate was, and remains, polarized in the philosophy of punishment, among both criminal justice practitioners and scholars. The *Aims of the Criminal Law* (Henry Hart, 1958)—whether for deterrence, retribution, the eradication of disparity, or preservation of equality—is a debate as pertinent and contested today as it was for our forebears. Law as a tool of social control has not changed, but neither has the division over what that tool is to carve. Also as in contemporary times, the founders debated the degree to which laws should be mirrors of evolving morality, whether laws should seek to instill morality, or whether criminal law should be divorced from morality. If Pestritto has an obvious goal, it is to convince the reader that punishment is a "fundamentally moral issue" (p. 152) and that society should embrace the notion of a "moral good in politics" (p. 153).

Of additional interest is the degree to which the political machinations of two centuries ago are mirrored in the contemporary politics of law and order. As early as 1787, Benjamin Rush led an assembly of what would now be termed "interest group lobbying" in pursuit of changes in Philadelphia's penal statutes and jail conditions.

The major contribution of this volume is that it allows us to hear this historical conversation. The work offers several possibilities for future policy exploration and application. First, Pestritto provides rich information and depth about policy formation during the founding period. More details about the ebb and flow of American political thought compared to changes in criminological thought and tides of domestic social policy would increase the practical dimension of the work. Further explorations should include the degree to which crime policy and punishment, a fundamentally political exercise, track changes in the larger political context, in particular whether penal policy is congruent with broad sociopolitical thought. There are two main schools of thought, the classic elite-driven (Walter Lippman, *Public Opinion*, 1922) or "top-down" model (Thomas Dye, *Policy-Making from the Top Down*, 2001) versus law as a reflection of public will, the latter resulting in reasonably high rates of congruence between public opinion and public policy (Benjamin Page and Robert Shapiro, *The Rational Public*, 1992). Pestritto is of the latter view, that credence should be afforded "reasoned public opinion, not fleeting public passion" (p. 154).

This work could more extensively explore the importance of focusing events, such as the brutal killing of Polly Klaas in California and its effect on the three-strikes law, in the passage of criminal law. Pestritto's discussion of the importance of the competition for ideas should be expanded—why some ideas emerge victorious—and that it is not sufficient simply to convince others of the merit of an idea. Penal policy is highly symbolic and politically charged. Pestritto acknowledges this in a discussion of the California three-strikes law, which allows for disproportionate punishment because the emphasis is on number of offenses rather than their severity. Typically, penal reform does not occur on the basis of policy evidence or from broad sweeping philosophical changes but, rather, through the constant presence of warring factions

debating the purpose of sanction, each looking for short-run opportunities to enact sentencing policies.

Policy diffusion across states is also a potential way to frame these findings in future work. In the international arena there is policy diffusion among common law countries in crime policy, particularly "zero-tolerance" models; domestically, Megan's law (community notification statutes) exemplifies the transference of policy among states. In the era about which Pestritto writes, when there was little mass communication and certainly not instant dissemination of information, it is quite remarkable to see how states influenced the course and shape of punishment in other states. Pennsylvania was a quintessential "laboratory of democracy" and founded the penitentiary, premised upon Cesare Beccaria's commitment to extricating morality from criminal law.

Pestritto tells a tale about the endurance of core American values—Seymour Martin Lipset's classic *American Exceptionalism* (1996)—populism, egalitarianism, individualism, liberty, and laissez-faire persist. Values also form a context in which policy is created and philosophy debated, and against which ideas may not survive (John Kingdon, *Agendas, Alternatives and Public Policies*, 1995). The obvious missing reference here is to Hebert Packer's classic, *The Limits of Criminal Sanction* (1968), which presents a continuum between due process and protection of individual liberty above all, and crime control, underscoring communal safety and the preservation of public order and good. Our debates about punitive sanction, individual liberty, and crime control, are a dynamic tension in a constant state of recalibration, in a setting of American public governance that wrestles with freedom from government versus what government should provide its citizens. Pestritto correctly concludes that criminal justice would be better served by viewing divergent philosophical perspectives not as mutually exclusive but simply as in healthy tension with each other. This allows the pursuit of often conflicting goals, both utilitarian and reformative, to co-exist and forces the prioritization of each relative to circumstance and time.

Representing Women: Sex, Gender, and Legislative Behavior in Arizona and California. By Beth Reingold. Chapel Hill: University of North Carolina Press, 2000. 338p. $55.00 cloth, $19.95 paper.

Marian Lief Palley, *University of Delaware*

Beth Reingold questions some of the conventional wisdom regarding women representing women and their interests. She does this by comparing the legislative behavior of women in the Arizona and California state legislatures in 1990, and she provides an understanding of the consequences and implications of electing women to public office. Reingold is interested in what these trends mean for the political representation of women in the United States, and it is this concern that focuses the volume.

Reingold begins with a discussion of some of the conventional wisdom. Research has shown, for example, that electing women implies the election of more liberal and disproportionately more Democratic officeholders. Moreover, some research indicates that women in public office ask different questions from men and thus can be effective in altering the policy agenda of the body in which they serve. In other words, even if they are not in leadership positions in legislatures, they look at issues from the vantage point of a woman's experience. In addition, when "women's issues" are discussed and then come to a vote, women legislators act as cue givers to male colleagues.

: Another body of research shows that women officehold-
ers tend to have distinctive ways of doing politics that
reflect female socialization. For example, they are assumed
to be more approachable and more responsive to constit-
uent demands and needs. Also, they are more likely to be
cooperative and are more inclusive in their approach to
politics. ·

Reingold finds that the assumption regarding women as
more liberal and more likely to support women's issues is
questionable. In fact, her research shows that the differences
among women officeholders are often more apparent than
the differences between them and men. There are liberal and
conservative women in public office, and some men are more
liberal than their female counterparts. Also, there are women
as well as men striving to improve conditions for women,
children, and families and to expand opportunities in educa-
tion, health, and welfare.

Similarly, Reingold finds that there are male legislators
who exhibit what are assumed to be female ways of doing
politics. That is, they are very concerned with constituent
relations and take a very cooperative and inclusive approach
to legislative politics. In fact, Reingold suggests that "the sex
differences uncovered are differences in degree, not kind."
There are men who are representative of women and women
who are not.

In the Arizona and California state legislatures, ideology
and partisanship are often greater dividers than gender when
political behavior and voting are concerned. The author
shows that there are not necessarily significant differences in
how men and women represent their constituents. In addi-
tion, in both cases examined, she reveals that men and
women have an equal opportunity to represent women, which
is somewhat at variance with scholars who argue that women
are better able to represent the interests of women. Although
Reingold does not find a relationship between gender and
adequate representation of women, she makes a strong case
for the importance of electing women. The symbolic value or
effect of women in public office is significant, she maintains,
and is one of the many avenues for challenging power
hierarchies.

**The Clinton Scandal and the Future of American Govern-
ment.** Edited by Mark J. Rozell and Clyde Wilcox. Wash-
ington, DC: Georgetown University Press, 2000. 288p.
$55.00 cloth, $18.95 paper.

Mark Silverstein, *Boston University*

Ted Lowi once wrote that scandals are a proper subject for
the political scientist because the observer is catching the
country in the act of being itself (Theodore J. Lowi, "Fore-
word," in Andrei S. Markovits and Mark Silverstein, eds., *The
Politics of Scandal: Power and Process in Liberal Democracies*,
1988, p. xii). Lowi's remark seems particularly apt when
applied to the study of politics in the United States. At the
heart of the American political tradition lies an acute ambiv-
alence concerning the nature and uses of political power. A
profound philosophical distrust of government is measured
continually against a pragmatic appreciation for the necessity
of state power. Americans venerate a constitutional arrange-
ment marked by the separation of powers and checks and
balances, but much of the political history of the United
States can be understood as an ongoing effort to escape the
constitutional limitations on the effective use of political
power. The impetus behind the growth of the party system,
for example, was in no small measure an attempt to link the

branches to provide for more efficient governing, despite the
framers' obvious desire to keep the branches separate.

Politics as usual in the United States is essentially politics
in which cooperation and compromise tend to obscure the
structural constraints of the Madisonian system. And, not
surprisingly, the political science study of American politics is
most typically the study of politics as usual. The academic
value of scandal is precisely that it is not the study of
customary politics and, as such, often presents the student
with a fresh and more defined picture of the political and
social system. Under stress the system is revealed. From this
perspective an obvious measure of any serious academic discus-
sion of political scandal is not what it reveals about a particular
scandal but the extent to which the scholar uses it as a means to
improve our understanding of the political system.

The essays in *The Clinton Scandal* certainly meet this
measure of academic credibility. The book is divided into two
sections; the first part examines the institutions and legal
issues connected to the scandal, and the second chronicles
the political influences at work in the impeachment process.
Subjects range from an in-depth study of the independent
counsel statute by Louis Fisher, to Stephen Wayne's discus-
sion of Clinton's personality, to John Maltese's analysis of the
changing nature of the media and the effect of these changes
on the Clinton presidency.

The contributions are of a uniformly high standard and
surpass in several instances the basic workmanlike effort one
might expect in a collection of essays. The book offers
genuinely intriguing observations about modern American
politics. David Yalof and Joel Grossman, for example, con-
tend that modern presidents in the years following Watergate
are subject to such unique legal burdens and political limita-
tions on the assertion of legal rights as to render the occupant
of the White House, at least when compared to the typical
citizen, "below the law." Law professor Michael Gerhardt
uses his informative review of the Clinton impeachment
process to support his argument that the independent coun-
sel, despite an almost unlimited budget and armed with all
the tools of a prosecutor but freed from the constraints of
public accountability, is nonetheless no match for the White
House in the inevitable public relations battle that is trig-
gered by any investigation of presidential activities. In dis-
cussing the implications of the Clinton scandal on the presi-
dency, Robert Spitzer challenges the conventional wisdom
that the events of 1998 seriously weakened the office. He
makes a persuasive case that little was altered in the rela-
tionship between executive and legislative branches precisely
because the impeachment provided few if any benefits to
Congress. In Spitzer's view, rather than invite a partisan
House in the future to race to impeachment, the experience
counsels legislative caution.

Against the backdrop of the 1998 events, the essays tell us
a good deal about the current state of American politics. In
itself this is no small accomplishment. Unlike Watergate or
the Iran-Contra affair, at the heart of which were abuses of
power and process, the Clinton debacle is a harder academic
study. Bill Clinton's sins were of a particularly private nature;
the lies he told were attempts not to conceal constitutional
transgressions but to hide personal indiscretions. It is easy to
moralize and sensationalize when speaking of the scandal. It
is far more difficult to step back and, with fairness and
detachment, chronicle what it reveals about American poli-
tics and political institutions. The contributions in this vol-
ume succeed admirably in this undertaking.

The Presidency and the Politics of Racial Inequality: Nation-Keeping from 1831 to 1965. By Russell L. Riley. New York: Columbia University Press, 1999. 373p. $49.50 cloth, $22.50 paper.

Nina M. Moore, *Colgate University*

Russell L. Riley offers an insightful account of how American presidents have grappled with race. His main concern is the causal forces that shape the institutional role of the presidency in American politics. The discussion centers specifically upon the determinants of presidential policy that affects the advancement of African Americans toward first-class citizenship. Riley asks what operative dictates and constraints shape presidential behavior vis-à-vis racial inequality politics. Using an historical approach that encompasses multiple presidencies, he presents the most comprehensive examination of race-related presidential politics to date. As the author notes, this broad lens helps him overcome the handicap of idiosyncrasy most presidential scholars face and develop an explanatory framework with broader than usual applicability.

The descriptive analysis is rich in detail but offers only a few surprises. Andrew Jackson and his successors thwart the abolitionist cause. Abraham Lincoln departs from that pathway but is essentially forced into his infamous glory. The presidents who follow resume the course of least action until the Franklin D. Roosevelt era, during which advances are notable but largely symbolic. Harry S. Truman's progressive gestures are more substantive but still of little practical meaning, due to southerners' stronghold in Congress. Dwight D. Eisenhower, like Lincoln, helps the cause of integration only after much hesitation and only in small ways. John F. Kennedy and Lyndon B. Johnson enter office with only "marginally more enthusiasm than their Republican predecessors" (p. 201) but eventually facilitate a second Reconstruction.

Despite the familiar ring of Riley's analytic conclusions, portions of his research base are nonetheless revealing. The study is packed with in-depth information about the dynamic interplay among key historical events, political personalities, and institutions. Careful detail is provided on how those presidents most noted for their racial liberalism were at times themselves actively aligned against the forces of racial change. They were more than just initially hesitant to take the lead; Riley supplies evidence that the basic posture assumed by many alleged White House progressives was decidedly conservative.

The more intriguing aspect of this work is Riley's interpretation of the empirical findings. The thesis offered is appealing but suffers from a number of weaknesses, the most important being a disconnection between what Riley actually finds and what he concludes. Let me begin by summarizing the theoretical argument. According to Riley, the "institutional logic" of the presidency is the fundamental driver of presidential behavior. This logic envisages the president as preserver of domestic tranquility and protector of the inherited political and social order (p. 10). Riley draws upon the *Federalist Papers*, stating that Alexander Hamilton lays out the logic of the presidency in the following passage: "[Energy in the executive] is essential to the protection of the community against foreign attacks . . . to the security of liberty against the enterprises and assaults of ambition, of faction, and of anarchy" (p. 13). In attempting to demonstrate the validity of this nation-keeping thesis in the case of racial inequality politics, Riley highlights various instances in which presidential statements and actions clearly reflect concerns about order and national security. He also gives examples of

major concessions and even aid proffered to forces that oppose civil rights. In many respects, the greatest appeal of Riley's work is the integration of an extensive, variegated history of presidential civil rights politics into a single, cohesive explanatory framework.

The nation-maintaining argument has shortcomings. First, on its face it is somewhat simplistic. To say that the president's primary constitutional role is to preserve the nation and that this was the preeminent concern of the administrations Riley analyzes is, in many respects, a given. The same can be said of Congress and the Supreme Court. They are all required to act in the best interest of the nation. Doing so means, at a minimum, preserving the nation. Second, even if one were to set aside this weaker claim and accept the more robust claim that nation-keeping really means the president's role is to mitigate change, one still finds flaws. In the *Federalist Papers* are sufficient grounds for an understanding of presidential purpose that is decidedly different from Riley's change avoidance thesis. In No. 70, Hamilton makes clear that the primary concern facing the framers in their efforts to structure a viable executive arm was to endow it with sufficient energy, by which Hamilton meant the wherewithal to respond readily and decisively to problems and issues as they arise.

This concern for responsiveness is reflected in Hamilton's argument on behalf of a single executive in No. 70: "Decision, activity, secrecy, and dispatch will generally characterize the proceedings of one man in a much more eminent degree than the proceedings of any greater number" (Alexander Hamilton, James Madison, and John Jay, *The Federalist Papers*, 1961, p. 424). The same concern is evident in the following passage from No. 70: "Those qualities in the executive which are the most necessary ingredients in its composition [are] vigor and expedition" (p. 427). Given this, as well as the list of constitutional responsibilities and powers entrusted to the president, one could easily conclude that the constitutional purpose of the presidency is to navigate rather than mitigate political change. Indeed, Riley alludes to this alternative thesis when he states that the presidential institution is in some ways analogous to a ship's captain, whose responsibility it is to "steer the vessel out of harm's way" (p. 166). The key word here is "steer."

Third, although Riley sets out to challenge the "monumental presidency" concept that dominates much of the literature, he falls prey to the same line of thinking. He attributes to presidents more control over policy agenda than perhaps is warranted. Specifically, he argues that, in the field of civil rights, presidents have been motivated toward a specific policy end: preserving the racial status quo. Implicit in this argument is the idea that presidents possess an independent will. It seems more plausible to conclude, however, that presidents are agents of majority will.

Riley's analysis supplies quite a bit of support for this alternative view. In the ship's captain analogy, the president "through a host of shipboard contrivances (physical and organizational) is made to feel the condition of ship and sea unlike anyone else on board" (p. 165). In various portions of the discussion it is noted that, when presidents acted either for or against racial progress, they did so in tandem with shifts in the national political environment and in ways that reflected the mood of the ruling majority. "The nation was electrified by" Lincoln's Emancipation Proclamation (p. 110); during post-Reconstruction, "the nation had lost its zeal for change (p. 121) and presidents became increasingly sensitized to demands for racial equality "because the politics they confronted in Washington and in the nation at large . . . required as much" (p. 136). Yet, despite evidence

that the presidency in many ways lacks a policy will that is independent of the electoral context, Riley suggests that, regarding racial inequality, American presidents have operated under a standing order of sorts. He concludes: "Each president is expected not just to preserve the United States Constitution (large 'C'), but America's constitution (small 'c')" (p. 19).

None of these shortcomings diminish the significance of Riley's work. Overall, the book is thought provoking and worthy of a careful reading by those interested in the study of institutions and of racial politics and policy. The contribution is threefold. The book broadens our conceptualization of presidential power and its use; challenges us to think more critically about the relevance of institutional roles and logic for understanding racial outcomes; and lays indispensable groundwork for further exploration of the intersection of race and institutions.

Does Business Learn?: Tax Breaks, Uncertainty, and Political Strategies. By Sandra L. Suarez. Ann Arbor: University of Michigan Press, 2000. 192p. $47.50.

Mark A. Smith, *University of Washington*

Sandra Suarez sets out to understand why corporations choose certain political strategies rather than others. She argues that firms want to maximize profits and recognize that politics affects their ability to do so, but they cannot easily translate their interests into concrete political strategies. Because plausible reasons can be constructed for many different positions and actions, firms live in a constant state of uncertainty in determining their behavior. Suarez seeks to explain how they overcome that uncertainty, reaching decisions to act and sometimes to change their behavior. She defines three possible strategies: Do nothing, and rely upon structural power to block unfavorable policy proposals; join forces and lobby as a collective; or engage in fragmented lobbying, with each firm advancing its own distinctive claims.

Suarez examines a common set of firms across time and several related policy battles that provide the impetus for their decision making. The empirical centerpiece of the book is a tax code provision that changed in scope and details over the twenty years under study but amounted to major tax reductions for U.S. companies with plants in Puerto Rico. Known as the possessions tax credit, it benefited capital-intensive enterprises the most. The largest beneficiaries were pharmaceutical manufacturers and some electronics companies, for whom the provision "has been more important in reducing their federal tax bills than have foreign tax credits, tax deferrals, capital gains tax breaks, or investment-tax credits" (p. 3). Policymakers in Congress and the executive branch considered modifying or eliminating the credit in 1976, 1982, 1986, and 1993; the efforts in 1982 and 1993 led to a curtailment and partial repeal of the provision.

The central argument is that the political strategies of firms evolve over time through a process of learning. In a sense, they are always fighting the last war, as their understanding of the actions necessary to advance their interests is based upon how they behaved the last time the issue arose as well as the outcome. They often overestimate the efficacy of their own actions and underestimate the importance of the political context. The breadth of their learning usually hinges upon whether a desirable outcome resulted from the last political struggle.

Suarez's perspective is developed and applied to the corporations who profited from the possessions tax credit. When the exemptions emerged intact after threatening proposals failed, firms used their same strategy the next time—whether inaction, collective lobbying, or fragmented lobbying—so long as the political context remained constant. In following a "don't fix it if it ain't broke" approach, the corporations in this study were slow to adjust to new environments and did not fully appreciate how much their fortunes depended upon forces beyond their control. When they did perceive a new political context, they reflexively turned to what Suarez's calls the "default" strategy of divided lobbying. Yet, their behavior evolved more quickly in the wake of political defeats, which they attributed to the strategies they undertook. A strategy associated with an unfavorable outcome was not repeated during the next battle. Instead, firms either employed the default strategy or, if they detected changes in the political environment, engaged in a systematic search for a new strategy that would best promote their goals. Thus, political defeat encouraged these corporations to assess their situations, as would a fully informed decision maker.

The book challenges some common presuppositions about how firms behave politically. Scholars often assume that firms can discern their strategies easily from their interests, but Suarez casts doubt on that belief. Instead, the evolution of strategies takes on a life of its own. It is commonly thought that large corporations such as pharmaceutical manufacturers monitor politics extensively and lobby constantly, but the companies studied were often tardy in reacting to threats and sometimes took no action. The amount of political inaction among corporations with millions of dollars on the line may be the most remarkable finding in the book. Related points were made by Raymond Bauer, Ithiel de Sola Pool, and Lewis Dexter in *American Business and Public Policy* (1963), but Suarez shows them to be still relevant despite the widespread and seemingly permanent mobilization of business during the 1970s.

The contention that firms failed to act in 1976 and 1982 because they could rely upon their structural power seems strained. The structural power theory advanced by Charles Lindblom and others begins with the observation that a capital strike would sink the economy, causing voters to toss the incumbents out of power. Because no policymakers with jurisdiction over the possessions tax credit ever had to face voters in Puerto Rico, the implicit or explicit threats of firms to disinvest from the island lacked the electoral punch upon which the notion of structural power relies. In addition, Suarez could have considered the ways in which the companies' experiences with other political issues affected their beliefs and actions on the possessions tax credit. It seems unlikely that decision making would be completely atomized, with issues being resolved independently of one another. Suarez does call for additional research on this question (pp. 138–9), but her conclusions might have been different had she considered linkages across issues.

Despite these limitations, *Does Business Learn?* makes valuable contributions to our understanding of how firms behave in politics. Presumably, its perspective could apply to other political actors, such as unions or public interest groups. Suarez succeeds in her main goal of showing that firms' political strategies depend upon the lessons drawn from their previous experiences.

Campaign Warriors: Political Consultants in Elections. Edited by James A. Thurber and Candice J. Nelson. Washington, DC: Brookings. 216p. $42.95 cloth, $17.95 paper.

Todd Donovan, *Western Washington University*

The study of political campaigns has an awkward place in political science. At one level, we have grown to accept that "campaigns matter." A growing literature now provides many different tests of this proposition. At another level, a cliché sometimes mouthed by campaign professionals and journalists is that 90% of what campaigns do does not matter—it is the remaining 10% that is critical. The relative accuracy of these proportions aside, this cliché raises an important question: What is the critical part of modern campaigns that "matters"?

The editors of this volume begin with the proposition that one answer is professional political consultants—those people paid by candidates to make strategic decisions about communications, media purchases, allocation of campaign staff, and myriad details associated with modern campaigns. Depending upon the chapter at issue, the working definition of "consultant" in this volume may include other individuals paid to conduct more or less specialized campaign tasks. A common theme of the book is that their role is important, if not critical, to understanding candidate success and election outcomes. By focusing on the role of consultants, the contributors take our understanding of "campaign effects" beyond well-established models of how spending affects election outcomes. Here, we see arguments for the importance of who spends the money.

The contributors provide an informative look at what consultants do, and their tests of the effects that consultants have on electoral politics raise some important questions. As rich as this volume is, it also reflects an enduring problem with the study of campaign professionals. Research has been dominated by insider accounts of campaigns and by descriptive studies of what key actors actually do when they practice their craft. Thus, we have built a large history of the evolution of campaigning, particularly in the United States (i.e., books by Stanley Kelly, Dan Nimmo, David Rosenbloom, and Larry Sabato). Much less, however, has been produced in terms of systematic theories about campaigns and campaign resources, and there is not much in the way of testable hypotheses.

This is due, in part, to the fact that campaign techniques are a bit of a moving target. Professionals are paid to apply new techniques in each election cycle, and several chapters in this volume include a substantial amount of description about what these actors do. Many chapters improve upon the descriptive literature by using systematic survey methods to assess what consultants do. For example, Thurber, Nelson, and David Duilio (chap. 2) report on a survey of 200 professionals engaged in various aspects of modern campaigns. They find that these professionals dislike the media, dislike campaign finance reform, and tend to believe "scare tactics" and "suppression of voter turnout" are not unethical. The authors' interpretation of these results is interesting but quite contestable, such as their optimism that "only one-half" of consultants said that unethical practices occurred "sometimes" or "very often" (p. 27). If most campaign professionals do not consider much to be unethical, however, a reader might ask whether the glass is half empty or half full.

Paul Herrnson's chapter also makes use of surveys to describe the role of consultants in U.S. House elections (chap. 5). Readers of his 1998 book on congressional elections may have seen some of these data before, but as used in this volume they put statements about consultant activity in better perspective. Herrnson illustrates that it is difficult to distinguish between the paid consultant and the congressional aides employed by 81% of incumbents to manage their campaign. He finds that these paid staff perform many of the activities that other scholars might attribute to consultants hired from outside.

Stephen Medvic (chap. 6) also notes that survey respondents (candidates) might not understand what is meant by the term "consultant" (p. 95). Using data from *Campaigns & Elections* magazine, Medvic reports that 64% of House candidates employed professional consultants in 1992, but Herrnson's method leads him to put the figure at 19%. Despite these differences in establishing how many candidates use "consultants," each author presents rich data on the types of activities (e.g., polling, GOTV, FEC reporting) in which a wide range of professionals engage. The difficulty, it seems, is establishing when someone is a staffer, a pollster, or a consultant.

Description of this sort is the most valuable component of the book. Additional chapters offer an overview of consulting as a business (Dennis Johnson, chap. 3) and from the perspective of a former Democratic Congressional Campaign Committee staffer who worked as a professional consultant (Martin Hamburger, chap. 4). Shaun Bowler and David Farrell (chap. 9) report the results of a survey of consultants outside the United States to give a portrait of the emerging internationalization of campaigns. Robin Kolodny (chap. 7) investigates how political parties use consultants. She argues that modern campaign techniques have exceeded the institutional capacity of parties, which now play a role in matching candidates with consultants. Although it is well established that national party committees behave this way, Kolodny uses survey data to illustrate that state parties engage in these collaborative relations with consultants.

David Magleby and Kelly Patterson (chap. 8) draw from detailed interviews with dozens of consultants and a survey of a larger sample to present a rich portrait of professionalization of ballot initiative campaigns. Although these occasionally retain a populist or grassroots image, the authors find that well-funded interest groups have substantial advantages. Failing to echo the editors' more sanguine impression of consultants, Magleby and Patterson suggest that consultants have assumed considerable control of the initiative process, and democracy has suffered as a result (p. 150).

Many contributors speculate about the overall effects of consultants (e.g., on election outcomes, party strength, voter attitudes), but there are few explicit attempts to test for the effects of their actions. Herrnson (p. 67) claims to use his data "to demonstrate that campaign professionalism has a positive effect" on campaigns, but no systematic tests are reported. Medvic, in contrast, uses OLS models to estimate the effect of professionalism on House elections. He finds that hiring more professionals had a significant, positive influence on challengers' vote margins in 1990 and 1992, but the models are misspecified. Incumbent and challenger spending, for example, is specified as independent of each other.

It will be interesting to see whether statistical models will detect any effects of consultants in future elections. Various contributors note that these professionals are adept at rapidly applying new technologies, that they "learn" (p. 92) what works, and that professional campaign staff is being hired increasingly from established, institutionalized firms. Over time, then, the use of professionals—and their potential

effect—may become a constant in most races. If anything, as dispersion of consultant use increases and as consultants become more professionalized, the marginal influence of their activity, as estimated in statistical models, should decline. This volume, although it provides several rich portraits of the consulting profession, would benefit from a concluding chapter that considers such issues and suggests directions for future research.

The New England Town Meeting: Democracy in Action. By Joseph F. Zimmerman. Westport, CT: Praeger, 1999. 248p. $59.95.

Frank Bryan, *University of Vermont*

With the exception of Jane Mansbridge's important and groundbreaking analysis of "Shelby," Vermont (*Beyond Adversary Democracy*, 1980), published scientific investigation of face-to-face democracy in the New England town meeting is almost nonexistent. Thus, Zimmerman's volume is not part of a genealogy of scholarship on what I call "real" democracy, to distinguish it from the direct democracy of referenda and initiatives with which it is often confused. For many years his interest has been what he terms (accurately) "law-making by assembled citizens." Given the general misuse of the term town meeting by politicians (which began with Carter and was perfected by Clinton), intent on cloaking a variety of self-serving public relations ploys in the robes of "pure" democracy, Zimmerman provides at the very least a much needed reality check for political scientists. In fact, in the popular American lexicon (and even in the understandings of many political scientists) town meeting has taken on a totally new meaning, as exemplified in Andrew Fergurson's essay ("Ye Olde Town Meeting Gimmick," *Time*, March 2, 1998).

Zimmerman's book is a mandatory first read for anyone interested in the study of America's oldest political institution, the New England town meeting. For political scientists willing to journey into the untouched terrain of real democracy, this book is the demarcation point. Its usefulness is found in the central six chapters, which describe the legal basis, structural parameters, and procedural variants of town meeting in each of the New England states. The roles of town officers (especially the moderator), citizens groups, and initiatives and other attendant processes to town meeting democracy also are discussed. No other source brings together this kind of essential material for a novice's introduction to the subject, and by novice I mean the huge proportion of political scientists.

I count several problems in the book. The first, atrocious editing, is more irritating than important. For instance, sentences seem to hopscotch through the book, landing here and there from earlier chapters almost in their entirety. The sequence and substance of the discussion is flat and predictable, which lends a manual-like tone to the prose. The second problem is more important. When attention switches from description of structure to analysis of process, the book's method draws into question the accuracy of the data and its comparative usefulness. The primary source is mailed questionnaires to town officers in each state, which suffers from all the familiar drawbacks of such techniques. This is especially true for the tables on the all-important matter of attendance rates. A primary problem is that, with exceedingly rare exceptions (such as Athens, Vermont), attendance is not formally recorded. The only way to know about attendance is to be there and count, although attendance varies throughout the meeting, so when it is counted is also critical. Zimmerman does not tell us whether there is uniformity in the

counting, when the counting occurred, or even whether a count was taken. Town clerks often equate attendance with the total number of votes cast when (and if) a paper ballot is used during the meeting, which often underestimates the count. Some clerks report attendance as the number who vote by day-long paper ballot (called the Australian ballot), which allows people to enter the town hall, vote, and then leave immediately without attending the meeting. This overestimates the counts. Zimmerman may have corrected for these problems, but that is not indicated in his book.

The third problem is that Zimmerman seems to let his enthusiasm for town meetings (which I share) becloud his judgment. Also he is not certain what the optimal defense should be. Often, wisely, he compares town meetings to other law-making institutions and asks: Where in the United States is political life more complete or fulfilled for the average citizen? This is when he is at his best. In fact, this argument could have been made with far more energy. If attendance at town meeting averages, for example, only 20% of the voters year in and year out, and if it takes three or four hours out of the day (or evening), which may cost the attenders a day's pay, is 20% not remarkably high compared to the national electorate, which can barely muster 50% turnout only once every four years for an act that seldom takes more than half an hour?

Zimmerman falls into the trap of defending town meetings not from the high ground of communitarianism but by charging into the cannons of liberalism. If attendance is low, it can be explained as de facto representation. If participation seems weak and uniformed, it is reinforced and enhanced by a committee system of advisory panels and citizen boards. Communal decision making is rescued by representation, and public talk is saved by legislative structures. Zimmerman seems unwilling to concede there are real problems associated with town meetings and ends up defending a perfection that does not exist. For instance, he dismisses Mansbridge's finding (and my own to some extent) that "town meeting attendees are not representative of the citizenry at large" with the notation that a similar charge would "apply equally to elected town councils" (p. 185). True enough. In fact, city councils are less representative of the people than town meetings. This is a very good point, but it is not *the* point. Mansbridge is correct in questioning the degree to which town meetings meet the test of a perfect match between citizenry and assembly, especially when it comes to the very lowest status groups in a town.

Zimmerman's defensiveness leads him to emphasize Mansbridge's criticisms of town meetings, and this deprives readers of his view on the broad range of insight she brings to bear, which in many respects is remarkably supportive of the town meeting. A puzzlement for me is that when political scientists refer to Mansbridge's work (and now and then my own) they nearly always go straight to the negatives. This is the "ah-ha!" of a mind-set primed for criticism. Perhaps that is understandable, given the superlatives in which town meeting defenders are all too willing to wallow. Besides, the town meeting is often considered a threat to liberalism, which is the principal paradigm today. Is the town meeting (to quote Robert Frost) "something we somehow haven't to deserve"? Could it be that the reason, à la Thomas Wolfe, we cannot go home again is because we are afraid to? The development of a truly communitarian alternative rests on the willingness of scholars such as Zimmerman to defend the town meeting on its own (communitarian) terms, face up to its real weaknesses, and see if these can be resolved in the context of the coming (and I hope decentralist) sociocultural paradigm.

All this aside, my instinct is to applaud Zimmerman for the

first comparative analysis of the structures and process of town meeting democracy in New England. His mailed questionnaires are limiting but do provide parameters that can guide future work. There is a reason for the lack of comparative studies on town meetings: The logistics of such an enterprise are daunting. I have problems with the direction of and reasons for Zimmerman's optimistic interpretations, but

I am inclined to forgive (and even applaud) his optimism. Town meetings (real democracy) remain a sacred longing in the American consciousness and continue to reside deep in the heartland of our dearest dreams. Exaggerating their beauty is easy. We can hope Zimmerman's courage and commitment will instill in the discipline the will to carry on his work.

COMPARATIVE POLITICS

Inventing Local Democracy: Grassroots Politics in Brazil. By Rebecca Neaera Abers. Boulder, CO, and London: Lynne Rienner, 2000. 269p. $59.95.

David Covin, *California State University, Sacramento*

In this case study of participatory democracy in Porto Alegre, Brazil, Rebecca Neaera Abers examines one aspect of the participatory democracy program implemented by the *Partido dos Trabalhadores* (PT), the Workers' Party, in Porto Alegre: the participatory budget process. In that process, people at the grassroots make policy decisions that the government is committed to implement. The PT took office in 1989, and Abers follows its participatory budget process from 1989 to 1997.

The PT government in Porto Alegre is an anomaly. It is leftist, progressive, and popular participation oriented in a country characterized by conservative, clientist politics. The PT has elected other administrations at the municipal and state levels, but many have been short lived. The dominant pattern at all levels in Brazil has been the election of conservative candidates who operate on the basis of clientist and patronage relationships and who restrict meaningful participation to corporate elites. Even when elected, PT administrations elsewhere have often reverted to elite-dominated patronage politics with little substantive popular participation.

Abers attempts to explain what accounts for the exceptionalism in Porto Alegre. Her quest, grounded in a wide range of political science literature, systematically examines events there. It is a case study deliberately tied to the literature most germane to the author's subject. She sets forth the major generalizations in the literature associated with her principal areas of interest and examines whether her findings support or contradict them.

The literature often offers several explanations for a single phenomenon. Theda Skopcol, for example ("Bringing the State Back In: Strategies of Analysis in Current Research," in Peter B. Evans, Deitrich Rueschemeyer, and Theda Skopcol, eds., *Bringing the State Back In*, 1985), argues that institutional constraints vary the capacities of the state to act autonomously, and Fred Block ("Beyond Relative Autonomy: State Managers as Historical Subjects," *New Political Science* [Fall 1981]: 33–49) suggests that economic relationships impose greater restrictions on government activity. Abers seeks to discover the extent to which each generalization applies to Porto Alegre.

The author is most concerned with participation, empowerment, and state-society relationships. She examines contradictory elements associated with participatory democracy— its tendency to reward the organized, those with resources, despite its emphasis on empowering the powerless. She interrogates political scientists who have addressed these

questions by applying their propositions to explicit conditions on the ground.

By defining her case narrowly Abers provides a multilayered, longitudinal examination of public policy. She concentrates most intently on two neighborhoods, and she develops a level of detail rarely available in a study assessing major propositions in the discipline. In the light of concrete circumstances she can ask whether many influential generalizations improve our understanding of events in Porto Alegre.

Abers draws on a broad cross-section of the literature to focus on a small range of behavior. Therefore, she brings great analytical power to bear on a single set of problems. The lines of inquiry that provide greatest illumination concern state-society relations, in particular those that emphasize the possibility of substantial state autonomy; social movements, especially work that suggests how the "locked out" become involved; and rational choice, particularly as it addresses the free-rider problem.

Abers does not perceive her study as objective and does not intend it to be. She is clearly a fan of participatory democracy. In Porto Alegre she finds circumstances under which she believes it works. She wants to understand why and on that basis spread the gospel. "I will attempt to broaden the understanding of the practical possibilities for implementing participatory democracy" (p. 4).

The author shares the Porto Alegre PT's value of inversion of priorities: empowering the poor and the excluded. Government policy should deliberately give priority to those persons who traditionally have been excluded from public power. This priority should be both in terms of physical representation and policy objectives. The goal is not equal representation or participation. It is participation and decision making heavily weighted in favor of the poor. Abers's advocacy does not diminish the quality of her scholarship. It simply identifies the perspective from which it was undertaken.

Abers asserts that participatory democracy in Porto Alegre works because the state can be autonomous. It can act independently to affect civil society. It can implement policies that result in the formation of strong, persistent institutions in civil society, institutions organized and sustained by the very poor. These findings are significant for students of politics, whatever their political dispositions.

Data were collected during three field trips to Brazil, the longest of which lasted eighteen months. In-depth interviews were conducted with 98 people involved in the participatory budget process. Short-answer surveys were administered to 622 participants in the budget process and to 65 members of regional budget forums. Abers attended 109 community meetings and examined a vast collection of documents. All her methods are clearly set forth in an appendix.

The study's strengths are also its weaknesses. A single city is examined, and an exceptional one, and data are drawn heavily from two neighborhoods. The participatory budget process is only one of many participatory democracy initia-

tives pursued by the municipal administration. We have no comparable assessment of these other projects and no way of judging whether Abers's propositions apply equally to them. We do not have enough detailed data from many neighborhoods to know whether the reasons for variations among them have been accurately identified. Because of the exceptionalism of the PT administration, the author's generalizations about state autonomy can have only a very narrow field of application. Few governments in Brazil or elsewhere exhibit the characteristics of this administration.

Also, Abers recognizes the role of serendipity in the success of the administration, but she gives it little emphasis. The national constitution, newly adopted at the time the PT came to power in Porto Alegre, mandated unprecedented federal payments to municipal governments and gave those governments great latitude in spending. The PT gave poor neighborhood organizations direct control over significant portions of that money. The best will in the world—without the resources—might have produced quite different effects for this newly energized public. As Abers states, referring to a recent change in federal policy: "With a smaller 'pie' of state resources, it may become increasingly difficult to devolve power to the hitherto excluded" (p. 227).

There are other puzzling omissions. In part of a chapter (pp. 115–21) that reviews the literature on how to encourage participation by the excluded, no mention is made of Paulo Friere. The author's focus on neighborhood organizing and organizations leads her to minimize consideration of marginalized populations organized on other bases across neighborhoods—race, gender, class, and religion. Abers pays some attention to the role of women in the process, as individuals and within neighborhood organizations, but she does not examine women's organizations. She touches on religious organizations primarily in providing historical background for community organizing in the city. She completely omits organized racial populations. In Porto Alegre, which has a long history of racial militance among residents of African descent, this is a curious omission. We have no indication, for example, whether race-based organizations played any role in neighborhood and cross-neighborhood organizing and networking, or the extent to which African-descent populations were incorporated into the empowering process.

Civil society is central to the discussion, but Abers provides no discourse on it. She does not define it. Hence, a key construct remains entirely amorphous.

Nevertheless, as Abers notes (quoting Mitchell McCorcle, "Stories in Context: Characteristics of Useful Case Studies in Planning and Evaluation," *Evaluation and Program Planning* 7 [1984]: 205–8), "detail is, to the case researcher, what a larger sample size is to the experimental or survey researcher—each detail adds another data point, thus increasing the potential precision of the conclusions" (p. 30). This is a case study whose potential precision is considerable. It is highly informative for those interested in social movements, state-society relations, participatory democracy, and exceptionalism as a means of testing the limits of general propositions. It is good, qualitative research. It makes a significant contribution to the discipline.

Saddam's Word: Political Discourse in Iraq. By Ofra Bengio. Oxford: Oxford University Press, 1998. 266p. $49.95.

Amatzia Baram, *Haifa University and Georgetown University*

Ofra Bengio's book is a scholarly and methodical endeavor to analyze the way in which Ba'thi Iraq (1968–present) has been

using (or misusing) the Arabic language, symbolism, history, and myths in order to legitimize its rule and policies. Bengio demonstrates how language has been twisted and manipulated in an attempt to terrorize political enemies, paralyze and enchant the vast majority of the Iraqis, induce them to perform or tolerate atrocities, and risk their lives in battle for leader and country. Sometimes, the regime is entrapped in its own rhetoric. The book shows how discourse, art, and symbols have been used to construct a cult of personality for Saddam Husayn, unmatched in the contemporary Middle East.

The author also provides historical depth through etymological study. The book follows the twists in usage of a large number of terms throughout the 30 years of Ba'th rule in Baghdad. These twists are not limited to pure academic interest: They are indicative of political changes. For example, the gradual decline of secular terminology and the commensurate growing recourse to Islamic rhetoric reflect the regime's realization that, partly due to its own disastrous policies, it needs to shore up dwindling mass support, and Islam seems to be the best way to do this.

The book demonstrates how even the most horrific disasters Saddam Husayn has brought upon Iraq are depicted as glorious victories, and the worst massacres as Islamic rights. In times of great peril, the leader and his regime use mystical notions to disguise the danger and provide false hope. It is, of course, anyone's guess as to what extent the whole Ba'thi propaganda machine, which is designed to confuse reality with its rhetorical representation, is successful in achieving its goals. (Bengio suggests in two places that it is, in two others that its influence is uncertain; pp. 77, 202, 206, 207.) Among other illuminating analyses, Bengio discusses Saddam Husayn's fascination with the military exploits of early Islam and his wish to "reenter" this chapter of history. This desire to relive the past has cost Iraq dearly. Still, much of the propaganda endeavors to convince the Iraqis of the absolute irreplaceability of Husayn, without whom Iraq would split and sink.

The book is arranged around five foci: the Ba'th party and its revolution; the political system and Saddam Husayn's leadership; nation building and communal cleavages; the treatment of enemies; and the manipulation of history.

In terms of content analysis and etymological study, this book is scholarship at its best. Bengio is sensitive to nuance; to the historical roots of modern Arabic, which provides an important dimension that complements narrative history; and to Iraqi politics and society. Most parts of the book also analyze correctly the interdependence between rhetoric and the real world of political action. This, however, is also somewhat of a weakness. On a number of occasions, Bengio fails to recognize the eclectic and equivocal nature of regime and leader. Because the regime has to appeal to a number of constituencies, a few apparently antagonistic ideological-cultural and political elements have coexisted in the official credo for many years. Also, by focusing in the relevant chapters (2, 5, 6, 12, 13) almost exclusively on the Islamization of rhetoric and symbolism (in itself a very important aspect), Bengio leaves out other crucial components of the evolving national identity, vigorously promoted by regime and leader.

Under Saddam Husayn, national identity (and much of Iraq's domestic and foreign policies) has rested essentially on four legs: the reinterpretation of Arabism, of ancient Mesopotamian history, of Islam, and of tribalism. Bengio deals extensively with the Islamic component but only partially with the Arab one, ignoring the transition from an old to a new pan-Arabism, and she glosses over the remaining two. Until

the early 1970s the prevailing view of what should come first, Iraqi identity and interests or Arab ones, was that the pan-Arabism should always prevail. National borders between Arab states were seen as illegitimate, and all-Arab unification was expected to create a melting pot into which all Arab peoples would disappear.

For domestic and foreign policy reasons in Iraq, this amalgamative view proved risky once the party was in power. Thus, within a few years, Saddam Husayn replaced it with a new one, Iraqi-centered and hegemonic. Even though the book hints at this new approach in a number of places (i.e., pp. 168, 193, 198), save for one enigmatic sentence (p. 96), Bengio does not seem to be fully aware of it (see, e.g., a mistaken analysis of the new Iraqi national anthem, pp. 92–4, and of the Report of 1973). Thus, what the author presents as a great dilemma (pp. 96, 97), how to carry "two watermelons [i.e., Iraqi and Arab interests, identities] in one hand," in reality under Saddam Husayn is no longer a dilemma. As he sees it, what benefits Iraq, "the pearl of the Arab crown," will eventually benefit the Arabs, and this justifies every policy that strengthens him and Iraq. Examples are the deferment of the final war against Israel, if necessary for twenty years (misunderstood by Bengio, p. 138), the rapprochement with the Iran of the shah, the assault on Islamic Iran, and the occupation of Kuwait. Bengio also does not mention the dramatic change in the way Saddam Husayn and his regime perceive the long-term future of the Arab world. Rather than the traditional melting pot notion, since 1979–80 he has envisioned a loose federation of the nation-states and peoples, led by Iraq (and himself).

The "Mesopotamian" identity flows from this new approach to Arab unity and politics. To justify Iraqi egotism as well as seniority in and leadership of the Arab family, there was need to legitimize the Iraqi entity and endow it with uniqueness. Indeed, since 1969–70 the Ba'thi regime, prodded by Husayn, has endeavored to convince the Iraqis of their singularity among the Arabs and mankind, being the offspring and heirs of the great peoples of ancient Mesopotamia. Since he became president in 1979, Husayn also has used the Mesopotamian identity as a very central component in his cult of personality. Throughout the 1980s and 1990s, the regime's media have glorified Saddam as a worthy successor to the greatest Akkadian, Babylonian, and Assyrian empire-builders and lawgivers. He has even been equated with the mythological hero, Gilgamesh, and the god Tammuz.

In the 1990s, albeit less prominent than Islamic themes, Mesopotamian ones are still conspicuous. Bengio's claim that these were used only during "the early years" of Ba'th rule" (p. 92) is contradicted by the evidence she provides to the contrary (pp. 136–7, 155, 166, 199–200). Also, the "Mesopotamian" identity is far from being merely "literary and cultural" (p. 92). Like the Islamic one, it is also manifestly political. Both the Islamic and the Mesopotamian identity have been promoted through precisely the same vehicles: literature, art, architecture, political rhetoric, state symbols, and state education. Babylon was reconstructed and mosques were built at the same time. Bengio also dismisses the Mesopotamian component on the ground that its influence on the public is not clear (p. 92), but the same may be said for the rest of the regime's discourse. Nevertheless, her book is of importance because it endeavors to expose the regime's goals and strategies.

Bengio glosses over the regime's resort to tribalism. It is not, as she suggests (p. 208), merely "a means of reinforcing Ba'thi control over the Shi'i community." Even though it runs contrary to traditional party ideology, it is a very central

mechanism for cementing Sunni-Arab support around the president. This is another example of the eclectic, ambiguous, and even cynical nature of the regime. Likewise, at the same time that Israel is demonized (pp. 134–9), Iraqi spokesmen imply that, albeit regrettable, Lebanon's peace with Israel is forgivable.

The book contains a few unsubstantiated assumptions, such as the claim that in the 1990s Saddam became a changed man and Islam "won out" in his soul (p. 182). His recourse to mysticism and Islamic rhetoric may be a calculated move for public consumption. (He has never trusted God sufficiently to dismiss his numerous bodyguards.) Also Bengio claims she is using previously untapped Iraqi sources (such as newspapers and party publications), but these have been used extensively before, including in her own previous publications. Without them any serious study of modern Iraq is not possible. This is also the appropriate place to suggest an additional source that could have benefited Bengio's study: Iraqi popular poetry (al-shi'r al-sha'bi) and songs containing political messages. A poem recited on radio and TV in the colloquial language can have a significant effect. This is the case, for example, with Hayaka ya Abu Hala, the most powerful poem glorifying Saddam Husayn.

It is incorrect to say that "Shi'is would consider any leader not descended from 'Ali a usurper" (p. 80). This is not exactly Shi'i traditional doctrine, and it is not part of the 1979 and 1989 constitutions of the Islamic Republic of Iran, for example. Also, it is true that Caliph Al-Mansur has been central to Saddam's image building (p. 81), to entice the Shi'is, but Ali has continued to play an important role.

All this does not detract from the value of the book. Bengio's study is an impressive and important contribution to the scholarship on modern Iraq and, indeed, to studies of dictatorship in general.

The State against the Peasantry: Rural Struggles in Colonial and Postcolonial Mozambique. By Merle L. Bowen. Charlottesville and London: University Press of Virginia, 2000. 256p. $65.00 cloth, $19.50 paper.

Graham Harrison, *University of Sheffield, UK*

Merle Bowen investigates the politics of the changing relationship between state and peasantry in Mozambique, principally through the elaboration of a case study, the Ilha Josina Machel in the southern province of Maputo. The book takes as point of departure the "developmental" period of Portuguese colonialism, from the early 1960s until the present day, although the main focus is on 1975–83. This is useful, because that period gives us a relatively clear insight into the reality of the Frelimo government's policies of socialism in the countryside, before all aspects of Mozambique's political economy were inundated by the war prosecuted by Renamo with South African backing. Although there has been a marked increase of research interest in local-level studies recently, few works that take a similar approach treat this period in such detail. For all those interested in the realities of rural socialism generally, or specifically Mozambique's Frelimo period, this book will be valuable indeed.

Bowen's central purpose is to produce a nuanced approach to state-peasant relations in Mozambique. This is done by articulating two key arguments. First, peasant societies are composed of such significant class and gender divisions that the notion of homogeneous peasant class is of very limited use. Second, the weakness of state power results in the generation of all kinds of local "compromises," because

authority must reconcile itself to the social dynamics of a particular community or village. These two points are well made in the book and build upon a growing body of research (e.g., Jocelyn Alexander, "The Local State in Post-war Mozambique: Political Practice and Ideas about Authority," *Africa* 67 [January 1997]: 1–25; Graham Harrison, *The Politics of Democratization in Rural Mozambique*, 2000; and Anne M. Pitcher, "Disruption without Transformation: Agrarian Relations and Livelihoods in Nampula Province, Mozambique 1975–1995," *Journal of Southern African Studies* 24 [January 1998]: 115–41).

For those who (unlike me) do not have a consuming passion for all things Mozambican, the chapters in part 2 are the most valuable. It is here that Bowen gives color to the case study. There is also sufficient recapitulation (less kindly, repetition) of the general points made in part 1 to allow the reader to dip into this section without great difficulty. The chapters provide detail about the gender relations involved in peasant productive activity and state power (e.g., pp. 101, 135, 142–7), as well as examples of peasant responses to arbitrary central state decisions and regulations (e.g., p. 142 et seq.). Chapter 6 gives a vivid account of the social relations of production. Peasants—rich or poor, men or women— engage in cultivation, artisanal production, wage labor, and trade in order to make a living or get on in life. This chapter provides life histories that illustrate not only the diversity of peasant labor but also the intensity of peasant interactions with broader socioeconomic forces and the historical contingency of these interactions.

Of course, Bowen makes her compelling arguments not only through the case study but also in relation to a theoretical conviction. It is difficult to make a clear concise account of Bowen's broader points. She begins with a clear statement that the Frelimo regime was a "bureaucratic, antipeasant, one-party state" (p. 1), but this point is elaborated inconsistently. There are two main areas of equivocation.

First, Bowen seems to have a general (and commendable) sympathy for the majority of the peasantry, but at no stage does she elaborate in theoretical terms how and why Frelimo was antipeasant. In fact, she seems partially encumbered by a desire to battle against "revisionists" (especially her bête noir, Christian Geffray) who have made precisely the point Bowen makes, but with a more explicit theoretical framing, which is opposed by Bowen. In her desire to counter Geffray, we are left wondering whether Frelimo was antipeasant at all. Richer peasants were alarmed by the "poor peasant line," a phrase Bowen puts in quotation marks but does not explain why. Later, she makes the point clearly that cooperatives and Frelimo's marketing structures more generally were a device to extract resources from all peasants (pp. 107–8). It is also not clear whether Frelimo was "mistaken," "overly ambitious" (p. 94), or the victim of the unforeseen consequences of otherwise "good policies"; or whether those policies sui generis were antipeasant.

Second, if we take Frelimo's antipeasant predisposition for granted, we are still left with a missing link: If Frelimo worked against peasant interests, what were these interests? There are ample hypotheses advanced in the literature. Bowen refutes Geffray's argument that Frelimo's opposition to the peasantry derived from its hostility toward so-called traditional power (chieftaincy and ethnicity). So, was Frelimo's opposition a result of attempts to stifle the inherent tendency of peasant "family farmers" to engage with markets and become petty commodity producers/entrepreneurs? Was it that the Frelimo regime was essentially an urban, technicist, and hierarchical organization that clashed with the rural

and diffuse social geography of peasant communities, which employ indigenous knowledge systems?

Regardless of how one theorizes "peasant interest" or Frelimo politics, Bowen's observation concerning the localized complexity of state power is an enlightening theoretical point. In various places, Bowen emphasizes that policies and plans emitted from Frelimo's Central Committee rarely translated to direct results "on the ground." Peasant societies are too diverse, and the state has too few resources to create the country after its own image. It would have been very interesting, therefore, to understand a little more about the local politics of Frelimo's authority.

In all, Bowen makes a valuable addition to research on rural politics in Mozambique. The book is quite focused on Mozambique, which makes it incumbent on the reader to develop comparative analysis. But the positive side of the same coin is that this is a detailed case study by a clearly engaged and sympathetic researcher.

Building Democracy in South Asia: India, Nepal, Pakistan. By Maya Chadda. Boulder, CO: Lynne Rienner, 2000. 247p. $49.95 cloth, $19.95 paper.

Elliot L. Tepper, *Carleton University, Ottawa*

Democracy is in vogue it seems. Everywhere in the world, the forces opposed to democracy seem to be in retreat, and the number of states calling themselves democracies is increasing. There are exceptions, of course: parts of Africa, tortured Burma, all over the Middle East, some aging Communist oligarchies, a few proud holdouts in sultanates and mountain monarchies. But they are increasingly anachronisms in the end-of-history world. Or are they? The literature is replete with controversy on the definition, durability, inevitably, and universality of democracy. Into this controversy comes a new book that takes direct aim at the literature of the past decades and provides a badly needed comparative analysis of some of the states in South Asia. Maya Chadda's goals are clear and ambitious: to bring the neglected experience of South Asia to the attention of a wider audience, in the context of the most central debates about the nature of democracy.

For the most part Chadda succeeds, and where there is weakness, the argument still serves a useful purpose. The path to democracy is not a singular one, she believes, and the complexity of the task of building democracy has been obscured by the insistence that there is only one way to go, through the stages of the Western experiences that are not relevant to all. This is not a rehash of the Asian values debate. It is a serious attempt to explain why some democracies take root and others fail. More centrally, it is an effort to reshape perceptions on the nature of democracy in South Asia.

The book is admirably organized to achieve its purposes. The argument is summarized in the opening chapter. For those more interested in democratic theory than in South Asia, all the key elements are presented. What is wrong with the views of Bruce Russett, Larry Diamond, and others is given. The first footnote is to Francis Fukayama. The rest of the book is detailed case studies of Pakistan, India, and Nepal, presenting for a new generation of readers the story of the South Asian states from the fresh perspective of Chadda's own take on Third Wave democracies. There are copious and apt footnotes, an extensive bibliography, and an almost adequate index.

For students of South Asia, the argument is recapitulated throughout but most especially in chapter 5, "State and

Democracy: the Politics of Consolidation in India." Here we find the real debate and perhaps the real purpose of the book. The author asserts that a sizable number of scholars refer to India as "a covert authoritarian state" or "at best a low quality democracy" (p. 143). Because of bias, the wrong paradigm is applied: "Unlike in Europe, India has attempted to use democracy to build the state. Nothing could be farther from the European experience, in which democracy evolved in opposition to the state" (p. 143).

The essence of Chadda's theoretical stance rests on two assertions: In South Asia, national consolidation is occurring simultaneously with the democratic process, and politics is about making accommodations, bargains, among elites. From this it follows that in India violence is the exception to the rule, used in order to remove obstacles to bargains not to crush opposition, as in the more drawn out stages of the European experience. Pakistan is an unconsolidated state. Nepal has made a successful transition to electoral democracy but is far from a consolidated nation-state. Hence, the title of the book needs to be examined more closely. The imperatives of national consolidation explain the politics of South Asia. Poverty and corruption are conditioning parameters, not structural impediments. Democracy is a tool of nation building. It differentiates South Asia from both the Western and the Far Eastern experience and makes its politics explicable.

This is where the argument becomes both a strength and a weakness of the book. It allows a reassessment of the past fifty years in South Asia and allows that experience to be put in a broad comparative framework. It also seems to suggest, however, that whatever exists is necessary and, in the case of India, exemplary. The author is not uncritical of India or the other states; far from it. But the criticism is separated from the areas of the book in which the theory is most in play. Hence, at least for some tastes, the book appears quite "soft" on the actions of Indira Gandhi or even Zulfikar Ali Bhutto in Pakistan. Both are seen by most scholars in South Asia and elsewhere as having deforming effects on the democratic process. Scholars always will find reasons for nitpicking interpretations of politics in favorite states, but the concern is more important when the interpretation may be strained to support a central thesis.

All that being noted, the book is clearly a major reinterpretation of states that do indeed need to be drawn to wider attention. It is unfortunate that even more of the South Asian Association for Regional Cooperation (SAARC) states were not included; comparative texts on South Asia are in short supply. For the most part Chadda has presented a well-researched, balanced, readable account of states that usually are neglected in the general literature and that deserve more attention. The richly textured, complex politics of South Asia have much to offer students of comparative polices, and this book is a service in that cause.

Failed Crusade: America and the Tragedy of Post-Communist Russia. By Stephen F. Cohen. New York: W. W. Norton, 2000. 320p. $21.95.
Russia's Stillborn Democracy?: From Gorbachev to Yeltsin. By Graeme Gill and Roger D. Markwick. Oxford: Oxford University Press, 2000. 296p. $65.00 cloth, $24.95 paper.

Mark R. Beissinger, *University of Wisconsin-Madison*

Both of these books seek to explain the failure or shortcomings of Russia's postcommunist democratic experiment. Both are critical of much of the transition literature within political science, and both identify certain features that make the

prescriptions of that literature problematic within the Russian and Soviet contexts. But they anchor their criticisms in contrasting explanations of the current travails. One is rooted in the importation of inappropriate models of economic and social development under Western prodding; the other points to the weakness of Russia's nascent civil society and the opportunities lost by political leaders to strengthen it. They provide opposing views not only of the causal processes underlying failed or incomplete democratization within Russia but also to some extent of the purposes of the social scientific enterprise itself.

Failed Crusade is a polemic against America's attempt to transform postcommunist Russia into a facsimile of U.S. democracy and capitalism—a project, Cohen suggests, that was doomed from the outset. Instead of "reform," the imposition of inappropriate, foreign assumptions onto Russian reality has produced a tragedy of singular scope: an economic downturn worse than the Great Depression; mass impoverishment; a media and political process manipulated by financial moguls; a monarch-like presidency; a government of corruption and favors; and a strong current of anti-Americanism. At the heart of this fiasco lies "transitionology"; this new orthodoxy, Cohen argues, views Russia's transition from communism to capitalism and democracy as inevitable, necessary, and progressive. He makes an analogy between the dominance of transitionology in the study of contemporary Russia and the hegemonic role of the totalitarian model in the study of the Soviet Union during the Cold War; both models serve larger political purposes, and their soothing conclusions mesmerize adherents into misinterpretation of Russian realities.

Cohen spares no invective in accusing American policymakers, journalists, and especially academics of engaging in professional "malpractice" for making Russia a laboratory for their foreign theories. "By trying to squeeze Russia's post-Communist realities into American and other 'comparative' preconceptions, by giving things names they do not warrant, scholars and journalists have debased the vocabulary of their professions" (p. 27). The remedy for transitionology, Cohen asserts, is a good dose of history. Journalists and scholars should liberate themselves from pseudo-experts who view Russia as just like any other country, and Russia needs to find a path of development more in accord with its past and political traditions, a third way between orthodox communism and Western dogmas. Despite Cohen's left-wing credentials, his argument strongly parallels interpretations one might find within the nationalist portion of the Russian political spectrum.

For Cohen, no special method or theory is necessary to arrive at this conclusion, "only a willingness to set aside preconceptions and learn as much as possible about Russia's past and present" (p. 69). Scholars of democratization should find such an approach both troublesome and provocative, for it challenges the assumptions underlying comparative analysis of democratic transitions, namely, that lessons can be learned and transferred from the successes or failures of democratization in other contexts. Yet, Cohen's analysis of political processes in contemporary Russia is exceedingly thin in comparison with his thicker focus on the foibles of American Russia-watchers. Even here, much of the critique remains at the level of platitudes and stereotypes. It is simply not true, for instance, that most Western scholars supported Yeltsin's violent assault on parliament in October 1993, enthusiastically embraced Yeltsin, or were uncritical about the application of "shock therapy" to the Russian economy. And the type of orthodoxy Cohen claims is the root cause of Russia's problems does not exist within the American schol-

arly or policy communities. His contribution to the debate over Russian democratization is his argument that its shortcomings are attributable by and large to the failure of Western cultural categories and expectations foisted upon Russia from outside, not primarily to the continuing effect of the Soviet legacy or the actions of Russians themselves.

This stands in marked contrast to the arguments of Gill and Markwick, who are critical of the democratization literature but ground their critique largely within its assumptions, rather than reject the transition literature as a form of ethnocentrism or cultural imperialism. They argue that this literature tends to emphasize the importance of pacts and agreements among political elites but underplays or ignores the pivotal role of civil society in democratization. They define a civil society as one in which "third-order groups," that is, groups "specifically political in their outlook and activities" (p. 6), are allowed to flourish, although the authors often treat civil society as an ideal type, referring at times to the absence of a "true civil society" (p. 115) in the Soviet Union and postcommunist Russia, despite political liberalization.

Gill and Markwick argue that the fate of democratization in the Soviet Union and Russia demonstrates that "a democratic outcome is much less likely when the process is dependent purely upon elite preference than when civil society forces are active and powerful" (p. 6). Russia's "stillborn democracy" is explained not by the importation of inappropriate models of economic and political development from abroad but by the very nature of Russian society—its incapacity to absorb democratic change and to curb the ambitions of elites and leaders, largely due to the unitarist, bureaucratic character of the Soviet polity and the ways in which subsequent leaders built upon this legacy.

Focusing on the failures of democratization in both the Gorbachev and Yeltsin years, Gill and Markwick root these outcomes in a stunted civil society and the consequent dysfunctions of elite-led democratization. During the Gorbachev years, the rudiments of a civil society began to emerge, but it remained relatively feeble. It exercised a weak influence on the outcome of glasnost politics, which left the political field instead to nationalist mobilization and to elites who tore the Soviet Union apart. In the Yeltsin years the development of a civil society was blocked by a dominant presidency and the monopolization of political decision making by the executive branch. Democracy, they conclude, cannot be made in a vacuum; it requires a polity in which elites represent social interests that are broader than their own narrow self-interest.

Gill and Markwick do not foreclose the possibility that democracy may eventually develop, but they provide no guidance as to how this might occur, and their tendency to speak of civil society as an ideal type seems to undermine such an exercise. Indeed, for all their emphasis upon the need for a vibrant civil society for successful democratization, they focus largely on elite actions rather than the struggles of civil society to emerge as a political force and the obstacles societal actors encountered. There also is no attempt to explain the differential outcomes of democratization across Soviet territory. Why did it proceed more smoothly in the Baltic than elsewhere, despite the same Soviet legacy of unitarist bureaucracy? Was nationalism a substitute for civil society (as the authors assert for the Gorbachev years) or the product of it? Was a stunted civil society the cause or result of incomplete democratization in the Soviet Union and Russia (both arguments are made at various points in the book)? And if civil society is critical to the outcome of democratization, have external efforts to foster the emer-

gence of such forces borne any fruit? Although these questions are not addressed, Gill and Markwick provide a useful chronicle of the momentous changes in Russia during these years.

Some readers may be tempted to interpret the differences between these works as the domestic and international faces of a single political crisis, but ultimately they must be understood as fundamentally irreconcilable positions concerning what transpired during a pivotal period of Russian political development. Was democratization short-circuited by the imposition of inappropriate models of socioeconomic development or by the inability of Russian society to absorb changes? As these books reveal by applying rival interpretive frames to the same facts, the divide between these perspectives is likely to endure so long as Russian democratization remains incomplete.

Paths toward Democracy: The Working Class and Elites in Western Europe and Latin America. By Ruth Berins Collier. Cambridge: Cambridge University Press, 1999. 230p. $49.95 cloth, $17.95 paper.

Robert M. Fishman, *University of Notre Dame*

Ruth Collier has written an important contribution to the literatures on democracy and on labor, one that should reorient much discussion and work on democratization. Collier makes a powerful case that labor has been a far more important and decisive actor in redemocratization than conventionally thought, and her reconceptualization of the cases on this basis yields an important new typology of regime transitions. The book casts a wide empirical net, examining the contribution of labor to the emergence of democracy in numerous countries in both the Americas and Europe. Drawing on the rich foundation of scholarship on particular national cases, Collier analyzes the role of labor in early, and often protracted, instances of democratization during the nineteenth and early twentieth century, as well as in more recent experiences of transition to democracy.

The dual focus on two substantially different historical periods leads Collier to assess prevailing theoretical approaches for each of them. In dealing with the earlier period, sociologically oriented analysts have argued for the causal priority of the working class in the construction of democracy, but in examining democratic transitions, political scientists have emphasized instead the agency and strategic decisions of elites. Collier's thorough and reasoned examination of the case studies leads her to question both of these interpretations, which she believes miss much of the story. She persuasively argues that the working class is less singularly crucial than has been argued for the early period and is often substantially more important than conventionally assumed in the Third Wave transitions.

The major contribution of this superb book rests in part on its sheer empirical scope. Collier examines scores of cases in sufficient depth to permit the reader to follow her comparative and conceptual logic, and she offers ample bibliographic references for those interested in pursuing country-level specificities in greater depth. The concise, systematic, and clear presentation of the cases adds appreciably to the book's accessibility; it should have a substantial influence on the thinking of comparativists. Even more important is the book's elegant conceptual reformulation of recent transitions to democracy. Collier's stimulating typology differentiates cases on the basis of the role played by labor. Her focus is clearly on the initial installation of democracy rather than on

the more protracted consolidation treated by some comparative analysts.

In Collier's formulation, labor is often a fundamental causal force in the move toward democracy, even if other actors appear far more relevant in the public arrangements. Thus, in the democratization path she labels "destabilization and extrication," pressure applied by labor's opposition to antidemocratic rule induces crucial sectors of the political elite to undertake the construction of democracy. In this scenario, many of the politically relevant divisions within the circles of authoritarian rule, typically cited as decisive in conventional interpretations, are attributed by Collier to the destabilizing pressure effectively applied by labor.

Collier weighs the empirical record of each case carefully to assign greater or lesser causal relevance to labor pressure. She scores labor as a relevant prodemocratic actor only if its democratic commitments are explicit and are accompanied by mobilizational capacities sufficient to reorient the strategies of regime incumbents. The four paths to democracy she formulates differ from one another in the magnitude of labor's protagonism, ranging from decisive importance in what she labels "destabilization and extrication" transitions, to "interelite game" transitions in which labor lacks significance. Country specialists will doubtless take up, and in places qualify, specific judgments, but Collier's overall comparative recasting of transitions will strongly influence scholars and students of democracy. Her achievement in distinguishing among transitions on the basis of labor's greater or lesser salience should not be underestimated.

In Collier's formulation, labor's contribution, where present, rests on its democratic intent and its ability to destabilize authoritarian rule. The roots of labor's political relevance are thus largely internal to this collective actor and are manifested in the course of democratization through workers' mobilizational capacity. Future research, building on this excellent contribution, may well revisit this point, given that it differs somewhat from other views found in the literature. In his highly stimulating comparative discussion of labor and transition, J. Samuel Valenzuela ("Labor Movements in Transitions to Democracy," *Comparative Politics* [July 1989]: 445–73) argues for the importance not only of mobilization but also of strategically determined moderation and restraint on the part of labor. Some of the country-specific literature also underscores the strategic role of both moderation and mobilization. Indeed, Collier finds evidence of labor restraint in some cases, such as Spain and Bolivia, but it plays little role in her theorization.

Some work on labor moderation deals more with the consolidation phase, whereas Collier focuses on installation of democracy, but an argument can be made that in some national cases labor's turn toward moderation and restraint contributed markedly to the establishment of democratic rule. Worker restraint can undercut the arguments of authoritarian hardliners who allege that a political opening will foster severe class confrontation. In this sense, it is possible that labor's overall contribution to the growth of democracy may be even greater than argued by Collier on the basis of crucial experiences of labor mobilization in the making of democracy.

A lasting contribution of this book should be a heightened awareness of labor's role and an intellectual commitment to use that as a basis for systematically conceptualizing the road to democracy. Nonetheless, neither Collier nor other students of labor would argue that the foundations of democratic transition lie exclusively within the working class. Indeed, much of *Paths toward Democracy* addresses the limitations on labor's protagonism, and its central claim for

the earlier period is that democracy was built on multiple class foundations. Still, in analyzing the recent cases, Collier draws explanations and distinctions rooted above all in the greater or lesser salience of labor.

One step that remains for future research is to explore systematically the determinants, outside the working class, of observed differences in labor strategies and capacities during regime transitions. Good work has been done along these lines, such as the recent study by Rafael Durán, who analyzes the enormous contrast between Spain and Portugal in the role of labor in democratic transition (*Contención y transgresión: Las movilizaciones sociales y el Estado en las transiciones española y Portuguesa*, 2000). Durán argues that labor's initial grievances were similar in both cases, but revolutionary mobilizations in Portugal and worker restraint in Spain were shaped by fundamental differences in state capacity during the transition. The broader comparative analysis of determinants of labor's transition strategies remains an ongoing task for future scholars, building on Collier's excellent contribution.

Democratic Institutional Design: The Powers and Incentives of Venezuelan Politicians and Interest Groups. By Brian F. Crisp. Stanford, CA: Stanford University Press, 2000. 294p. $100.00 cloth, $34.95 paper.

Unfinished Transitions: Women and the Gendered Development of Democracy in Venezuela, 1936–1996. By Elisabeth J. Friedman. University Park: University of Pennsylvania Press, 2000. 352p. $65.00.

Daniel C. Hellinger, *Webster University*

Venezuelan politics attracted little attention from political scientists for thirty years after the defeat of the *fidelista* guerrillas in the 1960s, but there has been a surge of interest in recent years. The country retained civilian, elected government through a dark period of authoritarianism in Latin America, which seemed to make it a good candidate for deriving lessons about transitions to democracy. In the 1990s, however, the democratic system entered into crisis. Venezuela experienced urban riots, two unsuccessful coups, removal of a president from office before completion of his term, rising electoral abstention, collapse of the traditional parties at the heart of the system, and the election of a coup leader to the presidency. Attention shifted from what went right to what went wrong. These books help us understand the limitations of the Venezuelan democratic model.

Crisp is concerned with the way Venezuela's constitutional design and extraordinarily disciplined political parties structured incentives for elected officials and influenced their exercise of legislative and representative functions. Although the 1960 Constitution gave few legislative powers to the presidency, most major legislation originated from the executive branch, and Congress rarely exercised its oversight functions effectively. The weakness of Congress, and local legislatures as well, lay in (1) the list-based system of proportional representation, (2) undemocratic internal party structures that deprived legislators of any accountability to constituents, and (3) a system of tripartite commissions that substituted themselves for legislative processes open to popular influence.

Crisp's thesis resembles that of Michael Coppedge (*Strong Parties and Lame Ducks*, 1994) but his examination of the role of the commissions adds significantly to our understanding of how Venezuela was governed during the "fourth republic," as Venezuelans now call this era from 1958 to 1998. The tripartite (business, labor, government) commis-

sions were an extension of the 1958 democratic pact and the minimum program forged at the conclusion of the dictatorship. They were the key instruments used by Venezuelan presidents to forge a policy consensus, but they so supplanted the congressional committee structure that alternates came to exercise routinely the diminished functions of the deputies and senators themselves.

The pluralism implied by representation on commissions did not benefit all sectors equally, as Crisp shows in his quantitative and qualitative analyses of the relative weight of capital and labor on the commissions and of their access to the executive branch and its important decree-making authority. Labor leaders linked to the traditional parties enjoyed political access, but other popular sectors were usually excluded. In addition, the economic development strategy that emerged was more favorable to capital. When capital flight occurred, the government tended to lessen labor's representation on commissions, and increased strike activity brought no corresponding increase in labor representation or decline in business participation.

Crisp locates his study within the institutionalist school, but one can question how deeply it reveals the roots of Venezuela's crisis. A more balkanized system on the U.S. model is just as susceptible to inertia because party leaders have limited means to affect the incentives of elected officials. Venezuelan party leaders conceivably could have used party discipline to induce politicians to accept change to rules of the game defined by the 1958 pact. The party system provided few incentives for representatives to challenge leaders and little power to affect policy, but what shaped the leadership?

In the "Postscript," Crisp comments on the 1999 Bolivarian Constitution and concludes that the institutional incentives for more democracy will be undermined if "organizational life becomes more formalized over time" (p. 234). Is it the formalization of internal party procedures or the way the party elite is integrated into broader structures of power (especially those defined by race, class, and gender) that makes the difference? As Crisp himself demonstrates, the success of the Venezuelan system was its ability to insulate the elite from popular influence, even as it allowed them to resolve their disputes with one another. Yet, institutional changes in and of themselves may not produce expected results. Their effect may vary according to the way they are imbedded in the broader cultural and socioeconomic context.

Elisabeth Friedman suggests that the insulation of Venezuelan politicians from popular influence cannot be understood without integrating gender into our analytical frameworks. An intriguing finding is that the demobilization of women in transitions leaves democratization itself unfinished. Her book follows paths broken by recent feminist and gender studies, but Friedman slashes down some undergrowth herself. She adds to evidence that women's influence rises during periods of antiauthoritarian struggle only to dissipate during the transition to and consolidation of more democratic regimes. Democratic transitions are gendered in the way that they reinforce patriarchy, but democracy itself is also left "unfinished" as a result of patriarchy.

Most research on women in democratic transitions is restricted to the so-called Third Wave era. Few studies reach back before 1980. Friedman provides cross-national analysis, but she also fills a void with her longitudinal perspective. She considers four periods, including two episodes of transition. The first runs from the death of the dictator Gómez in 1935 to the downfall of the first elected government of 1948; the second from the struggle to overthrow the Pérez Jiménez dictatorship (1952–58) to the start of the oil bonanza of 1973. Succeeding chapters examine the struggle of women's move-

ments to influence labor law, family law, and civil rights during the period of consolidation (1973–90) and the politics of crisis (1990–95).

In the two transitions, women were involved in democratizing movements either as members of party units for women or as *enlaces* ("links") who provided logistical support for male leaders. Both forms were imbedded in a patriarchy, but because men tended to regard women's bureaus as unimportant compared to labor, professional, and other sectors, women were more likely to be able to manage their own affairs. They were free of the close scrutiny and control that male leaders exercised over other party organs. Future leaders were more likely to emerge from this sector.

The high point of success for the women's movement was the enactment of progressive changes to the Civil Code in 1982. The code reform was not all that women wanted, but it was shaped and advanced by popular initiative and never coopted by the male-dominated parties. When male party leaders formed a commission to define a code at odds with women's goals, women responded with popular and autonomous pressure rare in Venezuelan democracy. This autonomy was due to the ability of women to unify others across party lines to take advantage of political opportunities.

Friedman freely employs the notion of political opportunity structure, now common in studies of social movements. She does not explain away shortcomings, particularly reluctance to challenge gender difference, by appealing to factors beyond the movement's control, however. Women's organizations achieved unity by eschewing "feminism" and limiting challenges to gender difference, not gender hierarchy. The movement succeeded in winning battles that were defined as defense of women's domestic responsibilities but failed, for example, to insist on valuing domestic labor as much as other forms of work. It also failed often to surmount the class and cultural biases of middle-class women, who made up most of the leadership.

Women's organizations suffered attrition when they failed to address the economic problems faced by poorer women, especially following neoliberal measures introduced after 1990. Also, women were victims of their own success. When their organizations demanded and received state support in the same way male-dominated sectors received it, this opened them to cooptation by the state and parties.

Institutional reforms designed to reduce the power of party elites did not necessarily increase democratic opportunities for women. Uninominal representation reduced incentives for parties to name women candidates. Political decentralization created opportunities for "instrumental feminists" (female politicians more interested in advancing their career than women's interests) to displace more honest brokers. These findings dispute the conclusions of institutionalists who argue that the reforms did not go far enough. A gendered analysis suggests that reforms rendered Venezuela less, not more, democratic in some important ways.

Friedman has left a few patches of undergrowth undisturbed along the way to advancing gender analysis in comparative politics. Gay and transgender issues are ignored. There is one brief reference to race, even though the economic gap between middle-class women and their domestic help in Venezuela is reinforced by a racial difference that overlaps class cleavages in Latin America. One also might have hoped for some gender analysis of the protests that drove President Carlos Andrés Pérez from power.

These authors completed their research before the 1998 electoral victory of President Hugo Chávez, the cashiered coup-plotter, sounded the death knell for the 1958 system. Both contribute to our understanding of the political decay of

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a democratic regime that many had thought consolidated. Friedman's book has the additional virtue of uncovering a subaltern history. It ought to influence not just gender studies but also studies of democratic transitions—provided a male-dominated profession such as political science is willing to ask more consistently: "What difference does gender make?"

State Legitimacy and Development in Africa. By Pierre Englebert. Boulder, CO: Lynne Rienner, 2000. 243p. $55.00.

James R. Scarritt, *University of Colorado at Boulder*

In this well-informed, theoretically and methodologically sophisticated, and highly innovative book, and in related articles that appeared recently in *Political Research Quarterly* (53 [March 2000]: 7–36) and *World Development* (28 [October 2000]: 1821–35), Pierre Englebert proposes and tests the hypothesis that variations in vertical and horizontal state legitimacy account for variations in the developmental capacity and economic growth of African states and of Africa in comparison to other world regions. Developmental capacity, which combines an index of specific economic policies (emphasizing a free market and human capital) and an index of good governance, is the crucial intervening variable between state legitimacy and growth.

Englebert argues that state legitimacy is more important for developmental capacity and growth than social capital or ethnic homogeneity, the competing explanations that receive the most attention in the literature. With such legitimacy the best way for leaders to remain in power is through pursuing policies that lead to development (defined as growth), but without it they feel they can remain in power (get control of their states and empower them to control their societies, p. 96) only through pursuing neopatrimonial fusion of elites. Nation-building policies that interfere with development transform all state institutions "into instruments of distribution, co-optation, or reward" (p. 104) and ultimately lead to state collapse. They cannot gain legitimacy through development because, "under conditions of weak initial legitimacy, bureaucrats are insufficiently loyal to the state and private agents too distrustful of its institutions" (p. 100).

The major controversy about Englebert's analysis is likely to center on his definition and measurement of state legitimacy, which he acknowledges to be significantly different from legitimacy as commonly defined. "A state is legitimate when its structures have evolved endogenously to its own society and there is some level of historical continuity to its institutions" (p. 4). Vertical legitimacy is the quality of relations between society and political institutions, and it is measured as a dichotomous variable through a decision tree. To be illegitimate states must have all the following characteristics: They were colonized, had human settlement before colonization that was not reduced to insignificance or assimilated under colonialism, did not re-create a precolonial sovereignty through gaining independence, and after independence do severe violence to existing institutions. Ten African states have vertical legitimacy: Ethiopia because it was never colonized; Botswana, Burundi, Lesotho, Rwanda, and Swaziland because they maintain a degree of continuity with, or do not do violence to, precolonial institutions; and Cape Verde, Mauritius, Sao Tome and Principe, and Seychelles because they were uninhabited before colonization. All other African states lack this form of legitimacy.

A more conventional definition would also classify seven of these states as highly legitimate but would classify Ethiopia, Rwanda, and Burundi as having low legitimacy because, as

Englebert recognizes (p. 155), they have been torn by civil war and have been highly repressive. Although not a product of Western colonialism, the boundaries of Ethiopia are the product of conquest by the Amhara-Tigre empire that occurred under Emperor Menelik during the period of European colonialism, and civil war was in part a product of resistance to this conquest. In Rwanda and Burundi the rule of the minority Tutsi over the majority Hutu was consolidated under German and Belgian colonialism, and civil war has been the product of resistance to and defense of this rule. These countries are included as legitimate to "make a reverse selection on the dependent variable, looking at the record of capacity and growth and giving the country the score that will least support the hypothesis" (p. 129). But in this case this laudable methodological principle may justify a less discriminating definition of legitimacy.

Horizontal legitimacy refers to the degree of continuity between pre- and postcolonial borders, and it is measured by the percentage of a country's population that belongs to ethnic groups *not* partitioned by borders. The two types of legitimacy are correlated at 0.68, and the average horizontal legitimacy of vertically legitimate states is 0.98. Although Englebert demonstrates that horizontal legitimacy has stronger effects on capacity and growth than does ethnic heterogeneity, he might have found an even stronger relationship if he had developed a single measure that combined partitioned groups and internal ethnopolitical fragmentation rather than ethnic heterogeneity (James R. Scarritt and Shaheen Mozaffar, "The Specification of Ethnic Cleavages and Ethnopolitical Groups for the Analysis of Democratic Competition in Contemporary Africa," *Nationalism and Ethnic Politics* 5 [Spring 1999]: 82–117; or Daniel N. Posner, "Ethnic Fractionalization in Africa: How Should It Be Measured? What Does It Explain about Economic Growth?," paper presented to the World Bank Development Research Group Seminar, 29 March 2000). This would have brought the author closer to his stated goal of "measuring the congruence between precolonial and postcolonial polities" (p. 155).

Englebert first illustrates his hypothesis with a ten-page most different systems comparison of Botswana and Congo. He then presents a sophisticated statistical analysis of the effects of vertical legitimacy on developmental capacity, its components, and growth in 99–126 (depending on data availability) developing countries. This analysis demonstrates that vertical state legitimacy is positively related to capacity and growth in the entire sample, and its inclusion in the analysis explains why performance on these variables is lower in African countries than elsewhere; legitimacy, in other words, makes the frequently discussed Africa dummy variable insignificant. Finally, he presents a similar analysis of thirty-four African countries, demonstrating that both vertical and horizontal legitimacy are positively related, with varying strength, to capacity, its aspects, and growth. He notes that the performance of legitimate African states remains below world averages on most of these indices, largely due to the inclusion of Ethiopia, Rwanda, and Burundi in this category.

In conclusion, Englebert suggests that weak legitimacy greatly hinders the implementation of structural adjustment programs in Africa, and he proposes decentralizing institutional reforms under which traditional groups would be given a greater role, as well as consideration of border changes for some countries. He is on target with the first two points, but he does not analyze the potential dangers involved in making border changes, given the multiple levels of ethnopolitical identity in almost all African countries. This book will have a lasting and positive influence on African and development

studies, and it would be excellent assigned reading for upper division and graduate courses if it were in paperback.

Closing the Shop: Information Cartels and Japan's Mass Media. By Laurie Anne Freeman. Princeton, NJ: Princeton University Press, 2000. 256p. $39.50.

Patricia L. Maclachlan, *University of Texas at Austin*

Although Japan scholars have long been aware that media institutions facilitate state efforts to curtail the free flow of information within Japanese society, *Closing the Shop* is the first comprehensive treatment of this important topic. The book was well worth the wait.

Of particular interest to Freeman are the so-called *kisha* (reporters) clubs that are attached to political and bureaucratic organs and that consist primarily of journalists from the mainstream newspapers. These clubs are governed by formal and informal rules that accord club members proprietary access to official information and incentives to cooperate both with one another and with their sources in the collection and dissemination of the news. Those who transgress the rules and norms, moreover, are subject to sanctions administered by either the club or its sources.

Freeman refers to these clubs as "information cartels," an interesting use of economic terminology that highlights the many parallels between the media and Japanese economic practices as well as the negative side-effects of club rules: the dampening of competition among rival journalists, the homogenization of newspaper reportage, and the creation of a risk-averse press that reacts to agendas set by bureaucrats and mainstream politicians, to cite just a few examples. Freeman rounds off her discussion of the contemporary industry with an exploration of two other sets of institutions that contribute to media cartelization: industry associations, including the Japan Newspaper Publishers and Editors Association (NSK), and the media conglomerates (*keiretsu*) controlled by the mainstream newspapers.

Freeman adopts a "relational" approach to her subject that focuses on formal and informal linkages among rival journalists and between those journalists and their sources. In keeping with most institutionalist treatments of political science topics, Freeman offers no simple theories or propositions; instead, she uses empirical evidence gleaned from interviews and the primary and secondary Japanese literature, and she adds comparisons with relevant examples in the West and her own personal experiences as an inside observer of the industry. She shows how the role and political influence of the media have varied according to specific institutional linkages and across periods. At the same time, Freeman highlights a degree of continuity within the newspaper media by tracing, in chapter 2, the development of constituent institutions, from their inception during the early Meiji period (1868–1911) to the present. This highly effective mix of historical and institutional analysis provides not only a clear picture of the media's place in modern Japanese politics but also a keen appreciation of the limits of change within media institutions.

Closing the Shop is a fine contribution to the field of comparative media studies and is an important addition to the comparative literature on Japanese domestic politics. A major strength is the persuasive analysis of the implications of Japan's distinctive media industry for state-society relations. For example, information cartels benefit sources as much as the media itself by serving as "a convenient means of filtering news and information and socially constructing the world view held by the public" (p. 5). This is not, however, a

straightforward argument on behalf of state supremacy over the citizenry, and it does not portray the media as mere lapdogs of state institutions. Instead, Freeman effectively depicts the media as "collaborators" or "coconspirators" of political and bureaucratic authority.

Key to understanding this distinctive relationship is the recognition that the media have had interests of their own over the years:

"It is not that journalists blindly follow the dictates and wishes of state sources, or that they are subservient to the powers that be, but that the system for gathering and reporting news—a system designed to serve media companies' economic and other interests—has frequently led them to support state goals" (p. 162).

Freeman goes on to show how the media have exchanged some of their autonomy in return for privileged access to official sources and other benefits that contribute to the industry's long-term security. Clearly, the only losers are information consumers: the Japanese public.

This book should be classified as part of an expanding literature on Japanese state-society relations, represented by Daniel Okimoto's *Between MITI and the Market: Japanese Industrial Policy for High Technology* (1989), Sheldon Garon's *Molding Japanese Minds: The State in Everyday Life* (1997), and political economists who write under the "network state" heading. As do many of these authors, Freeman argues that state capacity is ultimately determined not only by factors inherent within state institutions but also by state linkages with outside actors. The state in context, in other words, is stronger than the state acting alone. Garon has shown how collaborative linkages between state actors and citizen groups enable the former to "manage" the citizenry on behalf of controversial state projects; Freeman demonstrates how linkages between the state and media filter what the citizenry is told about events within the Japanese polity.

For some readers, *Closing the Shop* may raise an unanswered question or two. Although the point that the media do not perform significant agenda-setting functions within the political system is certainly well taken, to what extent does the media influence other aspects of the Japanese political process? For example, what effect, if any, have the media had on voting behavior? To what extent, if at all, have the media been able to influence the formulation stage of the policy process by conveying alternative points of view to mainstream policymakers? As it stands, the book focuses almost exclusively on examples of industry self-censorship, particularly with regards to the coverage (or lack thereof) of political scandal; by broadening the scope of her case studies, Freeman could have provided an even more compelling depiction of the mainstream media's risk-averse and reactive nature. Such questions are reduced to mere quibbles when measured against the book's many strengths. *Closing the Shop* is well researched, nicely written, and concisely argued; quite simply, it is a pleasure to read.

Unions, Employers, and Central Banks: Macroeconomic Coordination and Institutional Change in Social Market Economies. Edited by Torben Iversen, Jonas Pontusson, and David Soskice. Cambridge, UK: Cambridge University Press, 2000. 339p. $64.95 cloth, $22.95 paper.

Chris Howell, *Oberlin College*

Comparative political economy has been transformed since the end of the 1970s. The explanatory value of class conflict, the power resources of social classes, and the social base of

particular national models of political economy have been replaced by an emphasis upon the role of institutions in explaining both how contemporary political economies function and their capacity to manage international economic integration. The fruits of this institutional turn have now emerged into a fully fledged new approach, as evidenced by the volume under review, by *Continuity and Change in Contemporary Capitalism* (edited by Herbert Kitschelt, Peter Lange, Gary Marks, and John D. Stephens, 1999), and by a forthcoming volume, *Varieties of Capitalism*, edited by Peter Hall and David Soskice. These three books overlap to a great degree in both theoretical approach and list of contributors. It is not too strong to suggest that this approach has come to dominate comparative political economy.

Unions, Employers, and Central Banks has two particular strengths. First, it extends the conventional emphasis upon the role of institutions in mediating economic change by pointing to the interaction effects among different sets of institutions and to the manner in which institutional transformation in one area can change the preferences of economic actors, leading to defection from stable patterns of cooperative behavior. Second, several of the contributions offer a highly nuanced and often quite critical reflection upon the strengths, but the weaknesses as well, of new institutionalist political economy. This critical perspective is most evident in the introductory chapter by Iversen and Pontusson, which points to the dangers of entirely replacing the explanatory power of class conflict with bland notions of coordination and of permitting evidence of cross-class alliances to imply a convergence of class interests. In short, this volume maintains a valuable bridge to older traditions of political economy and resists the temptation to throw out insights derived from them.

The importance of interaction among institutions is central to several of the chapters, notably David Soskice's account of New Keynesian approaches to political economy, the examination by Robert Franzese and Peter Hall of the relationship between wage bargaining and monetary policy, and Torben Iversen's explanation for the worsening tradeoff between employment and equality for social democratic regimes. The main focus of these accounts is on why a general move toward greater central bank independence and nonaccommodating monetary policy generates different outcomes, in particular why the unemployment cost of this policy shift varies to such a great extent cross-nationally. These authors convincingly argue that the interaction of monetary policy with wage setting is crucial to the outcome. As Franzese and Hall point out, effective monetary policy rests upon successfully signaling central bank targets to those engaged in wage bargaining, and coordinated bargaining of some kind makes it more likely that monetary goals will be incorporated into wage setting. Iversen argues that the relationship between wage bargaining and monetary policy is further complicated by their interaction with social policy. Crucial parts of the social democratic model rest upon aspects of social policy—the expansion of the social wage, the state as employer of last resort—that have become increasingly unsustainable in the context of the turn to monetarist orthodoxy. The resulting shift in social policy has in turn undermined centralized wage bargaining.

The common theme of these contributions is that linear explanations which attempt to read off inflation and employment outcomes from the degree of central bank independence, or macroeconomic policy shifts more generally, will fail to capture the manner in which interaction among institutions influences outcomes. One caveat about these chapters is that the data on which they are based are quite

dated. Franzese and Hall's data end in 1990, and those of Soskice and Iversen end in 1993. The recent ability of Britain and the United States, with uncoordinated wage-setting arrangements, to achieve simultaneously low inflation and low unemployment at least raises the question of whether wage bargaining any longer influences the effect of monetary policy.

The "varieties of capitalism" approach to comparative political economy, of which this volume is a fine example, is an intervention in the debate over the degree to which one can identify a broad convergence in the political economies of advanced capitalist countries upon a neoliberal or "liberal market economy" model exemplified by the United States and Britain. The emphasis in this volume upon the refraction of international economic pressures through domestic institutions, the interaction of those institutions, and the extent to which comparative institutional advantage varies cross-nationally all point toward a rejection of the convergence thesis by the contributors. In the introductory chapter, Iversen and Pontusson argue instead in favor of "dual convergence," in which the more liberal market economies converge upon the United States, and the more coordinated market economies converge upon Germany. Thus, the dismantling of peak-level bargaining in social democracies is replaced by industry bargaining, not a more thorough-going decentralization of wage setting, and generous, universal welfare states are trimmed, not razed. Meanwhile, "the institutional differences between organized and liberal market economies remain as great as they ever were" (p. 33).

The dual convergence thesis is important because it rejects an overly static, path-dependent interpretation of contemporary developments in political economy. Iversen and Pontusson argue that broad economic shifts—particularly the growth of postindustrial employment and more flexible production strategies—have created severe stresses for coordinated market economies, leading to significant institutional change. Indeed, they offer a comprehensive critique of a contribution to their own volume: the Wallerstein and Golden chapter on wage setting in four Nordic countries. In that chapter, the authors conclude there has been little change and certainly no decentralization in collective bargaining, with the exception of Sweden. Iversen and Pontusson point out the limitations of the data used and suggest that more significant change has taken place.

Qualitative case studies address the subject of institutional change. Peter Swenson and Pontusson describe the breakdown of solidaristic wage bargaining in Sweden, and Kathleen Thelen explains German employer ambivalence toward the reform of collective bargaining; both emphasize that wage bargaining institutions mediate how broad economic pressures influence employer interests. In both cases, employers play the central role in reforming industrial relations, but in Sweden centralized wage bargaining made it difficult for them to attract skilled labor and implement new production strategies and incentive pay schemes, whereas in Germany any weakening of industry-level bargaining, although superficially attractive to many employers, would have threatened cooperative relationships with workers and the flexibility afforded employers by negotiating with works councils over work organization and skill issues. The result was a concerted and successful push to end peak bargaining by Swedish employers and a defense of industry bargaining by German employers. Thus, similar economic pressures led to different employer strategies because of the institutional configuration of industrial relations in the two countries.

Although arguing in favor of dual convergence is an advance upon arguing against any convergence, the empirical

evidence provided in this volume is not entirely convincing, primarily because of its scope. The contention that no convergence has taken place between liberal and coordinated market economies cannot be evaluated because no contribution examines the liberal market economies; the concern of the authors is overwhelmingly with developments among coordinated market economies. Furthermore, to a remarkable degree this volume focuses upon Sweden and Germany to the exclusion of other coordinated market economies, so it is not surprising that the relative institutional stability of the latter compared to the dramatic change in the former should appear interesting and suggestive. But that is not enough to validate a claim of widespread convergence on the German model. It also risks overemphasizing the stability of the German political economy. Thelen highlights the reasons wholesale restructuring of the German model is unlikely but makes it quite clear that growing numbers of employers are prepared to reenvision certain institutions. Developments since that chapter was written, particularly the election of a governing coalition of social democrats and greens, suggest more institutional fluidity than the dual convergence argument permits.

That said, this volume amply demonstrates the value of emphasizing the institutional configuration of advanced capitalist political economies and further develops that approach in important ways. The superb overview of, and reflection upon, the state of comparative political economy contained in the introductory chapter is alone worth the price of admission.

The Politics of Duplicity: Controlling Reproduction in Ceausescu's Romania. By Gail Kligman. Berkeley: University of California Press, 1998. 358p. $44.95.

Sharon L. Wolchik, *George Washington University*

Gail Kligman analyzes the reproductive policies of the Ceausescu regime and the responses of experts and the population to those policies. Drawing on extensive interviews with ordinary men and women as well as experts and on archival research, the author provides a wealth of information about demographic trends and reproductive policies in Romania from the outset of the communist era to the violent overthrow of the Ceausescu regime in late 1989. Kligman traces the way in which communist leaders used legislation, measures to "protect" women in the workplace, propaganda, and control of the media and other sources of information to propagate their desired model of women and influence reproductive choices. She analyzes the role of key professionals, such as demographic experts and, particularly, physicians and other medical personnel, in the making and implementing of reproductive policies. She also considers the role of international actors, including the United States, in perpetuating the Ceausescu regime.

The author sets her discussion of reproductive policies in the context of broader economic and political trends as well as developments in Romania's international relations. The reader is thus able to follow the increasingly repressive measures the Ceausescu regime adopted in its effort to control reproduction, including the outlawing of abortion, contraception, and divorce in the late 1960s, and the gynecological examinations at the workplace that women were forced to undergo to make sure that pregnant women did not abort the fetus. The book also analyzes the factors that led to the explosive spread of AIDS in a society that lacked both the resources and the will to fight its spread and the way in which reproductive policies and efforts to control sexuality in the

communist era contributed to the wrenching plight of the AIDS babies in Romania's orphanages. Kligman also examines the phenomenon of international abortion that developed in Romania after the end of communism and the steps that Romania's postcommunist leaders have taken to deal with this situation. In conclusion, the author discusses the lessons others can learn from Romania's experience, particularly the banning of abortion. Noting that the Romanian effort to control reproduction was an extreme case of what most states do to one extent or another, Kligman argues that access to abortion must be legally protected. She also calls for men to assume greater responsibility for their reproductive choices.

Throughout the book, the author deftly draws connections between official policies and popular responses. She illustrates the link between conceptions of gender roles that define women by their biological functions and policies that ignore women's right to control their own body. She also explores the connection between pronatalist policies and nationalist ideologies. The lucidity of Kligman's analysis of these factors alone would make this book very valuable. What raises it to the level of a work that must be read not only by those interested in reproduction, policies toward the family, or gender issues but also by all who want to understand the communist era is the way Kligman interweaves evidence from ordinary lives into her discussion of official policies.

The incorporation of the lived experience of those who were affected by government policies gives this book an immediacy and authenticity not often found in social science literature. It is impossible to read the accounts of women's experiences with botched illegal abortions, fears of pregnancy, and prosecution by the authorities without being horrified by the effect of the state's intrusion into the most private realms of human experience. These accounts illustrate the negative effects that fear of pregnancy and the potentially dire medical and legal repercussions of efforts to end unwanted pregnancies had on relations within the family. They also document the dilemmas doctors and other medical professionals faced in practicing under such conditions.

Yet, Kligman writes, "although less commonly noted, Ceausescu's Romania was not consistently perceived to be a living hell by all residing there" (p. 40). The regime's attempts to control the population in this area as in others evoked a variety of responses, ranging from complicity with the efforts of the elites to open resistance. The responses of most citizens fell somewhere in between. Kligman provides a nuanced discussion of these responses and of the factors that influenced individual choices and actions. As one physician noted, after talking about the way political authorities sometimes threatened to withhold surgery from women unless they identified the doctor who had performed their abortion: "Yet, not all prosecutors were maniacs or inhuman. There were real people among them, too. They knew full well that they also had wives, daughters, and mothers" (p. 162). By looking beyond official policies to the interaction of these policies and popular responses, this book gives us an extremely insightful look at how communist systems were experienced and how official policies were changed, mitigated, or countered by those who lived under them.

The choices faced by citizens, including those who were most often seen as powerless, in determining their responses to official policies were particularly poignant in the reproductive area, but they also existed in other areas of life. By focusing on what can be seen as an extreme or particularly

sensitive case, Kligman illuminates the dynamic of society-state relations that characterized communist systems. It is hard to think of another book that captures this dynamic and the complexity of the communist experience as well.

Consent, Dissent, and Patriotism. By Margaret Levi. Cambridge: Cambridge University Press, 1997. 215p. $59.95 cloth, $19.95 paper.

Charles Tilly, *Columbia University*

Through an opening epigraph from Norman Maclean, Margaret Levi proposes a vivid analogy between the complexities of conscription and of fire: In both cases many causes intersect to produce particular events, yet the analyst's job is to tease truth from complexity. Seeking to clarify costly political consent in general, Levi astutely analyzes resistance to and compliance with calls to military service, a quintessential case in which individuals face the choice of bearing large costs on behalf of benefits they will share little or not at all and to which their participation will make little difference. In the process, without ever quite saying so, she batters the postulate of universal self-interest that undergirds so much of rational choice argument in political science.

Levi self-consciously builds her analysis on game theory. She thereby commits herself to single actor explanations of social behavior: Individuals make decisions that affect other individuals in response to incentives operating within constraints. She moves beyond bare rational actor formulations, however, in two significant ways. First, she identifies relations with others as significant constraints on individual decision making; second, she sketches histories of the institutions that shape constraints, including relations with others. Repeatedly, as a result, she reaches beyond the self-imposed limits of her models to examine interactive processes, such as continuous bargaining. Concretely, she analyzes situations in which potential soldiers, governmental agents, and other subjects of the same government bargain out consent to military service or resistance to that consent.

Levi begins with the plausible idea that approval of a government's current policies as such promotes compliance with those policies. Analysis of real cases leads her to modify that view, and the resulting argument centers less on approval for particular policies than on generalized confidence in government's efficacy and equity. Levi's model of "contingent consent" states that individuals are more likely to comply with costly demands from their government, including demands for military service, to the degree that (1) citizens perceive the government to be trustworthy, (2) the proportion of other citizens who comply (i.e., the degree of "ethical reciprocity") increases, and (3) citizens receive information confirming governmental trustworthiness and the prevalence of ethical reciprocity.

More loosely, Levi argues that citizens consent to onerous obligations when they see their relations to governmental agents and to other citizens as both reliable and fair. Fairness and justice matter. Levi does not specify what mechanisms produce these effects; she treats them as empirical generalizations to verify or falsify. She implies, however, that the effective mechanisms are individual, that is, individual calculations concerning likely consequences of compliance or resistance. "Contingent consent requires that an individual believe not only that she is obliged to comply but also that others are or should be obliged to comply" (p. 205). Like other rational action theorists, she centers her explanations on cognitive processes.

Counterhypotheses Levi means to refute include (1) habit-

ual obedience, (2) ideological consent, and (3) opportunistic obedience. Each of these identifies a different cognitive orientation of subjects to authorities. Opportunism, as Levi defines it, can respond to a variety of incentives, including secret satisfaction, side benefits, and social security. Her evidence from the United States, Canada, the United Kingdom, France, New Zealand, Australia, and Vietnam documents none of the relevant incentives and cognitive events directly. The evidence concerns differential compliance with demands for military service according to period, population segment, and character of war. She does not analyze determinants of draft evasion or desertion as such, but she does provide hard-won data on overall levels of compliance with calls to arms. Observed differentials challenge habit, ideology, and opportunism accounts and confirm Levi's empirical generalizations summarizing contingent consent: On the whole, compliance with conscription is greater in situations of relatively high trust, and so on. Far from depending on selective incentives to particular segments of the population, military recruitment commands wider assent when governments recruit generally and uniformly in a context of universalistic laws.

Institutions, organizations, and social relations enter Levi's explanations as background variables—not as direct causes of compliance but as shapers of the perceptions and information that themselves explain compliance. Thus, Canada's sharp division between anglophones and francophones helps explain both readiness' of the Anglo majority to impose conscription on the entire country and greater resistance of the French-speaking minority to military service. Institutions, organizations, and social relations also affect available courses of action and their relative costs. Thus, French history, with its long establishment of the nation in arms and its weak development of pacifist sects, makes conscientious objection much less available to draft resisters in France than in Anglo-Saxon countries.

Toward the end of her analysis, Levi offers a larger opening to social structure: She argues that third-party enforcement strongly affects the actual likelihood of other people's compliance, hence any particular individual's perception of fairness. Governmental coercion of potential defectors significantly affects not only those recalcitrants themselves but also others who become more willing to serve when they know that others will have to serve as well. At this point in Levi's analysis, networks of interpersonal commitment start playing a significant and fairly direct part in the generation of social action. Levi offers another opening to social structure by recognizing how significantly governmental performance affects compliance; poorly or erratically performing governments, including the U.S. government during later phases of the Vietnam War, receive less compliance. (This very problem led Albert Hirschman to distinguish exit, voice, and loyalty as alternative responses to governmental failure, but Levi does not follow up that lead.) By this point, interactive processes are doing an important part of Levi's explanatory work.

The move to interaction complicates the story of compliance but brings it closer to fundamental issues of democracy. Democracies gain strength, Levi's analysis implies, by establishing mutually enforceable and general commitments between governmental agents and citizens, on one side, and among citizens, on the other. Any compromise of governmental fairness, inclusiveness, and capacity to meet commitments undermines democratic consent. Levi's lesson goes far beyond military service.

Changing Channels: Television and the Struggle for Power in Russia. Rev. and exp. ed. By Ellen Mickiewicz. Durham, NC: Duke University Press, 1999. 372p. $19.95 paper.

Jeffrey W. Hahn, *Villanova University*

For anyone who watched Soviet television before 1985, the contrast between then and now could not be more striking. As with all Soviet media, television was centrally controlled and used by the Communist Party to communicate only the information deemed necessary by its leaders. The news was sanitized, dreadfully dull, and politically correct. The collapse of communist rule in Russia opened up space for alternative sources of political information to develop. News content today is certainly more lively and attractively packaged, but the emergence of autonomous, multiple sources of information has only partially been realized, as Ellen Mickiewicz makes clear. Furthermore, an unfortunate legacy of the Soviet period remains alive in contemporary Russian practice. It is the principal thesis of this remarkably well-informed and readable book that now, as then, television news is a "zero-sum" game: Whoever controls the news, wins. Consequently, control over television news remains the critical mechanism for gaining and holding political power, and it is the consuming goal of those who would do so.

Except for a short preface, the book under review is a paperback edition of the volume published under the same title in 1997 by Oxford University Press. Indeed, it appears that exactly the same typeface was used for both. The new edition, however, has an "Afterword" that expands the analysis through the financial collapse of 1998.

The author's intention is to "illuminate the critical role television played at key times and with key political actors and institutions" (p. xiii) in Russia. Indeed, the first two-thirds of the book do just this by describing how policies on television coverage changed, both during the *perestroika* of Gorbachev, from 1985 to 1991, and under Yeltsin's leadership, through the 1996 presidential election. The focus is almost exclusively on television news and on the role of the central television stations in Moscow. The style adopted in these chapters is closer to investigative journalism than to academic analysis, but what a remarkably rich and detailed picture we get of the events and players in this period and of how television influenced both. Mickiewicz made good use of her position as director of the Commission on Radio and Television Policy, co-chaired by Jimmy Carter and Eduard Sagaleyev (founder of the first private television station in Russia), to interview virtually all the major political and television decision makers in this period. Her insider access yields fascinating insight into how the battle for control of the "most powerful medium" unfolded. There is nothing in English or in Russian to compare with it.

The remainder of the book contains equally valuable, indeed path-breaking, contributions to our understanding of the role of the media in Russia today, albeit within a more traditionally academic framework of analysis. Particularly noteworthy is the author's exploration in chapter 9 of how controversial issues were presented on television and of public tolerance for diverse viewpoints. Using data from survey research and systematic personal interviews with television executives and the general public conducted in waves between 1989 and 1995, Mickiewicz shows how closely one's tolerance for divergent opinions on controversial issues mirrored the political polarization of society and thereby "reduced the space for a tolerant middle" (p. 212). Also of value are the sections that deal with the effects of privatization on news presentation. One effect was the great enhancement of television as source of news at the expense of the

print media. Chapter 10 and portions of the Afterword are must reading for those who want to understand the struggle for media control currently underway in Russia. The Afterword also offers a new take on the Russian television viewer. Based on focus group data collected in 1998 in cooperation with the Public Opinion Foundation in Moscow, the author concludes that Russians possess extraordinary sophistication in how they process TV news information.

One of the most revealing sections of the book deals with the emergence of the first truly independent, private television station, NTV. The story of NTV demonstrates both the possibilities and the problems confronting television journalism in Russia. Soon after it came into being in 1994, NTV earned widespread public credibility for its coverage of the war in Chechnya. The reporting was balanced, independent, and smart. "NTV got it right" and in doing so "finally spelled the end of the Soviet media system" (p. 224). The backlash from the Yeltsin administration and its media channels was predictably harsh but only enhanced the viewership for NTV. Sadly, this independence was compromised when NTV felt compelled to join the rest of the television media in virtually open support of Yeltsin's reelection in 1996. As Mickiewicz shows, for television journalists, controlling the medium remained a zero-sum game; they could not afford a Ziuganov victory over Yeltsin. They had to take sides.

It is hard to find fault with a book as good as this one, but there are times when the claim for television's power to control events in Russia seems exaggerated. Certainly, political leaders in Russia, as everywhere, seek to manipulate television to get out "their" message, and media in Russia are probably more politicized than elsewhere, but surely control over the broadcasting tower in Ostankino is not the only key to holding power in Russia. Beyond that, one can quibble that the book as a whole does not address larger theoretical issues in the field of comparative politics or, for that matter, in the field of political communication. Fascinating though they are, the chapters in the first two-thirds of the book are atheoretical. Mickiewicz knows the scholarly literature as well as anyone in these fields. Why not use this splendid case study more systematically to illuminate further some of the issues it raises?

The Rational Politician: Exploiting the Media in New Democracies. By Andrew K. Milton. Aldershot, UK: Ashgate, 2000. 195p. $64.95.

Jane L. Curry, *Santa Clara University*

The media have always been major players in transitions from authoritarian rule toward democracy. For all the discussion of the media as actors in these transitions and as the objects of political tug-of-wars, little has actually been said about how the media work or how they have been managed. *The Rational Politician* is the first look at the media in the East European transitions from communism to democracy.

This is not a complete study of the transformation of the media in the process of democratization. It does not look either at what happens inside the media (how journalists and editors act and how their behaviors change) or at how the populations receive and use the media. Instead, Milton examines the elites' battles with and decisions about the media in Poland, Hungary, and Czechoslovakia (and the Czech Republic and Slovakia) to see whether the paths of the transitions are determined by elites' deliberate crafting or by the legacies of the different forms of communism. He finds a remarkable continuity in how the media are actually con-

trolled: Even as systems are attacked and torn down, their opponents discover that the old controls serve their interests.

Based on a detailed review of the history of the media in the three most successful democratizing states in East Europe and of the elites' battles with and treatment of the media after communism ·collapsed in 1989, Milton rather sensibly argues that neither elites nor national history alone determines the fate of the media. Instead, he argues, it is a combination of the two. In focusing on the conflicts and formal relations between the media and politicians and among politicians over the media, Milton leaves out the most ironic influence ·on the politics of the media in postcommunist transitions: Communism may have left a set of institutional controls and ways to manage the media, but many of the leaders of the new media and many of the first postcommunist elites learned their lessons from being members of the anticommunist opposition. What they learned has not made them supportive of a free press. Rather, they see the media as an important instrument for getting and keeping political power. After all, if the opposition could push communism over the edge with underground publications, the logic goes, free media that criticize their own rule could be lethal.

Elites, Milton concludes, want to remain in charge. To that end, just as the communist elites found many of the methods of control used by the interwar regime and then the Nazis worked for them, the new "democratic" elites often use the controls they opposed in the communist· system. Whatever their ideology, their real concern is with protecting their vested interests. In fact, as the media expand coverage and criticism, the elites often respond by imposing controls. They do this, they say, to protect and consolidate stability in the democracies they are building.

Milton brings together the details of how the media were managed in the precommunist, communist, and postcommunist eras. In the process, he provides a much-needed compendium of the crises in media-elite relations and of the legal and institutional regulations that existed before the transitions began in 1989 and during the· first decade of the transitions. That in itself is a real service. These are, after all, four of the most successful democratizers, models for the other later or slower transitions. If elites in these countries try to constrain media freedom, imagine how tempting such constraint is for other, less successful or liberal, "democratizers." Beyond this, Milton lays out clearly and calmly the debate between "transitologists" and those who say history makes the difference. His conclusion is a sensible one: The truth lies somewhere in between.

Moral Purity and Persecution in History. By Barrington Moore, Jr. Princeton, NJ: Princeton University Press, 2000. 158p. $19.95.

Adam B. Seligman, *Boston University*

From time out of mind, people have killed, maimed, and oppressed one another in two sorts of conflicts: over material interests (real estate, slave labor, agricultural surplus, war booty, and so on) and over what Max Weber would have termed "ideal interests" (conflicts over ultimate meanings, salvation, principles of justice, definitions of social order). Barrington Moore's classical and landmark study, *Social Origins of Democracy and Dictatorship: Lord and Peasant in the Making of the Modern World* (1966), explored the first mode of conflict in the making of modern nation-states. All social scientists are forever in his debt for that effort. It ranks

as one of the most important contributions to the fields of comparative and historical sociology.

The present work moves to the second realm, ideal interests, in the sense of the need to maintain boundaries of "moral purity" within collectivities and the rather vicious derivatives of this need in terms of social relations. A concern with moral purity leads to a concomitant concern with "pollution," which the author claims is the cause of much war and persecution. The book explores three cases—Old Testament Judaism (basically, the laws of Leviticus and the injunctions of the later prophets), the Huguenots in France, and Saint-Just and the French Revolution. Through these cases Moore seeks to establish the centrality of monotheism to a concern with moral purity and its lasting influence in defining issues of purity and pollution as loci of conflict, wars, and persecution in Western civilization.

At the end of the study, Moore engages in some brief asides to show how in Hinduism, Confucianism, and Buddhism concern with moral purity never led to the type of persecution that it did in monotheistic civilizations. True, Hinduism oppressed the lower castes, but it did not seek to eliminate them. A wider comparative net, however, drawing in such cases as Japanese civilization or the Zoroastrian, Sassanian Empire (226–652 C.E.), would have led to an appreciation of monotheism as only one among any number of civilizations engaged in such repressive behavior, not unique in itself. Moreover, and as Arnoldo Momigliano (*On Pagans, Jews, and Christians*, 1987) has shown, the "tolerance" of the pagan empire stopped abruptly short in matters touching on political rule or regime loyalty.

Moral purity is a notoriously difficult term, deeply resonant with Christian rather than strictly monotheistic meanings. Indeed, Moore's reading of the ancient Israelite holiness code in its ruling on scale disease (and so also, the case of menstruating women, uterine blood, seminal emissions, contact with the dead) as moral defects, forms of·moral·pollution (p. 20), is manifestly false. Purity· in the Old Testament did not have to do "mainly with sex" (p. 36). Moreover, Jacob Neusner (*The Idea of Purity in Ancient Judaism*, 1973) and others have argued convincingly for the inability to separate issues of ritual from moral purity in ancient Judaism. A specifically moral reading of issues of purity and pollution was not the legacy of early Hebrew monotheism. Indeed, when Moore claims that "astonishingly little" has changed in the "social implications of purity and impurity" from the ancient Israelites to the French Huguenots (p. 56), he glosses over what was perhaps the most significant change of all, that registered in· the seventh chapter of the Gospel of Mark, when Jesus proclaims: "Nothing that goes from outside into a man can defile him . . . . It is what comes out of a man that defiles him." Here is the real origin of a concern with moral purity, rather than the more general and cross-cultural issues of purity and pollution.

What "comes out of a man" relocates issues of purity and impurity from the external world of ritual acts (and states) to the internal realm of intentionality and belief. It was indeed the very focus on belief, on faith, as forgoing communal boundaries and solidarities that played such a salient role in the Protestant Reformation, not least in its Calvinist varieties. It is no doubt true that Calvin, and most especially Calvinists, maintained a strong separation between the churched and the unchurched, between the regenerate saints and unregenerate sinners (in the language of Congregational Puritanism), but it is not at all clear that this is analogous to the terms of moral purity invoked by Moore. It is equally far from sure that the term *vertu* and its corollary, "corruption," both of which were used with such vehemence by the radical

leaders of the French Revolution, can be understood in terms of moral purity and pollution. Indeed, if anything, the corruption of virtue, which was a revolutionary trope from Robespierre to Lenin, points to recognition of agency and intentionality (of inwardly processes and of choice) as critical to politics in a way that a more general concern with collective boundaries and ordering principles, which we find in the politics of purity and pollution, does not convey.

Even in this short book Moore does well at that in which he excels: explaining the structural and material conditions of politics and of elite and popular action. His speculative incursions in these realms, especially with the Huguenots, are convincing. Beyond this, we must note that the symbolic realm, especially that organized in terms of transcendent meanings (not just monotheistic ones), adds an intransigence and grimness to politics that was often lacking in pagan religions. In many ways, this is but the tragic and necessary concomitant of an equally uncompromising (yet salutary) ethics also bequeathed us by religion. This is a point the author does not make.

All collectivities strive to maintain their boundaries—even the West Roxbury Numismatic Society. When these boundaries are seen as coterminous with truth claims, the struggle often becomes brutal. This in itself does not make of every such struggle one over moral purity and pollution.

Organizing Democracy in Eastern Germany: Interest Groups in Post-Communist Society. By Stephen Padgett. Cambridge, UK and New York: Cambridge University Press, 2000. 200p. $57.95 cloth, $21.95 paper.

Christian Soe, *California State University, Long Beach*

Stephen Padgett is highly regarded for his scholarship on parties and other aspects of politics in the Federal Republic of Germany. In the present work he focuses on the "hesitant" emergence of associational activity in the postcommunist societies of eastern and central Europe, with particular emphasis on economic interest groups in eastern Germany. His tightly written book seeks to document and, above all, explain this state of affairs. The retarded growth of interest groups in the region is an important and relatively neglected topic, at both the theoretical and empirical levels aimed for here. It is also a development that has run counter to the once widely expected emergence of a vigorous pluralist civil society that would accompany the region's transition to market economics and democratic politics.

Padgett delivers a series of important new empirical findings on the peculiarities of interest group development in eastern Germany, but the book is primarily marked by a strong theoretical thrust from beginning to end. It draws systematically on associational theory in trying to explain the pattern of group formation and interaction in former communist societies. A long introduction explores several theoretical approaches that are later applied in the analysis of postcommunist interest group formation and activity: pluralism, corporatism, and rational choice theory, along with concepts of social capital and civic culture. Padgett draws insights from each approach but finds the logic of collective action as developed by rational choice theorists particularly useful in illuminating some business, labor, and professional group problems of development.

Chapter 1 discusses the democratic transition and the early setbacks for a pluralist civil society in these countries. It also establishes the general format followed in the rest of the book by first reviewing the topic in its broader central and eastern European context and then focusing on eastern

Germany as the major case study. Padgett underscores the exceptional circumstances of the German situation, above all the wholesale institutional transfer from the "old" western Federal Republic into the "new" eastern states of Länder. Yet, he sees eastern Germany as an advanced case of postcommunist development that may provide us with clues about what to expect elsewhere. He is also careful to emphasize the contextual differences for interest group politics in Poland, the Czech Republic, Russia, and other former communist-ruled societies, even as he makes general observations about their shared experiences.

In the remaining five chapters, the author systematically examines the weak social and economic foundations for new interest groups in eastern and central Europe, their underdeveloped organizational framework, their declining level of membership coverage and participation, the special role played by leaders and professional staff, and the often tenuous relationship among interest groups, the state, and public policy. In seeking to explain these matters, he emphasizes the influence of both the communist legacy and postcommunist transitional problems in the economy and polity. These have inhibited the emergence of the kind of socioeconomic formations that could have spawned autonomous and relatively stable business, professional, and labor associations with a greater and more predictable role in the articulation of interests. He shows that although postcommunist societies in Europe have seen the rise of many economic interest groups, these are relatively weak organizations in which elite domination, membership losses, and mass passivity appear to have become the rule, along with an often marginal role in the public policy process.

In eastern Germany, Padgett finds that the indigenous interest groups have been unable to compete successfully with some better resourced organizations transferred from western Germany. He also documents a general condition of fragmentation and individualization in private economy relations that bodes ill for the rise of any stronger and more stable associational order. For the future he postulates "a pervasive process of social dealignment" (p. 166) and concludes that we should not expect a delayed convergence with the more concentrated patterns in the longer established European democracies. Instead, he sees postmodern tendencies in eastern Germany that may point to a future pattern of interest group dynamics in the "older" parts of the Federal Republic and the rest of Western Europe as these societies become more "fluid" and "atomized."

In addition to the theory-driven discussion of economic interest groups, which form a part of associational life that is often missed in more general reflections on the condition of civil society, Padgett provides new empirical data and insights. In particular, he draws repeatedly and skillfully on his qualitative interviews with around 45 interest group officials in eastern Germany, conducted in 1994–95. They were chosen for having a strategic overview of their group's activity and constituted a relatively broad sample that included business and employer's associations, trade unions, and professional groups. It should be stressed that the book brings the discussion up to the end of the 1990s.

Padgett deals in an impressive manner with a very important and, at least in this form, hitherto relatively neglected subject. His compressed work is defined by its focus on economic interest group dynamics in postcommunist societies and the theoretical frameworks that can help explain them. He also realizes, even if he does not explore at any great length, a deeper significance of his topic. This is apparent early in the book, when he approvingly applies another author's evocative reference to "the strange death of

civil society" to much of eastern and central Europe. This valuable book will inform future discussions of these subjects. One hopes for a companion study of the current state of party politics in eastern Germany, where at least some of the findings might modify Padgett's conclusions.

States, Ideologies, and Social Revolutions: A Comparative Analysis of Iran, Nicaragua, and the Philippines. By Misagh Parsa. Cambridge: Cambridge University Press, 2000. 326p. $54.95 cloth, $19.95 paper.

Jack A. Goldstone, *University of California, Davis*

This intriguing volume is a direct challenge to Theda Skocpol's *States and Social Revolutions* (1979); by inserting "ideology" into the title, Parsa claims that Skocpol left out something important. He makes good on his effort to demonstrate the importance of ideology in recent Third World revolutions, but the book offers far more than that.

Several authors have compared the Iranian and Nicaraguan revolutions, which occurred in 1979, but to my knowledge this is the first book-length treatment to add the Philippines revolution against Ferdinand Marcos in 1986. The comparison is especially useful because the mass-mobilizing radical revolutionary movement in the Philippines—the communist New People's Army (NPA)—failed, even though a reformist urban revolution toppled the old regime. In contrast, in Iran and Nicaragua, the middle classes were at first overwhelmed by the more radical mass movements led by the Iranian clergy and the Sandinistas. Why these radical revolutions succeeded and the Philippine events left class structures largely unchanged is one of the puzzles that Parsa seeks to resolve.

The author begins by stating that structural factors weakened all the ruling regimes, setting the stage for revolutionary conflicts. These factors included an increasingly centralized, exclusionary, and interventionist role of the regime in the economy; periods of economic difficulty or crisis; and international pressures. The characteristics of the regimes were such that they received the blame for economic problems but provided no legitimate means for those outside the regime to correct them. Foreign pressure regarding human rights issues from the United States, hitherto a major supporter of these regimes, led to reduced repression of the opposition. Together, these conditions left the dictatorial government vulnerable to concerted attacks.

Parsa goes much farther and uses his comparisons to show that one cannot understand the course of these revolutions without closely examining the leadership, organization, participants, and ideology of the opposition, and how these interacted with the actions of the old regime. Based merely on organizational strength, the Philippine NPA, which had tens of thousands of armed and organized supporters in the countryside and the cities, should have dominated the revolutionary process. Indeed, if Marcos had shut off all peaceful means to reform, as did the shah in Iran and Somoza in Nicaragua, then the middle-class opposition might have been forced into an alliance with the NPA in order to change the regime. In such a coalition, the NPA probably would have dominated due to its popular support and organizational strength.

Instead, Marcos sought to outflank opponents and regain U.S. approval by calling a snap election, and the moderate opposition organized urban workers and rural farmers on a platform of peaceful electoral change. The NPA reacted by boycotting the election and refused to ally with the moderates, with the result that it was reduced to a bystander in subsequent events. When Marcos lost the election and tried to overturn the results, a portion of the army defected, forcing him out of power. The middle-class moderates were credited with the overthrow, and popular support shifted away from the NPA.

In contrast, in both Iran and Nicaragua the mass-mobilizing radicals initially played the role of moderate and subservient allies in an antiregime coalition with the reformist bureaucrats and bourgeoisie. The Sandinistas and Khomeini gathered broadly based followings and wholly isolated the regime and its small circle of supporters. Once in power they adopted a more radical line and distanced themselves from their erstwhile middle-class allies. In sum, variations in the use of ideology and in leadership played a crucial role in steering these revolutionary episodes on distinct paths.

It is one pleasure of this book that, rather than present a thin account of the cases tailored to his theory, Parsa provides unusually rich and detailed information, sufficient for readers to weigh his arguments against the extensive evidence. This very readable volume also casts welcome light on Cardinal Sin and the role of the church in the later stages of the anti-Marcos movement. Moreover, there is an arresting section on the importance of university students to revolution in Third World countries, where their numbers and prestige have been expanding far more rapidly than the number of professional jobs to which they aspire.

Parsa offers a fine combination of careful, nuanced empirical case studies and theoretical propositions regarding key factors omitted from structural theories of revolution. Along with other recent work influenced by the "new institutionalism," this volume points the way toward a new and richer synthesis of structure and agency in our understanding of revolutions and revolutionary processes.

Organizing Women in Contemporary Russia: Engendering Transition. By Valerie Sperling. Cambridge: Cambridge University Press, 1999. 303p. $59.95 cloth, $22.95 paper.

Rebecca Kay, *University of Glasgow*

In the introduction, Valerie Sperling notes that "Russia's transition from communism toward capitalism and a more democratic political arrangement has been both good and bad for women, presenting both obstacles and opportunities for organizing" (p. 7). She goes on to produce an engaging and thought-provoking analysis aimed at broadening the scope and explanatory power of social movement theory, which, she argues, has been developed by scholars who focus primarily on social movements in the "contemporary core democracies" (pp. 52–3). In contrast Sperling develops "a cross-cultural model of social movement organizing and development that explores five interrelated opportunity structures: socio-cultural or attitudinal, political, economic, political-historical, and international" (p. 53). In each subsequent chapter she tackles one of these opportunity structures and offers a number of fascinating insights into the world of post-Soviet social movements, based on the experience of her sample of women's organizations.

The book demonstrates a genuine concern to highlight the importance of a broader and more inclusive understanding of social movement organizing and practice. Sperling places much emphasis on the importance of cultural heritage and political history as well as contemporary social and political contexts in order to understand the different approaches and tactics employed by Russian women's organizations in the post-Soviet 1990s. The penultimate chapter presents a critical discussion of the effect of international influence; the poten-

tially negative side-effects of foreign aid, such as increased divisiveness and competition between local organizations, and the power invested in sponsors to set agendas and priorities that may not have the most resonance and value for the indigenous population.

Yet, this perspective notwithstanding, the analysis and the conclusions drawn from it come perilously close to essentializing Russian experience and practice at various junctures, primarily due to an idealization of and dangerously uncritical assumptions about Western social movement practices and experiences. In discussing Russian organizations' fund-raising strategies, membership outreach activities (or lack of them), political engagement, and attempts to influence the state, Sperling appears to assume a level of sophistication, development, cohesion, and organizational size and structure in Western social movements that may be far from the norm.

Certainly, the issues that influence membership numbers, the struggle for financial survival, or an aversion to formal political activity among many new social movements may vary cross-culturally. Nonetheless, a failure to point out the fallibility and instability of many Western organizations produces an aura of Western superiority and a presumption that following the lead of Western social movement activities will be the quickest and safest way to achieve democratic civil society, social justice, and equality in post-Soviet Russia.

Similarly, the lack of any detailed discussion of the deep ideological splits within Western feminism and the difficulties faced by Western women's organizations over the past decade in retaining credibility and sympathy, particularly among young women, produces the impression that the skepticism and hostility with which Russian women's organizations frequently must content are purely a product of Soviet social and political history, and they will be best overcome by adopting ideological stances, organizational structures, and political tactics most easily recognizable as "feminist" from a Western perspective. As a result, little space is allowed for the nascent Russian women's movement to develop its own and potentially very different approaches and priorities.

Sperling's analysis is based primarily on interviews with the leaders of fifty women's organizations; thirty-seven are based in Moscow, five each in Ivanovo and Cheboksary, and three in Ekaterinburg. The decision to interview only the leaders is justified on the basis that "only [they] had a grasp on the information I sought, whether it was about the organization's history, its membership, dues policies, or sometimes even its goals and activities" (p. 12). Yet, in attributing this authoritative knowledge to leaders only, Sperling loses the different perspectives and attitudes that might have been found among the rank-and-life. Her statement itself implies that members of the same organization may view its goals and activities differently; similarly, their interpretation of social and political trends as sources of opportunity or hindrance to the organization may vary.

The sample's quantitative bias toward Moscow-based respondents appears to be reinforced in the text by a tendency to attribute more value and general explanatory power to the statements of these women. For example, when discussing the importance of Western influence, Sperling notes a marked difference between the testimonies of Moscow activists and those from the provinces. Although nearly half of those in Moscow mentioned some connection to the West as instrumental in their path to activism, none of the provincial respondents mentioned such influences (p. 222). Nevertheless, having made this observation, Sperling proceeds to describe Western influence as crucial to the development of a Russian women's movement.

Sperling recognizes that there is a broad spectrum of women's groups and organizations, and she provides a convincing critique of "an inherent bias" in the theoretical division of groups into "practical" and "strategic," which tends to privilege "strategic" groups as the ones that are necessarily most "feminist" (p. 28). Nevertheless, she repeatedly refers to the relatively Westernized and often overtly feminist organizations clustered around the Independent Women's Forum as "the Russian women's movement," describing the Moscow Center for Gender Studies as "the most widely known feminist organization in Russia" (p. 249). These groups are certainly highly significant, and Sperling describes the crucially important work in which they are engaged: lobbying parliamentarians and ministries, attempting to bring issues of discrimination and inequality to light, and struggling to ensure that the Russian state is held to account as a signatory of numerous international treaties and domestic legislative acts guaranteeing equality for women and outlawing discrimination on the grounds of sex. Certainly, these are also the most well known of Russian women's organizations from a Western perspective, but whether they can, or should, be given the title of "the Russian Women's Movement," especially if this (unintentionally perhaps) implies that organizations with differing perspectives and priorities are ignored, is a moot point.

This is a well-written and carefully researched account of the activities of a particular set of Russian women's organizations in the 1990s. Sperling's lively and accessible style is successfully interwoven with interview excerpts to convey brilliantly the vitality of these organizations and the trials and tribulations faced by social movements in contemporary Russia. The book will certainly be of great interest to those concerned with the development of civil society and social movements in post-Soviet Russia as well as to those engaged with international women's studies and the study of post-Soviet society.

Women and Politics in Uganda. By Aili Mari Tripp. Madison: University of Wisconsin Press, 2000. 277p. $55.00 cloth, $25.95 paper.

Ronald Kassimir, *Social Science Research Council*

With women holding 18% of the seats in parliament and a woman serving as a vice-president, the status of women at the apex of Uganda's political system is impressive compared to most other countries, including the United States. In noting this surprising fact and in chronicling how it came about, Aili Mari Tripp has written a thought-provoking book that raises serious questions about what it means. She draws on empirical research in the realms of both "high" politics (i.e., the halls of parliament) and "deep" politics (urban working-class neighborhoods and rural villages) and provides a rich account of Ugandan women's associational life and political mobilization.

Beyond the numbers of women who hold high-level political office, Ugandan women are more broadly attempting to effect thoroughgoing transformations in power relations by pushing new issues onto the political agenda and critiquing the everyday practice of politics in still male-dominated social and political institutions (most prominently via patronage). Although their successes have been limited, politically engaged Ugandan women, Tripp argues, have put formerly "private" matters into the public arena in enduring ways. She maintains that the autonomy of women's associations—from local structures of power, external donors, and especially the state—holds the key to explaining the progress Ugandan

women have made. The catalyst for this progress has been the National Resistance Movement (NRM) regime led by President Yoweri Museveni, which came to power in 1986 after a five-year guerilla struggle.

Following an historical chapter on the development of women's organizations, Tripp concentrates on the continuities and changes in women's politics during the NRM's control of the state. The rise of a new regime changed what she calls the political opportunity structure for many forms of social mobilization, and Ugandan women took much advantage of the situation. The NRM kept a tight lid on political parties and instituted a registration process for nongovernmental organizations (NGOs), but it greatly opened up political space through a host of liberalizing reforms, albeit with some limits and exceptions. The regime reserved seats for women in parliament and in local councils, enacted affirmative action policies for women at the national university, and enforced strict discipline (including capital punishment) for rape committed by soldiers.

Tripp's account of how Ugandan women made the most of this new context is based on diverse data sources, including surveys, historical research, and extensive interviews. The latter include some leading female politicians and public figures as well as poor working women who throw themselves into grassroots organizing while still struggling to make ends meet. Tripp draws the connections between these levels and the issues—both mundane and provocative—that motivate women to enter the political arena. (Regarding rape, Tripp quotes the leader of one of Uganda's prominent women's NGOs: "Men are in possession of a potentially deadly instrument which should be cut off unless it is properly used, p. 79.")

Tripp is especially adept in explaining how women both see politics as it has been practiced in Uganda as a male-controlled "dirty game" yet use this antipolitics discourse strategically to thrust new issues (from land rights and domestic violence to education and participation) into the public sphere through formally apolitical, multipurpose associations. Interestingly, Tripp presents evidence that this antipolitical stance is connected to women's strong support (as compared to men) for President Museveni's goal of maintaining a "no-party" system rather than returning Uganda to multiparty politics. One wishes that Tripp had explored further whether women's interest in democratizing Ugandan politics, on the one hand, and the opposition of many (but not all) to a multiparty system, on the other, are contradictory impulses and, if so, how women reconcile them.

The core empirical chapters begin with a highly informative discussion of women's roles in parliament, new government ministries devoted to women's issues and livelihoods, and critical legislative and constitutional changes sought by women's groups. This is followed by case studies of women's attempts to control important local institutions (a health clinic and a market) that powerfully shape their lives; efforts to block a World Bank funded housing project in an urban neighborhood and to redirect those funds toward community interests; and the struggle to hold accountable a leader of a rural NGO who played a gate-keeping role to external donor assistance. In most of these cases, Tripp finds that the greatest obstacles for women's groups are local male leaders rather than state institutions (although the latter often acquiesce in allowing local councilors and elders to obstruct women's efforts).

Throughout the book, Tripp emphasizes women's agency and the ways women use the autonomy afforded them by their marginalization. She argues that through their organizational autonomy Ugandan women "are successfully under-

mining the institutional bases of female subordination. They have taken institutional structures that constrain their mobilization and have creatively and actively turned them around in an attempt to expand their choices, opportunities and control" (p. 91).

Given the complexities of the cases discussed, and their ambiguous and sometimes contradictory outcomes, the author's singular emphasis on the autonomy of women's groups is sometimes difficult to understand. Tripp is clearly aware of other factors (including political opportunity structures and the availability of foreign assistance; see chap. 5), but she returns to autonomy throughout the case studies, often without evaluating the comparative weight of other factors. Indeed, in some parts of the book, autonomy appears as the necessary condition for the success of Ugandan women's organizations, and in other parts it seems to be one key measure of that success.

It would have been helpful if the notion of "success" had itself been disaggregated. The book discusses three types of outcomes: the ability of women's groups to (1) politicize issues relevant to women, (2) achieve policy and other goals, and (3) effect changes in the everyday practice of patronage politics. The autonomy argument seems most persuasive in terms of the first kind of outcome. Tripp demonstrates how the ability of women's groups to set their own agenda enables them to force issues into the political arena that otherwise would be ignored. But the second outcome, accomplishing objectives, is contingent on the capacities of organizations as much as their autonomy.

In three of the four cases (the health clinic, the market, and the rural NGO), the groups failed to achieve their desired end; in the fourth (the housing project) the result was mixed—the World Bank withdrew its funding for a project that community groups opposed, but an alternate plan was not adopted. Tripp may be correct that women's marginalization from formal political networks and institutions helps shield their agendas from outside infiltration, but that same marginalization also seems to limit organizational capacity, which includes relationships to powerful others.

Changing the "political culture" of patronage politics is a long-term goal that one cannot expect to achieve overnight via women's emergence into the public sphere. Indeed, the case studies provide examples of (often successful) male resistance at the local level due to the cultural biases against women as political actors and the effect of patronage ties. But again, the question of the importance of autonomy can be raised. Tripp shows that most women's groups include members from different religious and ethnic identities, and she plausibly argues that this is evidence of autonomy—although more from dominant religious and ethnic institutions than from the state. It is unclear, however, how autonomy strengthens the possibilities for challenging political mobilization along "ascriptive" lines if it does not translate into the ability to build new kinds of coalitions around alternate models of politics.

These analytical points can be debated, but *Women and Politics in Uganda* achieves many of its goals. Because it makes the voices of Ugandan women come to life, the book is of interest to those concerned with the gender dimension of politics and the recent dramatic shifts in African political terrain. Also, scholars committed to understanding how the strength and limits of civil associations contribute to political change will learn much from this book. It is hoped that Tripp's meticulous research and empathetic approach will be emulated in future studies of women and other marginalized groups.

The Friendly Liquidation of the Past: The Politics of Diversity in Latin America. By Donna Lee Van Cott. Pittsburgh: University of Pittsburgh Press, 2000. 328p. $50.00 cloth, $24.95 paper.

Daniel L. Premo, *Washington College*

This ambitious new book is a valuable contribution to a growing literature that assumes political democracy cannot be fully achieved in Latin America without recognizing and acting on the region's ethnic and cultural diversity. Van Cott explores the link between ethnic politics, particularly the demands of indigenous peoples, and the constitutional reforms that have occurred in various countries in Latin America over the past decade. Relying primarily on comprehensive analyses of constitutional reforms in Colombia (1991) and Bolivia (1994), she develops the case for what she terms a new "multicultural" model for the region.

The initial chapter seeks to establish the work's relevance to social movement "theory," especially as it relates to democratization and constitutional transformation. Each case study consists of three chapters. The first provides an historical context for the country's constitutional reform, with particular emphasis on the emergence of ethnic movements. The second analyzes the constitutional process and substantive reforms adopted, especially as they relate to ethnic recognition and rights. The third examines critically each country's efforts at implementation, again with emphasis on state-indigenous relations. The author then assesses the effect of the reforms and prospects for constitutional transformation and concludes the study with a comparative analysis of "multicultural constitutionalism" for the region as a whole. The book is meticulously documented and enriched by the author's familiarity with the work of leading Colombian and Bolivian scholars, as well as the insights derived from more than 100 interviews with key players from both countries. Andean scholars will find it especially appealing.

Van Cott frames succinctly the fundamental dilemma represented by indigenous movements' demands for territorial autonomy—central to their efforts to build multiethnic, pluricultural states—and the resistance of traditional elites and private interests, who view such demands as a threat to national sovereignty. Predictably, indigenous movements throughout the region have had the least success in securing constitutional recognition for autonomous regimes. Not only do such claims "become more difficult to operationalize as indigenous populations become more urbanized" (p. 274), but also indigenous organizations themselves "are split on whether autonomy should be exercised at the community (submunicipal) level, the municipal level, or across more extensive multiethnic regions" (p. 275).

It is indisputable that constitutional reforms of importance to indigenous and other marginalized groups have occurred throughout Latin America during the 1990s. Yet, the author fails to demonstrate convincingly that such reforms have moved sufficiently beyond the merely rhetorical or symbolic realm to constitute a "genuinely multicultural model of constitutional reform" for the region (p. 265). Ironically, her study provides considerable evidence that weighs against such a conclusion by documenting many of the institutional and internal restraints that continue to weaken indigenous movements and impede the attainment of their goals.

In the case of Colombia, for example, Van Cott notes that indigenous aspirations that conflicted with the goals of political elites in the National Constituent Assembly (ANC) were "either excluded or phrased in ambiguous language," and specifics were left to statutory legislation (p. 88). In other examples, she reveals how indigenous leaders at all levels have proven just as suscep-

tible to internal dissension (p. 94), cooptation, and corruption (p. 110) as members of the political elite.

In the case of Bolivia, "the country's corrupt and personalistic political party culture has permeated indigenous peoples' organizations, generating divisions and diminishing the authority of traditional ethnic authorities" (p. 186). Moreover, the participation of indigenous leaders in partisan electoral politics has cost the movement "critical political and financial support of domestic and international agencies" (p. 216).

Although both countries' constitutions establish new rights for indigenous representatives to take part in decision making, the ability to exercise such rights continues to depend on the discretion of state, international, and nongovernmental officials (pp. 177–8). Or it may depend merely on the failure of principals to agree on the definition of such crucial terms as "participation" and "consultation" (p. 238). Van Cott documents superbly the tenuous nature of official support for the implementation of indigenous rights in her analysis of the Samper and Banzer regimes, which succeeded the original reformist administrations in their respective countries. In Colombia, the Samper government "failed to advance the constitutional agenda of ethnic rights and, in some respects, reversed the gains of the Gaviria administration" (p. 102). In Bolivia, the Banzer government's discourse on indigenous peoples "shifted from an emphasis on multiculturalism, participation, and human development to an emphasis almost exclusively on reducing poverty" (p. 214).

Despite such discouraging evidence, Van Cott predicts that the end result of constitutional reform in Colombia and Bolivia will be "a transformation of both countries' exclusionary states and regimes" (p. 223), leading to broader political participation and representation. In a later passage, however, she qualifies her optimism by acknowledging that "the greatest achievement" of ethnic groups in both countries has been limited largely to "symbolic rights" in the form of "state recognition of indigenous customs, traditions, authorities, and forms of political organization" (p. 238).

Van Cott assumes implicitly that the indigenous rights movement has been more successful than transnational movements that concentrate on human rights, women's rights, the environment, or class-based issues. Yet, her analysis largely overlooks the conflicting goals and exclusionary tendencies exhibited by such movements, especially between ethnic and gender groups, whose different agendas and perceptions of equality may impede the formation of a broader alliance in pursuit of such common goals as broader representation and participation. Democratic theorists, as well as political elites sympathetic to multicultural and multiethnic democracy, might plausibly argue that economic and political efficiency ultimately depends on the creation of strong institutions for the *aggregation* of interests as well as their representation.

Constitutional reform in many countries of the region has undeniably led to the formal recognition of indigenous peoples' rights and calls for state institutions to ensure their broader participation and representation at all levels. Nevertheless, the future of cultural nationalism remains uncertain. This would seem especially true in countries where indigenous groups represent only a fraction of the population, including Colombia, where Indians represent less than 3% of the population (p. 44). Van Cott demonstrates conclusively that indigenous people have acquired more political influence over the past decade in Colombia and Bolivia, but her analyses of the Samper and Banzer administrations underscore how difficult it is to overcome the region's legacy of exclusionary politics.

In view of recent events in both countries, especially

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Colombia, Van Cott is overly optimistic in suggesting that the new multicultural constitutions in the region have established "a framework for a permanent intercultural dialogue, or a continuing process of mutual adjustment" (p. 279). Few will dispute her claim that "symbolic recognition" of indigenous rights "is an important achievement" (p. 269). It represents a major change in the status of Latin America's indigenous peoples. For the first time, indigenous groups in many

countries have a constitutional base from which to combat the assumptions of intellectuals such as Mario Vargas Llosa, who not that long ago embraced assimilation as the "solution" to the region's indigenous "problem." Ultimately, however, as Van Cott concludes, indigenous peoples themselves "will have to decide whether being an integral part of the state and the wider political community is worth compromising cultural and political autonomy" (p. 279).

INTERNATIONAL RELATIONS

To Agree or Not to Agree: Leadership, Bargaining, and Arms Control. By Lisa A. Baglione. Ann Arbor: University of Michigan Press, 1999. 215p. $49.50.

The Politics of Verification. By Nancy W. Gallagher. Baltimore, MD: Johns Hopkins University Press, 1999. 311p. $45.00.

Jeffrey W. Knopf, *Naval Postgraduate School*

When the Soviet Union disappeared, so did much of academic political science's interest in nuclear arms control. This is unfortunate. First, the risk of nuclear war has not gone away and may be increasing because of nuclear proliferation. Second, U.S.-Soviet cooperation on arms control was in many ways unexpected, and it remains a useful laboratory for testing international relations theory.

These two books are welcome exceptions to the recent scholarly inattention to arms control. Both focus on the role of a particular variable in explaining success and failure in this area. Baglione examines leadership, and Gallagher traces the effect of debates about verification. By analyzing specific inadequately studied variables in depth, both books make useful contributions. Gallagher's analysis of verification politics is especially original and has relevance for many issues beyond arms control.

Both authors employ analytical frameworks that follow Robert Putnam's two-level game approach ("Diplomacy and Domestic Politics: The Logic of Two-Level Games," *International Organization* 42 [Summer 1988]: 427–60). Baglione describes her approach (pp. xv, 3) as derived primarily from bargaining theory and social constructivism, but it most closely resembles two-level games. Her model starts from recognition that state leaders are uniquely situated at the intersection of international and domestic bargaining games, and it thus places those leaders and their bargaining strategies "at the center" of the analysis (p. 3; see also pp. xxi, 18).

Gallagher is explicit about using two-level games as her starting point (pp. 20, 38–42). She places the greatest emphasis on processes of coalition formation, as in "second image reversed" approaches (Peter Gourevitch, "The Second Image Reversed," *International Organization* 32 [Autumn 1978]: 881–911). Gallagher also stresses the role of ideational factors. Because both authors are concerned with domestic-international interaction, they examine the internal decision making of the United States and the Soviet Union, in contrast to the majority of studies, which delve in detail only into the policymaking process in one country (both rely, however, only on English-language sources for their information about Soviet/Russian policymaking).

Baglione follows a largely positivist approach, whereas Gallagher is closer to the interpretive tradition of "thick description." Baglione's model involves two key variables: the

two leaders' preferred strategies and their domestic autonomy to pursue them. When the two sides implement strategies that involve seeking a negotiated outcome and these strategies are compatible (e.g., they do not both seek to achieve relative gains), an agreement is likely; otherwise, it is not.

Baglione applies this model to negotiations on a nuclear test ban in the late 1950s and early 1960s (a period Gallagher also examines), negotiations on intermediate-range nuclear forces (INF) from 1981 to 1987, and the conclusion of the first strategic arms reduction treaty (START I) and subsequent adoption of reciprocal unilateral measures in 1991. Baglione argues that her leadership model successfully explains both stalemate and cooperation in every case, and it does so better than two alternative approaches, based on realism and bureaucratic politics.

The most valuable contribution of *To Agree or Not to Agree* is simply to call our attention back to the importance of leadership. Journalistic accounts and popular histories of arms control usually focus heavily on the U.S. president and Soviet general secretary. But the leading theories of cooperation and grand strategy in international relations emphasize the international or domestic system, or the role of culture or organizations, thereby neglecting the role of individual leaders. Baglione's study is a worthy attempt to correct this imbalance.

Baglione also seeks to develop testable, potentially generalizable propositions that suggest some potentially useful directions for future research. For example, she identifies two sets of variables that she uses to predict each leader's preferred strategy (pp. 7–10) and level of autonomy (pp. 11–5), respectively. The two-level games literature stresses the role of domestic constraints and leaders' strategies, but it usually treats these as exogenous. Baglione's study suggests it would be worthwhile to make further efforts to explain these variables.

As an explanation of arms control, however, Baglione's leadership model has serious limitations. The author tends to impose her theory on the case studies, and she does not adequately recognize the role of domestic and transnational actors. In her initial summary of her model, she contends that arms control is most likely when leaders have moderate to high autonomy from domestic constraints and value arms control highly (pp. xxiii–xxiv, 18–9). Yet, several of her cases do not fit this model, which leads her to make ad hoc adjustments.

For example, in her discussion of the 1980s, Baglione argues (chaps. 4–5) that President Reagan got serious about INF talks and moved to conclude the INF Treaty largely in response to pressures from the nuclear freeze movement, congressional Democrats, and his decline in the polls after Iran-Contra. This is an important part of the story, but it forces her to modify her initial hypothesis and conclude that a leader's loss of autonomy can also be conducive to arms

control (p. 138). If domestic constraints explain cooperation, however, it seems a misnomer to describe this as support for a "leadership" theory. Moreover, on INF (as opposed to START), domestic pressures mattered less than pressures from Western Europe, which was the destination for U.S. INF deployments (see my "Beyond Two-Level Games," *International Organization* 47 [Autumn 1993]: 599–628). Baglione, reflecting a common problem with two-level game analyses, gives insufficient credit to transnational relations in this case and several others. This omission is most serious in her discussion of Mikhail Gorbachev. Baglione rightly credits his "new thinking" with transforming the prospects for arms control, but she never notes that transnational advocacy networks supplied many of the ideas that informed the new thinking (see Matthew Evangelista, *Unarmed Forces*, 1999).

Baglione also distorts history in some of her case studies. For example, in discussing the late 1950s (chap. 2), she claims the Soviets were only interested in a test ban treaty under which they could cheat (pp. 29, 42, 56). But many scientists at the time expected seismic monitoring to improve to the point at which it would be hard to get away with underground testing undetected (an expectation that Gallagher demonstrates was entirely correct), and Baglione presents no evidence that Khrushchev believed otherwise. Moreover, if the Soviets sought to test freely after the United States stopped, why did they not cheat on a testing moratorium that was initiated when the test ban talks got under way? Although they joined the moratorium three days late, there is no evidence that, once they joined, they conducted any clandestine tests. Even though there are flaws in Baglione's interpretations of history, which suggest that her specific model should not serve as the final word on this topic, her intuition that the literature needs a systematic way to talk about the effect of leadership is correct and important.

The Politics of Verification examines the history of the nuclear test ban issue from shortly after World War II until the signing of the Comprehensive Test Ban Treaty (CTBT) in 1996. Gallagher relates progress and setbacks in efforts to achieve a test ban to debates about verification and their effect on the formation of potential winning or blocking coalitions. Her approach starts from two insights. First, verification is never a purely technical question but always involves politics. Second, verification debates are unlikely ever to reach consensus, because opinions about verification are shaped by underlying philosophical principles that differ for different actors.

Gallagher identifies three "ideal type" perspectives: arms control advocates, cautious cooperators, and unilateralists. These, respectively, reflect Kantian, Grotian, and Hobbesian assumptions about international relations (pp. 3–14). Because the "swing voters" in this constellation, cautious cooperators, are highly attentive to verification issues, and because the other two groups need to bring cautious cooperators into their coalition to prevail, arguments about the adequacy of verification have great weight. Many earlier studies assume that other factors basically determine positions on verification, so that verification debates are only a sideshow (pp. 17–8). Gallagher persuasively refutes this presumption, arguing "verification is not just an epiphenomenon. The ideas and strategies that have dominated the politics of verification have had an independent effect on arms control decisions" (pp. 24–5).

Making verification the primary lens for understanding test ban debates works better in some cases than others, and the case study chapters generally have to introduce other factors to explain outcomes. In particular, domestic pressures to be tougher on the Soviets play a central role in Gallagher's discussions of Kennedy and Carter. Still, the case studies are basically sound historically, and they offer several important historical insights. For example, popular mythology identifies the 1963 Limited Test Ban Treaty (which banned atmospheric but not underground testing) as the greatest missed opportunity for a CTBT. Gallagher argues persuasively, however, that the real missed opportunity came three years earlier, when the U-2 incident prevented a possible agreement on a threshold test ban (which also would have banned underground tests above a certain size) (pp. 92, 115–7). Moreover, Gallagher points out that the U-2 shootdown played directly into Soviet fears about U.S. verification demands "by substantiating claims that the West was only interested in espionage" (p. 113). Elsewhere, Gallagher presents some disturbing evidence that officials in the Nixon and Reagan administrations deliberately sought to suppress scientific research that showed detection of underground tests to be easier than official policy acknowledged (pp. 156–7, 188–9).

Gallagher's framework best proves its value in the post–Cold War period. Most theorists expected improved political relations to make cooperation easier, but Gallagher shows that the same verification dilemmas that existed in the Cold War still operate today. Her examination of multilateral CTBT negotiations in 1994–96 (pp. 226–40) confirms her point that earlier verification disputes were not just a by-product of U.S.-Soviet tensions but reflected a more general, independent politics of verification. Indeed, although her study was completed before the U.S. Senate voted on the CTBT, it accurately anticipates many of the reasons the Senate stunned observers in October 1999 by voting against ratification.

Gallagher could have made her conclusions stronger by explicitly drawing out the implications of her study for other literature in international relations. If debates over verification details are equally important in explaining success and failure in other areas in which states seek cooperation, this would challenge both the neorealist emphasis on relative gains (apart from those that might arise from cheating) and the neoliberal assumption that states will turn to international institutions to supply the information they seek to monitor agreements.

Finally, both books are noteworthy for their attentiveness to the different and sometimes surprising ways that domestic-international interaction can affect outcomes. Baglione notes that domestic developments that affect a leader's autonomy also sometimes influence the other side's image of the first state; conversely, foreign policy behavior of one side, in addition to influencing the other side's assessment of that state, also sometimes affects the autonomy of the other side's leader (p. 140). Gallagher notes a number of instances in which U.S. policymakers failed to recognize internal Soviet splits and so made verification demands that undermined the position of moderates (p. 223). She also points out that attempts to use verification strategically sometimes backfired, on both advocates and opponents of arms control, which leads her to caution against making insincere verification arguments (p. 242). Owing to their insightful observations about two-level dynamics, both *To Agree or Not to Agree* and *The Politics of Verification* are useful additions not just to the literature on arms control but also to our understanding of domestic-international interaction.

People and Parliament in the European Union: Participation, Democracy, Legitimacy. By Jean Blondel, Richard Sinnott, and Palle Svensson. Oxford: Clarendon, 1998. 287p. $70.00.

Andrew Moravcsik, *Harvard University*

This book seeks to explain why participation rates in elections to the European Parliament (EP) are low and declining. Turnout has fallen from 66% in 1979 to 59% in 1994—20–40% below levels in national elections. This is a critical issue, the authors insist, because low turnout confirms pervasive doubts that the European Union (EU) is "democratically legitimate." Behind this lies the central question of this era in modern European history, namely, whether a transnational polity with a common discourse and shared institutions can form.

What exactly accounts for such low turnout? The authors approach this question by analyzing cross-regional variation in turnout in light of Eurobarometer data about public attitudes. Leaving aside generic concerns about Eurobarometer data, the careful analysis generates many details of interest to any student of European integration, international cooperation, and modern democratic politics in a globalizing world. I will focus on the major findings concerning the causes of low turnout.

In contrast to what one might suppose from the press, European citizens tend to view the European political process as legitimate. They clearly distinguish between international issues (e.g., drugs, trade, foreign policy, and currency policy) and domestic issues (e.g., health/safety, education, and culture), and they consider EU activity as legitimate in the former cases but less so in the latter.

The authors find similar disconfirmation for the "second-order" theory of turnout, namely, that electoral participation is low because potential voters perceive that "less is at stake" than in national elections. This appears to be untrue, at least in the very limited sense that Europeans do not perceive their votes to be inefficacious. More precisely, the EP is considered a less powerful body than national parliaments, but the strength of such opinions is uncorrelated with participation. Also, the party choices at the European level are not viewed as denying in any way an effective choice among policies. In sum, Europeans do not, according to the data, perceive a "democratic deficit."

So what is at the heart of low turnout? The results of this study support only modest and somewhat speculative positive conclusions about the reasons. A portion of the abstention is explained by institutional factors: the legacy of compulsory voting, the timing of elections, and the generally higher level of participation in proportional representation systems. Low levels of information and knowledge are also related to low turnout.

Yet, the most important determinants of variation in participation appear to lie in attitudes toward Europe. The major impediment appears to be widespread apathy among European voters. Yet, this is offset in countries with a political culture generally favorable to European integration (e.g., Belgium, Greece, Italy, and Luxembourg), where turnout is high. Turnout is lowest, somewhat paradoxically, in countries (e.g., Britain, the Netherlands, and Portugal) with a legacy of intense controversy over European integration.

Even this set of considerations fails to explain either the bulk of the variation in or the overall low level of turnout. At the end of the book we are left with an even deeper puzzle: What influences general voter attitudes toward Europe? We do not know. Given the agnostic analysis, the policy implications can only be somewhat sketchy and pessimistic. The

authors recommend institutional changes to render voting less costly, such as Sunday elections, easier registration, and concurrent elections for national and European positions. These might somewhat increase turnout, but they would be unlikely either to increase it to levels prevailing in national elections or to resolve the underlying apathy of Europeans toward the EU.

How should we explain the paradoxical transformation wrought by the EU in Europe, whereby decision making seems to have been disconnected from direct democratic participation? One factor the authors overlook is the nature of the issues that the EU handles: trade policy, monetary policy, industrial standardization, competition policy, constitutional adjudication, human rights, and even some environmental policies and peacekeeping. These rarely influence individual voting behavior in any advanced industrial democracies. The issues left to the nation-states—social welfare provisions, public procurement, small business regulation, cultural policy, education, and most immigration issues—constitute the major political concerns of Europeans. Is it any surprise that voters, while conceding the importance of the EP, are apathetic about marginal shifts in EU policies? And is it surprising, therefore, that citizens do not perceive the European democratic deficit that scholars decry?

Much more needs to be done to understand the sources of public opinion and voting behavior in the emergent multilevel European polity. Future studies must pay close attention to the judicious yet paradoxical findings of this book. One step forward would be to measure more precisely the level of issue salience and concern among voters, as a baseline for their expected behavior in general elections.

The Geography of Money. By Benjamin J. Cohen. Ithaca, NY, and London: Cornell University Press, 1998. 229p. $37.50 cloth, $17.95 paper.

Tony Porter, *McMaster University*

This new book by one of the most distinguished senior contributors to scholarship on the international political economy (IPE) of money is a fascinating and ambitious effort to remap our understandings of the geography of money. Cohen argues convincingly that the prevailing mental map based on one nation/one money is outmoded. Monies are primarily differentiated today not by territory but by their function in a hierarchical pyramid-like arrangement; the few currencies at the top are used widely internationally, whereas those at the bottom have been displaced even within their home borders and may now serve only trivial administrative functions. The great number of theoretical and empirical insights into an important process of global restructuring, and the engaging and straightforward style, should make this book of interest well beyond those who specialize in the sometimes dauntingly technical literature on global money and finance.

Perhaps most interesting is the way Cohen focuses on the social institutions that lie between the categories usually stressed by IPE scholars. This includes his emphasis on mapping and spaces as social constructions anchored in both the empirical world and the world of ideas. Similarly, money is seen as involving social networks that operate between and interact with the anonymous market forces and self-interested states on which much IPE scholarship focuses. Cohen's concept of money's authoritative domain, which blends elements of the market-based transactional domain and the state-centered territorial domain, is very useful in capturing relations between top currencies and subordinate ones that

otherwise would be obscured. His treatment of states as oligopolists competing to provide money nicely captures the types of relationships that can result from the complex interplay of state and markets. Even his policy prescriptions include options, such as enhancing state legitimacy by subordinating the national currency to a more credible international money (pp. 121–2), that avoid the binary alternatives of being a powerful state or being a state eroded by globalization that are often presented.

Cohen also does an exemplary job of highlighting hitherto underrecognized political dimensions of economic processes in a manner consistent with the best IPE tradition. This is evident in his specification of four aspects of money that relate to state power: political symbolism; seigniorage (the revenues that come from creating money); macroeconomic management; and monetary insulation (the ability to protect citizens from negative international influences). Cohen usefully compares the old territorial money to the contemporary geography of money for each of these. His emphasis on the political dimension of money is also evident in his convincing answer to a question that more narrowly economic models have been unable to answer: Why do some multilateral monetary arrangements survive and others fail? He points to the importance of a hegemonic leader within the multilateral arrangement and to the sense of community among member states, and he supports this argument by comparing various monetary alliances from the nineteenth and twentieth centuries. Also politically noteworthy is Cohen's emphasis on the very different constraints and opportunities facing states at different levels of his pyramid-like hierarchy of currencies. This is a welcome corrective to a common tendency to treat states as essentially alike in the constraints they face due to globalization or in the policies that are prescribed for them.

The *Geography of Money* raises two sets of questions that need further consideration. The first is historical and bears on current debates about what aspects of globalization are really new. Despite stressing the brevity of the historical period in which territorial currencies existed, Cohen describes their rise as a calculated outcome of state action in a way that reinforces the overly state-centric perspective he is determined to criticize elsewhere in the book. Yet, as Hendrik Spruyt (*The Sovereign State and Its Competitors*, 1994, pp. 161–2) argues, the standardization of money by states was beneficial for commerce and thus helped territorial states in their competition with other jurisdictions. Perhaps in monetary matters states have always needed to respond to market actors. One can argue that national monies are appropriate when national manufacturing is more important than multinational production, and it is the scale of production more than the relationship among states, markets, and money that has changed today.

The second and related question raised by Cohen's book is the character of relationships between monetary networks and other institutions. Any author must set practical limits to a book's scope, so it is not unreasonable that Cohen did not consider this relationship. Nevertheless, his emphasis on monetary networks comes with a cost that should be addressed in future research. Three such relationships stand out. First are the institutions in the "real" economy. Aside from a brief acknowledgment of particular societal interests (pp. 63–4), such as the role of exporters in exchange-rate politics, there is not much here on the relationship of the real economy to money. Yet, one can argue that confidence in the capacity of a nation's economy to produce sufficient goods that holders of the national currency want to buy can be as important as confidence in the state's commitment not to print too much money. Second are financial institutions. One

lesson of the East Asian crisis is that the adequacy of banks and other formal and informal institutions that manage and produce information about financial flows can have a major effect on the value of a currency—and earlier links between the strength of financial institutions and money can be found in the role of financial centers in supporting international monies. Third are political institutions. Most striking is the lack of discussion of the International Monetary Fund's political significance in Cohen's book. In short, more can be done to extend Cohen's network analysis to fill in more specifically the institutional connections among money, market actors, and states.

Overall, Cohen makes an excellent contribution not just to our understanding of the critically important question of international money but also to the ongoing effort in IPE and elsewhere to conceptualize economic and political relationships in a globalizing world.

The Origins of Major War. By Dale C. Copeland. Ithaca, NY: Cornell University Press, 2000. 322p. $52.00 cloth, $29.50 paper.

William Reed, *Michigan State University*

Studies of the relationship between the distribution of power and prospects for conflict or cooperation have a distinguished history in the field of international politics. Several competing theories have been offered to explain variation in foreign policy behavior as a function of power distributions. Moreover, a wealth of empirical evidence suggests the crucial role that power plays in international bargaining. *The Origins of Major War* is a deeply penetrating, extraordinarily wide-ranging, and judicious treatment of the onset of major conflict, and it offers an explanation and some evidence for the relationship between power differentials and major power conflict. Copeland provides a dynamic theory of major power war, building upon classic realism. In my view, his diagnosis and combination of prescription are substantially correct.

In the introductory chapter, Copeland claims to know why there is a common cause of major wars. The importance of his contribution lies in the inadequacy of traditional realist and neorealist approaches to the study of major war. To account for these inadequacies, Copeland offers the dynamic differentials theory: Powerful states, anticipating significant declines in capability, typically initiate major wars. Coupled with the expectation of a significant decline in power is the fear of attack or coercion by other states. The assumption that all declining states fear the future is at the heart of Copeland's theory.

Although the central focus is on power differentials and shifts, Copeland also encourages scholars to think more rigorously about types of decline. He argues for three sorts: failing economic and technological bases; strong military capability but diminished economic power; and weakening due to an arms race and/or alliance building. Another strength of the book is its treatment of conflicts short of war. Copeland suggests how states can use such actions and moves beyond the common dichotomy of war or no war.

Following a discussion of his theory, Copeland sets out to evaluate the model with a series of case studies, which make up the bulk of the book. They include a discussion of the outbreak of World War I, the onset of World War II, the Cold War conflict, the Cuban missile crisis, and a sampling of other wars among major powers.

A shortcoming is Copeland's incomplete treatment of

other theories of war that focus on how shifts in power may enhance the prospects for cooperation or conflict. Specifically, little attention is given to power transition theory, beyond dated work by A.F.K. Organski and Jacek Kugler. Even more surprising, there is no discussion of Robert Powell's *In the Shadow of Power* (1999), perhaps the most rigorous and convincing treatment of power differentials and their effect on bargained outcomes.

On the whole, Copeland provides an elegant theory of major power war and evaluates it with several interesting case studies. Future work might explore the power of his theory with a statistical test that controls for competing explanations of the same phenomenon. This could provide a better gauge of how Copeland's dynamic differentials theory stands up to power transition theory and Powell's bargaining models.

Domestic Sources of International Environmental Policy: Industry, Environmentalists, and U.S. Power. By Elizabeth R. DeSombre. Cambridge, MA: MIT Press, 2000. 316p. $22.00 paper.

Understanding Global Environmental Politics: Domination, Accumulation, Resistance. By Matthew Paterson. London: Macmillan, 2000. 216p. $49.95.

Ronald B. Mitchell, *University of Oregon*

In two provocative new books, Elizabeth DeSombre and Matthew Paterson attack two issues that call into question common assumptions about international environmental politics (IEP). Paterson asks: Does our global environmental predicament reflect unfortunate, but essentially unrelated, secular trends or the influence of deeper, structural forces? DeSombre asks: Does resolution of environmental problems, whatever their sources, require broad support among many countries, or can solutions arise from unilateral action by a single powerful state? Paterson's answer involves a refreshing critique of the IEP literature that shows how traditional realist and liberal approaches systematically ignore the underlying causes of global environmental change. DeSombre provides a trenchant analysis of when, how, and why a country will attempt, and succeed at, internalization of its own domestic environmental regulations. Both books make significant contributions to the growing IEP literature, extending it to important new areas of research.

In *Understanding Global Environmental Politics*, Paterson critiques international relations theories of IEP from a distinctly Green perspective. He claims, correctly, that most literature fails to ask about the origin of global environmental problems. Rather, it is assumed that they result from either "(a) an interstate 'tragedy of the commons' and/or (b) a set of secular trends which are treated as exogenous to any conceptual or theoretical enquiry" (p. 11). Paterson problematizes that assumption, offering instead a structural explanation that sees international environmental problems as predictable consequences of "four main, interrelated, power structures of world politics: the state system; capitalism; knowledge; and patriarchy" (p. 40). For Paterson, these power structures produce an inexorable drive for domination of and accumulation from nature, which in turn produces the plethora of environmental problems we face. He rejects the notion that environmental nongovernmental organizations, networks, and movements—in short, global civil society—can transform the practices of multinational corporations, governments, and consumers so long as underlying and "intrinsically unsustainable" structures remain in place (pp. 141, 159). Instead, resolving our global environmental crisis requires "a

politics of resistance, not of improving the way international treaties and institutions operate" (pp. 40, 160).

Paterson poses a frequently ignored question and proposes a challenging answer to it. Diverging from most IEP authors' grounding in mainstream theories of international relations, he builds on ecosocialism, social ecology, ecofeminism, green politics, and deep ecology (p. 4). From this base, he rejects the common view that "international power structures are [merely] inconsistent with principles of sustainability" and argues instead that they systematically and "systemically *produce* environmental change in the first place" (p. 5). Paterson begins by extracting the often implicit explanations of the causes of global environmental problems that are embedded in IEP scholarship. After showing these to be unsatisfactory, he presents a self-conscious and thoughtful argument that demonstrates how global environmental change is driven by dynamics inherent to the state system (state-building, military competition, environmental displacement, and hierarchy); to capitalism (growth, commodification, profit maximization, and inequity); to knowledge (the human-nature duality and the dominance of scientific legitimacy); and to patriarchy (individualism, instrumental rationality, domination).

Paterson evaluates these arguments through case studies of British experience with ocean encroachment and sea defenses, the development and support of an automobile culture, and the development of meat-eating culture and "McDonaldization." These cases provide absorbing illustrations of how the exigencies of state-building, global capitalism, the power accorded to Western science, and patriarchy explain efforts to tame the ocean and promote automobile-based and meat-based economies. The chapter on sea defenses highlights the "domination and accumulation" aspects of his argument, and the chapters on automobile culture and on fast food, consumer, and meat-eating cultures bring out his third theme of "resistance and transformation" to global power structures.

Although the book is interesting and well written, more careful case selection would have allowed Paterson to provide a stronger empirical foundation for his argument. As he notes, "it may seem odd, even indefensible, to base the book on cases drawn exclusively from the UK" (p. 8). Notwithstanding his argument that the cases reflect "practices and institutions [that] are 'local everywhere' " (p. 9), stronger support for the claim that global environmental change is driven by global political power structures would come from cases representing a wider variety of contexts. If global forces are the sources of our environmental woes, then similar evidence from other countries and regions should be readily available. Even adding anecdotal evidence drawn from, say, the United States and other European countries would have strengthened the arguments about the development of car and meat-eating cultures. Although Paterson clearly intended the cases to illustrate rather than test his theory, one still wants more evidence to show that the cases represent systemic problems of state-building, capitalism, and patriarchy, not just problems of British state-building, capitalism, and patriarchy.

Whereas Paterson focuses on the source of environmental problems, DeSombre focuses on their solutions. In *Domestic Sources of International Environmental Policy*, she addresses two different but interconnected questions: When will states attempt to internationalize their domestic environmental policies, and when will those attempts succeed? She develops a compelling theoretical argument that powerful states, and specifically the United States, will pressure other states to adopt their environmental standards whenever such interna-

tionalization simultaneously broadens protection of the re-
source and averts competitive disadvantages arising from
unilateral regulation (p. 245). "A coalition of 'Baptists and
bootleggers'—environmentalists and industry—forms" that
pressures decision makers to extend domestic standards
abroad (p. 45). Environmental and protectionist arguments
can be persuasive in tandem, but neither proves persuasive
alone. DeSombre makes specific, theoretically informed pre-
dictions about when incentives for internationalization
should be strongest, namely, when the regulated industry
competes abroad, when regulations involve processes and not
products, and when effective environmental protection re-
quires international action (p. 46).

To test her theory, DeSombre selects U.S. regulations
involving five endangered species, three air pollutants, and
five fisheries. These vary in terms of incentives for interna-
tionalization (her independent variable) and the environ-
mental issues involved, but they are relatively representative
of environmental issues generally and susceptible to system-
atic evaluation of domestic regulations and efforts to inter-
nationalize them (pp. 47–8). By selecting cases that vary on
her independent variable rather than cases in which the
United States attempted internationalization (her dependent
variable, at this stage), DeSombre can compare predictions
of when the United States will seek to internationalize
regulations against actual experience. These predictions sup-
port her argument not only by showing that when economic
and environmental incentives existed the United States
sought internationalization, but also by showing that when
either incentive was weak or absent the United States did not
seek internationalization. By presenting cases in which a
coalition of interests did not achieve internationalization, she
is able to conclude, appropriately, that "a coalition of Bap-
tists and bootleggers is thus a necessary (though perhaps not
sufficient) condition to explain internationalization" (p. 131).

In the second half of the book, DeSombre switches atten-
tion to attempts to convince other states to adopt the desired
environmental regulations (p. 137). Building on the extensive
literature on economic sanctions, she examines the respon-
siveness of target states to U.S. pressure. DeSombre argues
that the target state may adopt a new environmental policy
because it believes doing so as in its own self-interest,
because it views the request as legitimate in light of existing
international rules and norms, or because the request is
backed by a potent and credible threat. To evaluate this
portion of her argument, DeSombre extends the analysis of
the cases already developed. The cases provide strong sup-
port for her conclusion that self-interest and legitimacy "are
not insignificant factors" (p. 138), but of greater significance
is a threat made potent by the sending state's market power
(dependent largely on exogenous characteristics of trade in
the good in question) and made credible when the coalition
pushing for adoption remains in existence and has incentives
to follow through on imposing it (p. 247).

An otherwise impressive book is weakened by DeSombre's
choice of an ambitious number of cases and a structure that,
at times, obscures her theoretical argument. The three chap-
ters on the push for internationalization each begin with
about fifteen pages of case description, and the chapter on
the success of internationalization efforts begins with 50
pages of background. The case descriptions vary considerably
in thoroughness: Some are only a paragraph, others extend to
nine pages. Theoretically motivated readers may find these
materials too extensive, but empirically minded readers may
find them too cursory. DeSombre's skill in cross-case analyses
could have been used to better advantage had the whole of
each empirical chapter, rather than only the second half,

been structured around theoretical constructs rather than
cases.

Both Paterson and DeSombre have written engaging books
that pose challenging new questions, questions that move
beyond those that have commanded the attention of most
authors working on international environmental politics for
the last ten years. Just as important, both authors provide
valuable theoretical frameworks that will point the way for
others trying to answer them.

War and Peace in International Rivalry. By Paul F. Diehl
and Gary Goertz. Ann Arbor: University of Michigan
Press, 2000. 319p. $49.50.

John Vasquez, *Vanderbilt University*

When the intellectual history of international relations in-
quiry is written for our time, *War and Peace in International
Rivalry* may very well be seen as a seminal book. Along with
Frank Wayman, Diehl and Goertz have been at the forefront
of a major conceptual breakthrough in the way peace and war
are studied. This book is their major statement of the subject
and presents their most important findings.

Diehl and Goertz argue that the key to unlocking the
causes of war is not to study the militarized disputes that
precede the conflict and try to analyze why some situations
escalate to war and others do not. Rather, we need to
examine the underlying relationship that connects these
disputes and can produce an enduring rivalry, which occurs
when two states have six or more disputes within twenty years
(p. 44). As they show, enduring rivals have a greater proba-
bility of going to war. Theoretically, this is a conceptual
breakthrough because it takes scholars back to studying the
underlying relationship that gives rise to war.

Too much of current research treats militarized interstate
disputes (MIDs) as if they were discrete independent events,
unrelated to what precedes or follows. Challenging this
assumption not only makes sense theoretically but also has
implications for statistical analyses, because researchers of-
ten employ statistics that assume their observations are
independent. Diehl and Goertz suggest that MIDs are not
independent. They argue that rather than search for the
causes of war, we should analyze how and why some pairs of
states become enduring rivals, what it is about the dynamics
of their rivalry that produces war, and how those dynamics
might be changed to produce a peaceful relationship that
would end the rivalry.

The book makes great headway in accomplishing this goal
by addressing and often overcoming major conceptual and
measurement problems, marshaling data that can provide
some answers to the key questions, and demonstrating that
such an approach will bear important empirical fruit. The
authors begin with an analysis of how rivalry should be
defined. Even critics of their particular definition, like myself,
must be impressed with the thoroughness of their discussion
and their review of the history of the concept. They produce
a typology—isolated rivals, proto-rivals, and enduring ri-
vals—that will be widely used in the field. My one quibble is
that I prefer to think of it as a typology of conflict relation-
ships that have become militarized, with isolated conflict at
one end and rivalry at the other.

A key contribution is data on interstate enduring rivalry
from 1816 to 1992. There is a wealth of descriptive informa-
tion in this book of use not only to quantitative scholars but
also to more historically oriented scholars. This includes
information on which states are enduring rivals (pp. 145–6);
how rivalries are distributed over time; the number of

enduring rivalries of specific states; the basic pattern of recurring disputes in each enduring rivalry (e.g., increasing, decreasing, flat); their volatility; and the extent to which they are linked to other enduring rivalries. There are also important measurement contributions, particularly the new index of dispute severity in Appendix B. Those who take a case study approach will find these measures useful, because they can find here statistics on individual rivalries, such as the Soviet-American, Sino-Soviet, and Anglo-German.

More central is the attempt by Diehl and Goertz to show how the rivalry approach will add to our knowledge of peace and war. They also show how this conceptual shift can provide fresh insights into how to study the democratic peace and conflict management. The book contains a number of significant findings that any serious scholar and student of war and conflict will find informative. Of these, the most important is that enduring rivals have a much higher probability of going to war (.59) than states that engage in isolated conflict (.16) or proto-rivals (.32) (pp. 61–3). Almost 60% of the enduring rivals from 1816 to 1992 have at least one war, whereas other states with militarized disputes are considerably less war prone. Overall, only 21% of the dyads with militarized disputes ever have a war. War, then, is intimately connected with states that have repeated disputes. Therefore, it is reasonable to assume, as Diehl and Goertz advocate, that understanding the relationship that gives rise to recurring disputes will tell us something about war, and changing the relationship so that disputes do not recur will tell us something about conflict management and peace.

When looking at cases that have a change in the democratic status of one of the parties, Diehl and Goertz find that when both sides are democratic, they are less likely to have a MID (p. 120). They also present evidence that regime shifts are associated with rivalry termination; 11 of 19 (58%) enduring rivalries ended when one side shifted so that both became democratic (p. 123), although it is unclear how much of this finding is driven by the end of the Cold War.

Several analyses examine whether enduring rivalries gradually evolve or follow a punctuated equilibrium model (begin and end with major shocks). On the whole, Diehl and Goertz favor the latter, although their most impressive evidence for it is based on predicting a random pattern (pp. 172–8), and at times they find some individual rivalries that follow a more evolutionary model.

Diehl and Goertz also find that mediation can delay the occurrence of a MID in an enduring rivalry but does not seem to affect the basic level of conflict (p. 214). Enduring rivalries that involve territorial disagreements have more frequent disputes than those that do not (p. 214). Similarly, enduring rivalries that are linked by a common dispute have a higher basic level of conflict, increased volatility, and more frequent wars than enduring rivalries that are not so linked (pp. 258–60). The latter finding, however, will have to be examined for selection effects to see whether it is unique to enduring rivals or is also present in dyads that are proto-rivals or just have isolated conflict.

The focus on interstate rivalry has the potential to change and radically improve our understanding of why war occurs and how we approach the study of world politics. For Diehl and Goertz, one of the most theoretically significant events in history is the rise and fall of interstate rivalries. Why they occur, intensify, and end are key questions we need to answer if we are to understand the fundamental forces underlying world politics and history, including the outbreak of war. Diehl and Goertz not only have brought these questions center stage but also have gone a long way toward providing answers based on replicable evidence. No serious scholar or student of conflict can afford to ignore this book.

Exporting Environmentalism: U.S. Multinational Chemical Corporations in Brazil and Mexico. By Ronie Garcia-Johnson. Cambridge, MA: MIT Press, 2000. 282p. $20.00.

David V. Carruthers, *San Diego State University*

As toxic pollutants and hazardous production emerged across the twentieth century, first-world states came to play a guardian role, imposing constraints on capital to safeguard their workers, communities, and environments. As we enter the new century, vigorous protest and debate over the character and conditions of neoliberal globalization center on whether or how to reaffirm or extend such protections in an era of free trade, capital mobility, privatization, and deregulation. From Rio to Seattle, Kyoto to Prague, controversy arises over where the locus of authority for codes of corporate conduct might rest. Can states be redeemed as guardians and still avert a destructive "race to the bottom"? Can existing supranational bodies or regimes be charged with design and enforcement of global standards, or are new international institutions imperative? Dare we trust multinational corporations (MNCs) to police themselves? What is the proper role of advocacy networks and civic organizations?

Into this multifaceted debate jumps Ronie Garcia-Johnson, to examine corporate self-regulation. The core thesis of her provocative text is that environmental values, ideas, and practices are "exported" to the Third World via an unlikely conduit: voluntary compliance programs spearheaded by such multinational chemical giants as DuPont, Union Carbide, Dow, Monsanto, and Exxon. With memories of the Bhopal and Valdez catastrophes still fresh, students of environmental and community advocacy will recoil instinctively at this argument, tempted to dismiss the author as another apologist for cynical corporate "greenwashing." Beware the rush to judgment. This is a thought-provoking book, carefully argued, and it makes a unique contribution to the literature on MNCs—who are not just international actors but perhaps contributors to the environmental conscience of a globalizing civil society.

Garcia-Johnson argues that companies embrace voluntary compliance to gain competitive advantage, avoid disadvantage, or avert regulatory intervention. She focuses on the chemical industry's "Responsible Care" program, which to her represents "an institutionalized version of environmental ideology—corporate, voluntarist environmentalism" (p. 74–5). Her optimistic reading begins with background on environmental politics in the United States, Mexico, and Brazil, and she presents an unusual nomenclature that casts green traditions as "ideologies" and the diffusion of environmental values as "exports" and "imports."

The author then lays out the U.S. chemical industry's response to environmentalism, from attacks on Rachel Carson's *Silent Spring* (1962), through the rise of the regulatory state, to new grassroots antitoxics challenges, to the campaign to sabotage right-to-know. She presents the global expansion of the program in the 1990s as sincere in its ecological values but also pragmatic: to resuscitate a tarnished public image, deflect the "pollution haven" argument on Third World production, and to shape emerging global trade and production regimes. Case studies of Mexico and Brazil demonstrate the adoption of the program as recounted by industry officials and other observers ("imported" into Mexico by environmen-

tally enlightened executives, "exported" to Brazil by strategic U.S.-based MNCs):

Because the author's enthusiasm is so contagious, her prose so adept, and her story told with such freshness, even skeptics will find the argument persuasive. The book seems to promise a middle course between the unvarnished corporate boosterism of E. Bruce Harrison (*Going Green*, 1993) or Stephan Schmidheiny (*Changing Course*, 1992) and the position of their critics, who see volunteerism as a sinister campaign to coopt the language of resistance and divide and conquer the environmental movement (e.g., John Stauber and Sheldon Rampton, *Toxic Sludge Is Good for You*, 1995; Jed Greer and Kenny Bruno, *Greenwash*, 1996; Joshua Karliner, *The Corporate Planet*, 1997). The author portrays herself as above the fray: "It has not been my intention to take a side in this debate" (p. 195). But, of course, she does take sides, offering scarce comfort to critics of corporate greening.

Part of the problem may begin with conceiving environmentalism as ideology. John Dryzek's discourse approach (*The Politics of the Earth*, 1997) would offer stronger conceptual footing. For one thing, it articulates with greater clarity the array of green worldviews. Particularly frustrating in Garcia-Johnson's account is her final distillation of environmentalism into two divergent camps: a corporate volunteerism of "market-driven innovation and competition" versus a radical antitoxics movement that "questions whether we should continue our reliance on chemicals, and calls for government to enforce tough solutions" (p. 193). Dryzek's approach enables disaggregation of the former; it reveals a discourse rooted in the "economic rationalism" of the free-marketers, enhanced for Third World adaptation with selective elements of post-Rio "sustainable development," and it gains a further sheen of legitimacy with "ecological modernization" principles (such as "pollution prevention pays"), borrowed from the European corporatists. Similarly, the universe of critical discourses would not so easily be compressed and dismissed as a singular camp of antichemical Luddites eager to wield an authoritarian state.

Dryzek's analysis also considers ontology, agency, motivation, and metaphor, which better reveals the power dimension lacking in the neutralized account of Garcia-Johnson. She grants authenticity to the program, exuberantly proclaiming that "87 percent of the chemical production on the globe is taking place in the shadow of U.S. and Canadian environmentalism" (p. 100). But discourses can be usurped and reconfigured to serve instrumental purposes. What if the actual "export" is not environmentalism at all, but a package of select discursive elements strategically adopted to preempt authentic environmentalism? We should be concerned about this Mexican executive's statement: "We want to be sure what happened to the U.S. chemical industry doesn't happen to us, getting to the point where there is so much regulation. . . . We see the program as a vaccine" (p. 123).

In fairness, the author recognizes these concerns and is careful not to overreach. She states that her task is not empirical. Her aim is to generate propositions, not test them. She concedes that she cannot assess industry motives and cannot demonstrate that environmental policy or outcomes were measurably affected in either country. Although we are ultimately left with taking industry intentions at face value, the book's innovation is to suggest that a story more nuanced than simple greenwash does exist. Perhaps industry executives are indeed becoming agents of global environmentalism, even if unwittingly. Programs on this scale, whatever their origins, invariably generate unintended consequences and open unanticipated spaces for creativity. That is not the story told here, but Garcia-Johnson has illuminated a path of inquiry well worth following.

War and Punishment; The Causes of War Termination and the First World War. By Hein E. Goemans. Princeton, NJ: Princeton University Press, 2000. 355p. $49.50 cloth, $19.95 paper.

D. Scott Bennett, *Pennsylvania State University*

Hein Goemans develops and tests a rationalist model of war termination that incorporates domestic politics, focusing on leader tenure and survival in expected postwar political environments. The book is one of a growing number of works that look beyond the initiation of conflict to its conclusion, examine the dynamics of conflict over time, and incorporate domestic political factors through a multimethod analysis.

Goemans's basic conception of war is that it is a bargaining process designed to find the other side's reservation price, that is, the minimum terms of settlement it is willing to accept to end the conflict. Goemans makes two key arguments about war termination. First, a necessary condition is that there be an overlap in the range of acceptable settlement terms. The alternative at any point to settling on some terms is continued conflict; the likely outcome of continued conflict is a function of state strength and resolve. But strength and resolve are private information, and state leaders have incentives to misrepresent these, so at the beginning of a war the expected utility of fighting for both states is greater than the utility of settling the conflict without violence. After a war begins, it will end only when sufficient new information reveals that a bargaining space has opened. Updated estimates of relative strength and resolve lead to changes in states' minimum settlement terms and, in turn, to the possibility of war termination.

The second argument introduces domestic politics into the equation. Goemans maintains that different regime types will respond differently to new information, and leaders in semirepressive states will prefer to continue to fight, even when given clear evidence that they are losing. This is not a function of motivated misperception but a perfectly rational choice, given the domestic environment. Rejecting the unitary actor assumption, Goemans distinguishes between the costs and benefits to leaders and the costs and benefits to the rest of the state. Leaders in semirepressive regimes can anticipate severe domestic punishment (exile, imprisonment, or death) if they lose a war, even if they lose moderately. By contrast, leaders in nonrepressive regimes (e.g., democracies) are likely to be less severely punished if they lose (they are just removed from office), and leaders in highly repressive regimes possess the means to remain in power even if they lose a war moderately.

Anticipating severe punishment, leaders in semirepressive states may rationally and deliberately adopt a high-variance fighting strategy. This bears significant costs to the state but holds out the prospect of a surprise victory, which will allow leaders to reward their followers and avoid punishment. A surprising conclusion here is that leaders in this type of regime are likely to increase their war aims even as they become more pessimistic about the likely outcome. Another is that wars with semirepressive and semiexclusionary losers will last longer than wars with other types of states.

Goemans tests his hypotheses using both statistical analysis and a set of case studies from World War I. Through a series of well-constructed statistical tests, Goemans first examines the relationship between war outcomes and leaders' fate, as well as the relationship between war duration and the regime

type of defeated states. Then, in six detailed case study chapters, Goemans tracks the evolving war aims of Britain, France, Germany, and Russia as indicators of their reservation price and examines how new information affected those aims. Given their reservation prices, he asks whether a mutually acceptable settlement existed in each year of the war, and the final termination of the conflict is traced to the development of an overlap in acceptable settlements.

Goemans is explicitly conscious of methods in the case studies, comparing two nonrepressive nonexclusionary regimes (Britain and France) and two semirepressive, semiexclusionary regimes (Germany and Russia). With explicit measurement over time he gains the variation necessary to test his argument. In each case, he examines primary source material (e.g., internal memos and cables) along with secondary sources in English, German, and French. Such exemplary historiography distinguishes this work from the variety of studies on World War I that use only documents in English. When the evidence is unclear, Goemans readily admits it; there is little sense that evidence is selectively presented.

One fascinating element emerges from the cases: Leaders were clearly aware of the domestic constraints of foreign leaders and of the need to create settlements that allowed for their survival or exit without punishment. Realists might argue that such efforts are just window dressing, and once the evidence on the battlefield proves who is stronger, the details of settlements are peripheral. But Goemans shows that leaders considered such details as a central part of ongoing demands, negotiations, and settlement offers.

The book's hypotheses are clearly derived from a central model, and the case studies are an integral part of testing the theory, not brief illustrations of the role of certain variables. The book is also unusually well organized in its logical progression and presentation of argument and evidence. Some parts of the conclusions are more convincing than others, however. National leaders clearly took into account their adversaries' domestic political constraints, and the evidence on leadership tenure after the war clearly supports the central argument about leaders' incentives in different regimes. But the evidence that the expansion of states' bargaining space is a key to war termination is ultimately less convincing. By the end of World War I, the participants' war aims had converged, and we observe that the states reached a settlement. We conclude that their reservation prices had changed, and therefore the bargaining space had expanded. But the argument then seems to reverse: Because this bargaining space opened, there was a settlement. Since the settlement is used as part of judging the expansion of the bargaining space, and then it is concluded that the expansion allowed the settlement, there remains a question of tautology here. Is it possible that some other factor, such as a political or economic shock, drove settlement, which we are then taking as evidence of a newly opened bargaining space?

To be fair, direct examination of actors' bargaining space and measurement of their reservation price are notoriously hard problems in assessing rational bargaining models. Here the question is whether "war aims" as Goemans measures them provide us with a good estimate of states' reservation price and the bargaining space. The author himself notes "some slack" between war aims and reservation price (p. 73, n. 1), although they should track closely. Goemans has made perhaps the best effort thus far to measure directly the key elements of this model in a specific context by a thorough analysis of archival materials (publicly stated war aims alone would clearly be inadequate).

The book also may claim too much when it suggests how many social science issues are accounted for in the analysis.

For instance, Goemans discusses strategic behavior and selection effects, recognizing that many of our conclusions in international relations are affected by unmeasured off-the-equilibrium-path behavior, and his cases include options that leaders considered but did not implement. Yet, he does not discuss statistical models that have been developed to deal with selection effects that may bias conclusions about causal effects even within the sample. As a result, we are left unsure whether selection is critical to the argument merely incidental.

War and Punishment is a fine example of social science theory mixed with thorough substantive analysis of a critical topic. It presents a well-developed argument, appropriate statistical tests, and thorough case analysis. It should be read by scholars of war termination, and it deserves a place in courses that examine conflict resolution and/or multimethod approaches to theory development and testing.

Ruling the World: Power Politics and the Rise of Supranational Institutions. By Lloyd Gruber. Princeton, NJ: Princeton University Press, 2000. 316p. $70.00 cloth, $24.95 paper.

Alan Cafruny, *Hamilton College*

The salience of supranational structures of governance increased dramatically during the last decade. The World Trade Organization (WTO) and North American Free Trade Agreement (NAFTA), for example, not only establish detailed norms, rules, and decision-making procedures but also endow supranational governing boards with sweeping powers of enforcement. The European Monetary Union (EMU) greatly constrains national sovereignty by eliminating national currencies and allowing the European Central Bank to play a key role in national economic policymaking. In his study of NAFTA and the European Monetary System (EMS), Lloyd Gruber contends that international relations scholars have had difficulty coming to grips with the trend toward global governance because the dominant theoretical approaches—neoliberalism and neorealism—tend to assume incorrectly that international cooperation produces mutual benefits for all contracting parties.

Neoliberalism originated in the 1970s, when it became apparent that international organizations and regimes were developing within the context of international power relations and not in opposition to them. Conceding that sovereignty was not "at bay," scholars sought to identify the conditions under which international cooperation could be achieved. Robert Keohane, for example, argued that states are reluctant to engage in collective action not because they may become worse off but because of negative externalities, lack of information, and free-rider problems. During the 1970s political leadership—specifically, American "hegemonic power"—was proposed as a means of overcoming these obstacles to cooperation. In the 1980s, when American hegemony was viewed as declining, attention shifted to the feasibility of multilateral cooperation (Robert O. Keohane, *After Hegemony*, 1984).

The realist tradition, of course, has been far more skeptical of the possibilities of international cooperation, especially in the form of strong, supranational agreements. Scholars such as Joseph Grieco argue that cooperation produces absolute benefits, but it nevertheless threatens to bring about losses in states' relative power positions, which leaders are generally unwilling to risk (Joseph Grieco, *Cooperation among Nations: Europe, America, and Non-Tariff Barriers to Trade*, 1990). Hence, neorealists emphasize the constraints on cooperation

that result from distributional conflicts and predict that supranational organizations will be weak and lacking in enforcement mechanisms. Yet, the proliferation of structures such as NAFTA, the WTO, and the post-Maastricht European Union confounds this expectation.

Although most scholarship directs attention to the differences between these two theoretical perspectives, Gruber contends that both employ a one-dimensional concept of power, which "assumes that the cooperative equilibria that emerge once ... distributional conflicts are resolved leave all participants, strong and weak alike, at least as well off as they would have been had the original, pre-cooperative status quo remained in effect" (p. 3). The emphasis on mutual benefits greatly limits our understanding of the "institutional design" of given international agreements and, more generally, of the implications of such agreements for states and societies.

Gruber's notion of go-it-alone power is informed by the classic work of Peter Bachrach and Morton Baratz on power as control over the agenda ("Two Faces of Power," *American Political Science Review* 56 [December 1962]: 947–52). Structural power shapes not only the decisions but also nondecisions that result from formally noncoercive constraints on state action. Powerful states establish a limited range of options for weaker states by threatening to act unilaterally, or go it alone. Weaker states cooperate "voluntarily" because exclusion would involve even greater costs. To be sure, government leaders seldom claim that the agreements they embrace leave them worse off in the long term, which leads Gruber to question the utility of constructivism and other subjectivist approaches to international relations. Therefore, scholars must assess the costs of collective action empirically and with reference to the counterfactual: Would the weaker parties have been better off under the noncooperative status quo? A plausible account of costs requires one to abandon the realist view of states as "internally undifferentiated entities" and to identify the "preferences of particular parties, groups, and individuals who govern them" (p. 259).

This conceptual framework provides a means of evaluating the effect of NAFTA and the EMS. Gruber shows clearly that the concept of mutual gains does not advance our understanding of these formally cooperative agreements. NAFTA involved significant costs for both Canada and Mexico, subjecting each to greater influence from the United States, and it eventually proved fatal to the electoral fortunes of the governing parties that negotiated or ratified it. European monetary cooperation, similarly, has imposed significant costs on both Italy and the UK.

NAFTA did not in itself precipitate Mexico's transition to neoliberalism, which had already begun in the mid-1980s as a result of the debt crisis. It nevertheless accelerated the process in ways that contributed to rural dislocation and greater penetration of U.S. multinationals into the Mexican energy sector. Economic and political instability that arose in part from NAFTA contributed to the peso crisis of 1995, from which Mexico is just now recovering. Canada's Liberal Party stridently opposed NAFTA when the Conservatives negotiated it in 1988, vowing to rescind the agreement when it returned to power. By 1993, however, despite evidence that free trade was harmful to significant sectors of Canadian industry, the Liberals ratified the pact because they feared the consequences of exclusion.

In his second case study, Gruber shows clearly that the EMS (and all subsequent European monetary regimes including EMU) have represented a Franco-German compact within a larger framework of German power and interest. Weaker states, such as Italy and Britain (not yet in the EMU), have borne substantial costs to avoid the perceived

greater risk of exclusion. Ruling parties in Germany and France have built varying degrees of flexibility into the agreements in order to ensure the adherence of successor governments.

The conceptual framework employed in *Ruling the World* represents a welcome addition to the discipline, but it is original only with respect to the emergent realist/liberal theoretical synthesis, which Gruber calls the "collective-action paradigm" (p. 258). Gruber does not discuss parallel efforts to reformulate and extend "Gramscian" concepts of hegemony and power to the study of international institutions (see, e.g., Stephen Gill, ed., *Gramsci, Historical Materialism, and International Relations*, 1993). Similarly, although the case studies are carefully constructed and well-documented, the conclusions are not novel or counterintuitive. Many knowledgeable observers of Mexican, Canadian, Italian, and British politics would agree that regional integration imposes substantial costs and risks on weaker states.

Almost two decades ago Susan Strange directed attention to the strong parochial and normative undercurrents in U.S. scholarship on international relations (*"Cave! Hic dragones:* A Critique of Regime Analysis," in Stephen D. Krasner, ed., *International Regimes*, 1983). As long as international institutions continue to reflect U.S. power and preferences, there will be a tendency for American scholars to emphasize their benefits. By applying a more sophisticated concept of power relations that draws attention to the costs of cooperation, *Ruling the World* contributes to the development of a more comprehensive and balanced framework for analysis.

New and Old Wars: Organized Violence in a Global Era. By Mary Kaldor. Stanford, CA: Stanford University Press, 1999. 192p. $45.00 cloth, $16.95 paper.

Barbara F. Walter, *University of California, San Diego*

The 1990s were characterized by a substantial increase in the number of ethnic conflicts within states. The international community, especially the United Nations, responded by intervening to prevent or resolve violence but frequently failed to do so. Mary Kaldor attempts to explain why identity-based violence has increased and why the international community's actions have so often failed.

The main argument is that increasing globalization has created a new type of organized violence that world leaders do not understand. Using Bosnia-Herzegovina as her model, Kaldor argues that wars have changed in three important ways. First, the goals of these "new" wars tend to be about identity politics rather than ideology or territory. Second, combatants increasingly rely on guerrilla methods to move or eliminate ethnic rivals from land rather than on conventional battles to conquer territory. Third, these new wars are financed from a variety of subnational and transnational sources, such as diaspora communities, humanitarian assistance, or illegal trade in arms and drugs, rather than from state coffers. As a result, these new wars tend to produce greater civilian deaths and larger refugee flows, and they are more likely to spread to neighboring states.

For Kaldor, the recent wars in the Balkans, the Caucasus, the Horn of Africa, Central Asia, and West Africa are not the result of a power vacuum left by the end of the Cold War or the result of a revolution in technology, as many people have argued. Instead, these new identity-based conflicts are an unanticipated side-effect of the globalization process that began after World War II. Greater integration at the global level not only eroded the autonomy of states and their monopoly on the legitimate use of force but also allowed

substate actors, such as organized crime and paramilitary groups, to pursue their own selfish goals, which they did by manipulating identity and espousing exclusionist, nationalist policies.

Kaldor builds her argument in two ways. First, she offers a brief history of warfare, emphasizing the way in which conventional methods were linked to the emergence of the European nation-state and showing how old notions of war continue to dominate policymakers' conception of security. Second, she then presents a more detailed account of the conflict in Bosnia-Herzegovina, which she argues is "the archetypal example, the paradigm" of the new type of war.

According to Kaldor, three characteristics of the war in Bosnia exemplify the new type of war. First, Bosnian Serb and Bosnian Croat elites aimed to establish ethnically homogeneous territories that would eventually become part of Serbia and Croatia, and they used virulent ethnic nationalism to achieve these goals. Their message found a receptive audience because of discontent arising from uneven development and the growing divide between the economic and scientific elite and backward rural regions that emerged as a result of globalization.

Second, Yugoslavia epitomizes the new type of war in its methods and sources of funding. Violence was pursued predominantly by irregular forces—paramilitary organizations, foreign mercenary groups, local police, and armed civilians—rather than the forces of the state, and violence was directed against civilian populations rather than opposing forces.

Third, Yugoslavia represented a new war because international involvement was both extensive and ineffective. Had the international community understood that a fundamentally different type of war had emerged, it never would have agreed to partition society along ethnic lines, which delivered to the nationalists the very goals they pursued. What was needed instead was greater humanitarian intervention in the form of civilian protection, law enforcement, and civil involvement, which air strikes and consent-based peacekeeping could not do. Precisely because the international community failed to understand the new character of war its efforts to reconstruct a peaceful Yugoslavia failed.

Kaldor argues that the international community should abandon its current policy of pushing for negotiated solutions in favor of what she calls a more "cosmopolitan" approach. Peace negotiations only raise the profile of warring parties and extend public legitimacy to individuals who might be criminals. What is needed is to control violence long enough for civil society to reemerge. This can be done in a number of ways. First and foremost, outsiders can serve the important role of restoring law and order so that normal life can resume and refugees and displaced persons can be repatriated. Providing secure areas not only would protect civilians—the main targets in these wars—but also would allow alternative forms of inclusive politics to emerge. Many of the tactics developed in recent wars, such as safety zones, humanitarian corridors, or no-fly zones, could serve this purpose. Outsiders also can aid in disarming factions, capturing war criminals, policing and training local police forces, and restoring judiciaries. Finally, outsiders can help restore the local economy in order to remove the atmosphere of fear in which people live and to offer young people an alternative livelihood to the army or mafia.

Three principles in particular should be followed if a new civil society is to be rebuilt. First, all assistance projects should be based on the principles of openness and integration. That is, anyone should be able to benefit from postwar projects, and these should be explicitly directed toward bringing people together through the employment of refugees, displaced persons, or demobilized soldiers, or through sharing. Second, outside assistance should be decentralized to encourage local initiatives. Third, outsiders could rely on local specialists and encourage a wide-ranging local debate about how aid should be provided. Each of these strategies should help increase efficiency, increase transparency, reduce corruption, and build civic engagement.

In short, Kaldor advocates a form of "global overwatch" whereby nation-states and international organizations monitor and enforce human rights and humanitarian law. Just as it is increasingly accepted that governments can intervene in family affairs to stop domestic violence, so it should be possible for outsiders to intervene in domestic affairs to stop civilian violence. For Kaldor, this type of cosmopolitan governance is needed to fill the void left by the erosion of the nation-state and its growing inability to regulate violence within its borders.

This book is an attempt to rethink traditional notions of security and to offer a different perspective on the evolving nature and conduct of war. It is also a call for innovation in the international community's understanding of and response to violence. Ultimately, the author may be right that the nature of war has fundamentally changed and that the international community's response is inadequate. In the end, however, it is difficult to know whether these claims are correct, given the lack of evidence. One is left with the nagging feeling that the increase in civil wars between 1990 and 2000 was simply a bubble, similar to that which occurred at the end of colonialism, and not part of a larger trend.

More important, few specific recommendations are offered for how cosmopolitan governance can be successfully enforced. Policymakers interested in preventing and resolving violence will find few practical guidelines for pursuing a global overwatch system or for overcoming the impediments to such an interventionist policy. Scholars interested in understanding the root causes of ethnic conflict, the motivations behind predatory elites, or the appeal of nationalist ideology will also find few answers. What readers will find is an argument that advocates a new vision of and approach to security, in which islands of civility within states are protected and defended by translational institutions willing and able to intervene in the affairs of individual states. The nuts and bolts of how this is to be done, and the trade-offs involved, are left for the reader to determine.

Bargaining and Learning in Recurring Crises: The Soviet-American, Egyptian-Israeli, and Indo-Pakistani Rivalries. By Russell J. Leng. Ann Arbor: University of Michigan Press, 2000. 336p. $64.50 cloth, $24.95 paper.

                                        Dan Reiter, *Emory University*

In the past ten years there has been a burst of theoretical and empirical research on the topic of learning in international relations. Russell J. Leng's new book is the latest addition to this body of scholarship, and it builds on his past research on learning and crisis bargaining. Leng examines the role of learning in crisis bargaining strategies within ongoing, international rivalries. He asks a series of questions, including: Do patterns of crisis behavior repeat from one crisis to the next? Does the management of crises within an ongoing rivalry improve over time? Can changes in crisis behavior be traced to experiential learning? Do rival states draw the same lessons from their recurring crises? Does learning contribute to more effective crisis management?

To tackle these questions, Leng builds on his theoretical

and empirical work, in particular (*Journal of Conflict Resolution*) and the assembly and analysis of the Behavioral Correlates of War data set. He first frames the analysis (Russell Leng, "When Will They Ever Learn?," *Journal of Conflict Resolution* 27 [September 1983]: 379–419) by describing four kinds of crises—Fight, Resistance, Standoff, and Put-Down— and five basic influence strategies a state can adopt during a crisis: Bullying, Cautious Bullying, Reciprocating, Trial and Error, and Stonewalling. The central focus is on the role of learning in determining crisis behavior, duration, and outcomes.

The bulk of the book is an examination of three ongoing international rivalries. Leng analyzes the principal crises within each rivalry, focusing on whether lessons are drawn from earlier experiences and applied to behavior in later crises. He engages in both qualitative case studies and some quantitative analysis, the latter of which concerns coding the hostility levels of each side within each crisis.

The first rivalry examined is the Soviet-American competition during the Cold War. Specifically, Leng describes and analyzes both Berlin crises, the 1962 Cuban missile crisis, and the alerts crisis surrounding the 1973 Yom Kippur War. He found that from each crisis the superpowers frequently drew vicarious lessons (such as Kennedy's study of the July 1914 crisis) and also inferred that they would need to display greater resolve the next time. Overall, Leng found evidence of some "diagnostic" learning, the acquisition by each side of the other's motivations and intentions, but little evidence of "complex" learning, which requires a new interpretation or conceptualization of the entire relationship.

The second rivalry examined is between Egypt and Israel. The crises analyzed are the major conflicts between the two states: the 1948 Palestine War, the 1956 Suez War, the 1967 Six-Day War, and the 1973 Yom Kippur War. Leng describes an array of different lessons learned by each side after each crisis. The Egyptians learned after Suez the significant role played by outside powers in Middle Eastern politics, and after the Six-Day War they learned to appreciate both the importance of preemption and the power of the Israeli military. The Israelis learned most centrally that maintaining their military might was crucial for their national survival.

The third rivalry is between India and Pakistan, for which Leng describes the two Kashmir wars, the 1965 Rann of Kutch crisis, and the 1971 Bangladesh War. His principal finding is that the lessons drawn reinforced each side's hostile image of the other, which served to entrench their conflict. Furthermore, both drew inferences that served to contribute to low estimates of the other side's military capabilities. The image of the other as hostile but weak deepened the cycle of violence.

Leng uses the case studies to draw a number of general conclusions about the role of learning in crisis bargaining. He usefully points to three kinds of "dysfunctional lessons," which undermine effective crisis management, that actors can draw: those that drive self-reinforcing hostility, those that misapply historical analogies, and those that tend to strengthen realist belief systems. He expands on this last category to develop a "realpolitik experiential learning" (REL) model, which forecasts that lessons tend to be drawn from and reinforce realist belief systems. The REL model produces three specific predictions: following a successful outcome policymakers will adopt the same influence strategy; following a failed outcome policymakers will adopt a more coercive influence strategy; and following a crisis that ends in war the defender state will adopt a more coercive influence strategy. The model provides

a somewhat pessimistic vision: States nurture hostile relationships through the continuation and escalation of coercive behavior across time.

This book is a welcome expansion of our understanding of how the behavior and outcomes of past crises affect future crises. The REL model usefully integrates psychological and systemic factors to produce innovative predictions. The overall argument could have benefited, however, from more extensive theoretical development. In particular, the case studies would have been even more interesting if the REL model had been presented at the outset and its hypotheses tested on the three rivalries. More broadly, one wonders about the role learning may have played in the termination of these rivalries, in particular the Camp David Accords and the end of the Cold War in the early 1990s. Overall, the book contributes to our understanding of both crisis bargaining behavior and the learning process.

The Political Economy of NATO: Past, Present, and into the 21st Century. By Todd Sandler and Keith Hartley. Cambridge: Cambridge University Press, 1999. 240p. $29.95, cloth.

Glenn Palmer, *Pennsylvania State University*

This book has an admirable goal: to present policy-relevant results of past academic research to "a wide audience of students, practitioners, policy makers and researchers who want a modern treatment of NATO" (p. xii). To direct fruitfully the results of scholarly work to less academic audiences is a difficult task, because many theoretical assumptions and methodological issues must be skimmed over and criticisms of the work summarized or ignored. Nonetheless, the goal is a good one, and the authors are well positioned to accomplish their purpose, which they do to a large extent. For more than two decades Todd Sandler has been the primary proponent of the "joint product" model of alliances. This model is possibly the most prominent challenge to (or modification of) the public-goods approach to the study of alliances that was pioneered by Mancur Olson and Richard Zeckhauser. Keith Hartley, director of the Centre for Defence Economics at the University of York, has written extensively on defense procurement, disarmament, and conversion.

My comments are directed at two main points, the first being the policy-relevant aim of the book. The seven substantive chapters address such disparate issues as burden sharing, expansion, the organizational structure of NATO, and peacekeeping. Regarding the last, Sandler and Hartley argue that NATO's purposes are now directed more toward peacekeeping operations than was the case during the Cold War, when it provided deterrence (a public good) and protection (both a public and a private good) to its members. The security-oriented products of NATO evolved over time from pure deterrence to a mixture of deterrence and protection. According to the authors, that evolution resulted in a diminished tendency for free riding, as smaller allies could no longer simply rely on American nuclear capabilities and doctrine to deter Soviet aggression. NATO's new functions, with the doctrine of crisis management that emphasizes peacekeeping, "may involve more purely public outputs than those derived from NATO's defense activities in the previous two decades" (p. 51).

The authors are right to argue that peacekeeping has elements of a public good: Halting ethnic cleansing is a benefit enjoyed by all. Therefore, we can expect greater free riding in the future than in the recent past. An unhappy

alternative is feasible, however. If peacekeeping missions take place in areas that border on particular members of the alliance, such as Italy and Greece (and, in the future, conceivably politically unstable Poland and the Czech Republic), those states would derive greater benefit from NATO's peacekeeping and might be tempted to pursue it more energetically. Therefore, the most politically unstable alliance members might have the greatest incentive to increase their peacekeeping capabilities, that is, their military effectiveness, an unpleasant outcome. It is in NATO's interest, then, to provide an effective and alliance-supplied peacekeeping capability, which most likely would include a viable and continued American presence.

Sandler and Hartley explore the effects of an expanded post–Cold War NATO on the defense industrial base of the United States and NATO countries. Their conclusions are far ranging, but they worry that the creation of a NATO free-trade area (akin to the European Union) may lead to a smaller number of defense firms, which in turn may lead to "higher prices, inefficiency, monopoly profits, and reduced incentives to innovate" (p. 164). They also consider the most desirable size of NATO and conclude that, whatever the optimal number, continued expansion has political, structural, and defense-related costs that may well exceed the benefits.

Chapter 8 covers issues related to the "linkages among the allies and the proper organizational structure within NATO itself" (p. 223). This is one of the more interesting chapters, for it gets to the question of NATO's purpose since the demise of the Soviet Union and the disappearance of its military threat. A number of political scientists have argued recently that past and present alliances are not solely concerned with increasing the security of their members. Following James Morrow's article in the *American Journal of Political Science* ("Alliances and Asymmetry," *American Journal of Political Science* 4 [November 1991]: 904–34), some are moving to accept that many alliances involve the transfer of "autonomy" for "security"; that is, smaller allies exchange some of their political independence (to simplify slightly) for the security provided by the more powerful. Both the powerful and the weak gain from this trade.

Sandler and Hartley argue that their approach, which concentrates on linkage costs and benefits, includes the political things that particularly smaller allies must give up to ensure the involvement of larger states. I do not agree. The authors do an excellent job of applying their elegant and general model, but the political costs and benefits of NATO membership—particularly for the smaller allies—could be discussed more directly, even if in a general and theoretical fashion. If the value of the American-supplied security declines, for instance, presumably the smaller allies will be less willing to adopt policies preferred by the United States. If the United States wants to retain NATO as a vehicle for policy coordination, it needs to provide the allies those public goods, such as peacekeeping, for which it is well suited. The book might have benefited from greater exploration of the political relations between and among the allies.

The second focus of my comments is directed at the theoretical underpinnings of the academic work from which Sandler and Hartley draw their arguments and conclusions. The basic source for most of their policy analyses is the joint-product model developed and tested over the years by Sandler, along with his many colleagues. It asserts that some of the alliance-provided security comes in the form of protection rather than deterrence. Protection has some elements of a private good (it is rival and slightly excludable). To the extent that NATO's security is provided by protection,

free riding should be observed less. The joint-product model argues that changes in NATO's strategic doctrine, such as the move from assured destruction to flexible response, emphasize protection rather than deterrence, which weakens the incentives for free riding.

Empirically, the usual method for identifying free riding is through a strong positive correlation between income (GNP) and defense spending as a proportion of GNP. Indeed, according to that indicator, free riding has lessened within NATO since the 1960s. One might conclude that the joint-product model is correct. (A number of other empirical tests have been provided for the model, but their logic is not as tight, and their persuasiveness is weaker.) But alternative explanations exist for this empirical finding. For instance, the credibility of NATO's security may have allowed some allies the luxury of pursuing private benefits, either through fighting each other (Greece and Turkey) or through engaging in more active foreign policies outside Europe.

The joint-product model also does not counter well the argument that, whether through threatened nuclear deterrence or perceived capable protection, whether through assured destruction or flexible response, the point of NATO during the Cold War was to deter aggression. That is the goal and the product that bound the allies together through much of the Cold War, and the weaponry through which that was accomplished is secondary in importance to the mutual commitment the allies demonstrated. The book does not provide alternative theoretical approaches to the evolution of NATO, and the reader may be tempted to ascribe a greater weight to the arguments based on the model than might be merited.

Last, and most important, the book accomplishes its stated goal well. It is addressed to those outside academia who would benefit from a more theoretically based analysis of NATO's current situation. Its theoretical orientation is clear, its method approachable, and its conclusions or prescriptions follow. Too frequently researchers are satisfied to develop their arguments solely for other academics and are hesitant to apply their models to contemporary issues. Sandler and Hartley have fruitfully applied a large body of academic work to a wide range of issues. They have appropriately applied the academic work to each of the issues they address to provide coherent and consistent answers to the policy problems. That is something more of us might do well to emulate.

How to Democratize the European Union ... And Why Bother? By Philippe C. Schmitter. Lanham, MD, and Oxford: Rowman & Littlefield, 2000. 150p. $59.00 cloth, $17.95 paper.

Elizabeth Bomberg, *University of Edinburgh*

This thoughtful book builds on Schmitter's earlier work on European integration and, more recently, his interest in democratization in Latin America and southern Europe. Schmitter joins the ever expanding ranks of scholars and practitioners from a variety of backgrounds who have tackled the devilish issue of European Union (EU) democracy and legitimacy. This attempt is more hands-on than others. As the title suggests, Schmitter offers a do-it-yourself checklist of reforms to improve the EU's democratic credentials. His larger self-assigned task is to "provoke as wide-ranging a debate as possible on an issue that has just begun to receive the attention it deserves" (p. viii).

The book is indeed provocative, and the arguments are forceful. Schmitter is explicit about his assumptions as well as his views concerning the EU's future: The EU can be and

needs to be democratized, and the effort is worth the bother. Although he warns us "not to read further" (p. 2) should we find his assumptions implausible, of course we do read on, and with significant reward.

Schmitter's arguments are clearly laid out. The essay is structured around a broad examination of democracy and three component parts (citizenship, representation, and decision making). The opening discussion (chap. 1) is enriched by insights drawn from the literature on democratization. But translated to the EU, the discussion seems to confuse (or at least conflate) democracy and legitimacy (see esp. pp. 12–4). This overlap or possible confusion matters a lot, because the EU—unlike its member states—derives most of its legitimacy not from its democratic credentials but from what it puts out, namely, delivering peace and prosperity. The urgency of any "legitimacy crisis" thus rests as much on the EU's ability to deliver the goods as it does on the EU's appearance as an accountable or representative polity.

Among the more provocative assumptions about democratic reform, two stand out. First, Schmitter assumes that reform must be formal; it must involve "new rules and rights" agreed by member states. He laments the "absence of an explicitly constitutional bill of rights" because it means that questions of what is legitimate have been "largely defined at the national level or left to the vagaries of practice at the supranational level" (p. 3). What is needed, then, is the explicit endowment of new rules and rights by member states (p. 2).

But this emphasis on formal rules and rights is problematic. As Schmitter concludes in the chapter on citizenship, reform or fundamental change of this sort cannot be imposed formally, either through new rules or rights. The Maastricht Treaty codified citizenship into the EU rules, giving every citizen of every EU state European citizenship. Yet, the resulting change in citizenship or sense of "belonging" among the EU polity was imperceptible. Changes in identity are organic and slow; they cannot be introduced from above or magically instilled because of a change in—or even endowment of—rules or rights. More recently, the difficulties surrounding the drafting and adoption of a charter of fundamental rights underscore this fact.

Second, a related questionable assumption is that change or reform must come from the leaders: "The initiative for regime change in the EU has to come from above" (p. 1). Schmitter suggests the Monnet method (small steps leading toward grand effects) be used to further democratization, just as it was successfully used to further economic integration. Of course, what Schmitter neglects is the negative fallout of applying the Monnet method. Elite-driven and secretive, it is increasingly criticized for causing the very disillusionment Schmitter wants to dispel.

Not all the proposals are provocative, and several are noncontroversial. Most would agree with the author's assessment of the pathetic level of deliberative debate within member states concerning the issue of Euro-democracy. Now widely acknowledged is the claim that the problem of interest representation in the EU is not its lack but its skewed character (p. 54). Schmitter is also correct to assume that the unique character of the EU makes it very difficult to categorize. The EU clearly is not a state; it is not even a polity in the traditional sense but, rather, a system of multilevel governance, "a plurality of polities at different levels of aggregation—national, subnational, and supranational—that overlap in a multitude of domains" (p. 16).

The uniqueness of this "*objet politique nonidentifié*" (p. 2; Schmitter seems compelled to apply foreign labels whenever possible) leads the author to insist that novel institutions and

new forms of democratic practices will be necessitated (p. 14). Yet, when searching for models of reform Schmitter curiously relies on traditional notions of democracy and statehood. He critiques the EU for not upholding majoritarian democratic standards (pp. 4 and 10). He argues the EU is closer to a polity than a state, but his list of polity characteristics (pp. 16–7) seems remarkably similar to textbook characteristics of a state. In short, he wants to have it both ways; he insists the EU is unique, so we cannot draw on traditional state-defined notions of democracy, but he then draws on precisely those traditional notions (majoritarian democracy, state legitimacy, statehood) to evaluate and assess the "Euro-polity" and its democratic credentials.

A final assertion (in chap. 4) is that the decision-making procedures of the Council of Ministers need to be simplified and reformed. This need has been long acknowledged, but practical measures to deliver this reform (most recently at the Nice summit in 2000) have often ended in failure or something close to it. Like many of the problems Schmitter outlines, the hurdle to reform is neither lack of problem recognition nor lack of proposed solutions. The crux is the EU's inability to deliver these reforms and the politics that render an eminently sensible reform a nonstarter. On how to overcome these wider barriers to reform—often perverse dynamics of intergovernmental bargaining, the entrenchment of institutional norms and practices—Schmitter has less to offer.

The real strength of the book lies in Schmitter's successful grafting of insights from the literature on democratization, citizenship, and representation (nicely consolidated in chap. 5). His tough and always confident proposals for reform (labeled "modest," but not really) will animate wider debate on EU reform. Yet, although grounded in the literature on democratization, Schmitter writes as though isolated from the vast scholarship on the EU and EU democracy, at least that published in the last several years. He notes that the literature on the democratic deficit is "enormous" (p. 20). It is a shame, then, that he does not engage this rich body of work.

The attention—by academics and practitioners—to the question of EU democracy in recent years is staggering. Although not all this literature is well informed or convincing, much of it is just that, addressing the questions in a different but equally revealing way; testing assumptions Schmitter insists we take as given; drawing comparisons (including with the early United States) that Schmitter underplays. Schmitter does provide a few scattered references in the odd footnote, but he offers no sustained engagement with the burgeoning work on EU democracy. The lack is all the more disappointing precisely because one assumes Schmitter could take on these other interpretations and robustly defend—if not refine?—his own assumptions and proposals. One wishes he had done it here.

Principles of Global Security. By John D. Steinbruner. Washington, DC: Brookings Institution Press, 2000. 270p. $44.95 cloth, $18.95 paper.

Lynn H. Miller, *Temple University*

Now that more than a decade and two American presidencies have come and gone since the end of the Cold War, the United States has articulated no new grand strategy to address the novel security demands of the new age. There is nothing remotely comparable to the Truman Doctrine and the Marshall Plan that so radically reoriented U.S. strategic policy at the start of the era now ended. No doubt, the

reasons for that absence have something to do with the "if-it-ain't-broke" dictum. Confrontation and deterrence evidently worked to win the Cold War and, by this logic, should continue to serve the nation's security into the murky future. Yet, such thinking takes little account of the radically different world that is still being born out of the ashes of bipolarity and ignores the dangers in muddling along in an old mindset, oblivious to how that can induce the kind of future one wants. Such thinking also confines most of the recent security debate to only half the subject, that is, issues of our military capacity, whereas the purposes to which that capacity should be directed seemingly are too self-evident to demand such scrutiny.

John Steinbruner's latest work is the most persuasive argument to date for a radical reconception of contemporary security policy based upon an assessment of what purposes it ought to serve. Such a change is both compelling and now, more than ten years after the end of the Cold War, almost desperately overdue. In simplest terms, his thesis is that, in this age in which American power is unprecedented and largely unchallenged, the United States must have the wisdom to reorient its capability away from confrontation and deterrence to postures of equity and reassurance. If we are to have true security in a globalized world in the decades and generations to come, then we surely must overcome our zero-sum habits of thought that assume my security must be at your expense. Such a summary, regrettably, does little to suggest either the richness of Steinbruner's analysis or the overwhelming logic of the case he makes.

In the most sustained and systematic way, Steinbruner explores the nature of nuclear capability, real and prospective; the balance of conventional forces on the part of major players; the social explanations and implications for the spread of civil violence in our time; the potential for widespread harm of chemicals and biological pathogens; and the security needs and dilemmas of the most important actors on the world scene. On every count, he shows how a conscious attention on the part of the United States and its allies to moving away from deterrence and confrontation and toward policies of cooperation with those outside the alliance— including especially Russia, China, India, and North Korea— will enhance security for all, not least the United States.

This is a work of synthesis on a magisterial scale. As in any first-rate synthesis, Steinbruner presents data that many in the foreign policy community no doubt already know, but his assessment is both provocative and original. In the opening chapter, he reviews well-known trends in communication, population dynamics, and uneven military power by way of demonstrating how much is out of joint in perpetuating the strategic thinking of the Cold War, whose central principle he sees as providing "countervailing military power directed against deliberately calculated mass aggression."

Among the reasons Steinbruner cites for that doctrine's untenability in the new age are, first, the lack of a plausible opponent "to justify advanced states of readiness for . . . traditional missions on a large scale and with short notice"; second, the costliness of ruling principally by force; and, third, the West's "record of assertive military action creates an incentive for countries outside the U.S. alliance to develop some form of countervailing capability" (pp. 16–7). To overcome these problems, the legitimate defensive objectives of others must be recognized so that they systematically reassure one another not to contest those objectives. Steinbruner notes that the United States was adept at practicing systematic reassurance in a way that transformed its World War II enemies into allies. Now the challenge is to attempt

similar transformations of both the major Cold War enemies and smaller dissident states.

Some of what impresses about this work is the tight and careful marshaling of empirical evidence in support of what, in less learned hands, could be the kind of jeremiad some realists indulgently dismiss on grounds that the conflict of state interests is an immutable fact of political life. Steinbruner is not just a reasonable advocate for sounder thinking about our security. His analysis assumes every worst-case scenario of the most truculent realist; he fully acknowledges the difficulties in moving policy planners and, especially, bureaucracies into uncharted strategic waters and then makes plain why cooperative, rather than confrontational, military strategies are the way to avoid or overcome ugly futures. He shows repeatedly how zero-sum thinking about security matters has the greatest potential for creating worse insecurities and more dismal outcomes. He sees globalization as the phenomenon that invites—begs for—more nonzero-sum approaches that seek to enhance the security of rivals as a way of increasing the security of all.

The case of Russia is most salient. Steinbruner is eloquent in showing how the continuation by Bush and Clinton of policies of confrontation and deterrence, albeit tinkered with at the margins, is having potentially dangerous consequences for Russia's security posture. Despite various forms of security cooperation that the United States and NATO have developed with Russia, their reassuring effects "have been overwhelmed by what most Russians consider to be a threatening combination of adverse circumstance and political betrayal" (p. 206). Russia acceded to German unification because of the promise that NATO would not expand eastward, but within less than a decade NATO membership extended right up to the Russian border. Despite assurances that the alliance was defensive in character, NATO's action against Yugoslavia was seen in Russia as a repudiation of those assurances. Add to this the ongoing determination of U.S. presidents to continue a program of missile defense, even at the cost of repudiating the 1972 ABM Treaty. Steinbruner warns: "Any U.S. missile defense deployment of whatever size would constitute a threat to the viability of the Russian deterrent force and would make credible strategic reassurance virtually impossible to achieve" (p. 210).

This book should be at the top of the required reading list for the incoming president and his secretaries of State and Defense. It should be read carefully by everyone in the foreign policymaking establishments of the U.S. alliance. Many might then be persuaded to embrace its vision. Then, and only then, will it truly be possible to begin to increase the security of much of humankind in unprecedented ways.

Governing with Judges: Constitutional Politics in Europe. By Alec Stone Sweet. Oxford: Oxford University Press, 2000. 232p. $60.00 cloth, $24.95 paper.

Michael J. Baun, *Valdosta State University*

It is evident to any serious observer that the practice of governance in Western democracies is increasingly divergent from traditional theoretical and conceptual models. In Western Europe, traditional models of parliamentary democracy no longer accurately describe reality. The neocorporatist literature of the 1970s and 1980s pointed out the key role of organized interests and their (often formally recognized) privileged relationship with the state, creating arrangements that bypassed or subverted usual parliamentary processes. Alec Stone Sweet addresses yet another important development that has transformed the nature of democratic gover-

nance in continental Europe: the increased role of judges in policymaking and legislating and the growing interdependence of the judicial and legislative branches and functions of government. As he points out, this development has undermined core notions of parliamentary sovereignty and separation of powers that have underpinned the traditional European model of parliamentary democracy.

In brief, Sweet argues that the post-1945 establishment of enforceable constitutions in continental European democracies, with specialized courts possessing powers of constitutional review, has initiated a self-sustaining and expansionary dynamic that has resulted in the increased "judicialization" of lawmaking and "politicization" of constitutional review. (Britain remains an outstanding exception to this trend, although popular pressures and European integration may be leading it down a similar path.) In the process, constitutional courts and legislatures have become increasingly interdependent. Through their rulings (or even the prospect of them), constitutional courts shape ("construct") legislative activity, placing lawmaking "inexorably ... in the shadow of constitutional review" (p. 202). Operating within this constraining context, legislators increasingly apply constitutional reasoning to their own deliberations and effectively become courts of first instance, with constitutional courts as appellate venues. Through this mutual interpenetration of activities and modes of thinking, constitutional courts have become part of the legislative process and legislators constitutional judges. The net result is the growing role not only of judges in governing but also of constitutional reasoning and modes of deliberation in governance. As the author nicely puts it: "In the end, governing with judges also means governing like judges" (p. 204).

The book presents the argument in a compact and clearly organized manner. Chapter 1 elaborates the basic conceptual approach and surveys several perspectives on constitutional law that are used or addressed in subsequent chapters. Chapter 2 discusses the establishment and growing prominence of constitutional review in postwar continental Europe, focusing on the four main cases of Germany, Spain, Italy, and France; proper emphasis is given to the role of human rights in this new constitutionalism. The author also carefully explains the difference between European constitutional review and American-style judicial review, although subsequent chapters make clear that this difference is being eroded in practice. Chapters 3 and 4 describe the growing interdependence of the judicial and legislative branches and functions of government in European democracies, with specific examples from each of the four countries. Chapter 5 tackles the crucial question of "legitimacy" (more on this below).

Chapter 6 shifts from the four national cases to the supranational level, in particular the role of the European Court of Justice (ECJ) in "constitutionalizing" the European Community (EC) treaties over the past four decades. The creation of a supranational community of law and a hierarchical European court system, it is argued, has bolstered judicialization within the member states by empowering ordinary courts to engage in judicial review, which simultaneously enhances the policymaking role of the ECJ and ordinary national judges. In the concluding chapter, Sweet constructs a general theory of constitutional politics, elaborating a causal dynamic that is both self-sustaining and expansionary.

In the preface, Sweet anticipates many potential criticisms by admitting the book's insufficient attention to three key issues and their implications for his argument: federalism, the European Convention of Human Rights, and theories of constitutional interpretation. The author attributes these

deficiencies to the insistence of his editors on a relatively short and readable volume. Another possible criticism, however, concerns Sweet's treatment of the crucial question of legitimacy: If the growing interdependence of the legislative and judicial branches is undermining traditional doctrines of parliamentary sovereignty and separation of powers (and, hence, the authority of the core principles of majoritarian rule and the "general will"), how can this be legitimized as a basis for democratic governance? The author responds that such legitimacy is not provided by static and outmoded (Kelsenian) theories of constitutional review, which assume a separation of powers that no longer exists in reality, but by the participatory nature of constitutional politics, involving litigants (governments, political parties, private individuals), judges, and academic doctrinal experts. Moreover, this legitimacy is constructed over time, through the actual practice of constitutional politics. Thus, Sweet argues for a "democratic" basis of legitimacy for constitutional review using a constructivist-institutionalist approach.

This argument is fine as far as it goes, but it contains its own contradictions. As the author admits, the ultimate legitimacy of constitutional review (and of the increased governance role of judges) lies in the perception of the judge's basic neutrality (see Justice Stevens's dissenting opinion in Bush v. Gore). Yet, the dynamic of judicialization described in the book entails the inevitable politicization of the constitutional review process, which can only damage this perception of neutrality and expose the inherently political nature of the process. Politicization is also promoted by the participatory nature of constitutional politics that the author points to as a basis for legitimacy.

The result, as Sweet recognizes, is "a potentially intractable dilemma for the constitutional judge" and an ongoing "crisis of legitimacy" for the constitutional review process (p. 199). One way out, according to him, is for constitutional judges to cast their decisions in strictly normative terms and to fashion complex decisions that avoid declaring clear winners and losers, in these ways seeking to avoid political backlash. Another is reliance upon the popular perception that constitutional judges are more effective in protecting basic rights than governments and parliaments, which justifies the role of constitutional courts in functional terms.

This presumes, of course, that the protection of rights remains a more important value for the citizens of Western democracies than other, possibly conflicting, goals that seek their satisfaction through enforcement of the "general will" or majoritarian means. On the whole, the author's discussion of the legitimacy question leaves us unsatisfied and uneasy, although more acutely aware of the tenuous basis of legitimacy of this increasingly prevalent mode of democratic governance.

Governing with Judges succeeds in being very concise and readable. Despite its technical subject, it is well written and free of jargon, which makes it appropriate for upper-level undergraduate and graduate classes as well as for general readers without a special background in public or constitutional law. The book will be of interest to political scientists of varying interests and specializations, since the interdisciplinary nature of its subject and approach crosses the normal boundaries among public law, comparative politics, political theory, and international relations and law. As a contribution to the literature, it fills a noticeable void in the realm of comparative public law. Specialists in European politics will find the book particularly valuable for explaining an important aspect of the transformation of democratic governance in Western Europe, and they will undoubtedly look forward to Sweet's further studies of this topic.

Presidents and Prime Ministers: Conviction Politics in the Anglo-American Tradition. By Patricia Lee Sykes. Lawrence: University of Kansas Press, 2000. 399p. $45.00.

Stephen J. Wayne, *Georgetown University*

In the Stephen Skowronek tradition of leadership studies (*The Politics Presidents Make*, 1993), Patricia Lee Sykes makes an important substantive and methodological contribution with her cross-national comparison of conviction politicians in the United States and the United Kingdom. She begins with a topology that dichotomizes two basic leadership styles: conviction and consensus. She carefully defines these on the basis of their goals and strategies and the external conditions that facilitate them.

For conviction politicians, the goals are likely to justify the means, whereas for consensus politicians means and ends may not be nearly as distinguishable. For conviction politicians, principle counts; for consensus politicians, pragmatism works. Conviction politicians emphasize strong leadership; they seize the moment, convincing the public to rally to their cause. Consensus politicians are more sensitive and reactive to public concerns; they follow and lead. As Sykes sees it, conviction politicians can transform a political order, whereas consensus politicians accomplish incremental transactions within the system, thereby preserving the political order they inherit. Conviction politicians are passionate; consensus politicians emphasize rationality.

Although the beliefs and skills of these leadership types have much to do with their perceived success, Sykes suggests that the political environment is an important variable in determining which type emerges, when, for how long, and with what success. Much depends on perceived public needs, which facilitate or constrain a particular leadership type.

The author uses four comparative case studies to document her thesis: Andrew Jackson and Sir Robert Peel, William Gladstone and Grover Cleveland, Woodrow Wilson and David Lloyd George, and Margaret Thatcher and Ronald Reagan. The Thatcher-Reagan example receives the most attention and the most in-depth analysis, in part because it illustrates how contemporary conviction politicians can adapt to the institutional and political forces that shape and constrain them today: political parties, mass media, executive authority, and the international arena. Sykes devotes a chapter to each of these forces and how they condition leadership-oriented political activity.

The merits of this book are many. It is highly readable, very sophisticated, somewhat speculative—but always documented, in many cases with primary sources—and provocative throughout. I found myself both agreeing and arguing with the author in almost every chapter. Any book that forces one to rethink previously held assumptions, conceptions, and conclusions is a very worthwhile read, which this book certainly is.

An early argument I had with Sykes concerns her dismissal of personality, which she claims, "provides only a secondary explanatory factor in the study of political leadership" (p. 3). One cannot help but note character-based similarities that help explain certain conviction politicians' rise to power: strong adherence to the sanctity of their beliefs, unwillingness to compromise, and inability to tolerate, much less learn from, criticism. The Georges' classic study of Woodrow Wilson (Alexander L. George and Juliette L. George, *Woodrow Wilson and Colonel House*, 1956), which is not even referenced in the chapter on Wilson, postulates in a very persuasive way that his feelings about and relationship with his father had much to do with his intransigence on the

Versailles Treaty and League of Nations as well as his highly personal battle with Senator Lodge.

Similarly, several studies of Thatcher (Leo Abse, *Margaret, Daughter of Beatrice: A Politicians's Psycho-Biography of Margaret Thatcher*, 1989; Kenneth Minogue and Michael Biddis, *Thatcherism: Personality and Politics*, 1987; Chris Ogden, *Maggie: An Intimate Portrait of a Woman in Power*, 1990), none of which are cited by Sykes, plus Thatcher's own writings (*The Downing Street Years*, 1993, and *The Path to Power*, 1995) point to her strong association with male role models, particularly her father, and her need to reinforce her beliefs and opinions by surrounding herself with like-minded men, her unwillingness to tolerate any dissent within her inner circle, her need to dominate.

In the case of Reagan, although the personality structure is very different from that of Wilson and Thatcher, an argument can be made that his conversion to conservatism was a product of real-life disappointments combined with a script that became a lifeline to new friends, a new job and financial success, and later to political success. Also, Reagan did not have the intellectual curiosity and/or analytic skills to challenge this new belief structure, which did so much for him personally and politically.

My point is simply that leadership style is a product of many factors, internal and external, psychological and philosophical. A study of leadership needs to be sensitive to these factors rather than cavalierly dismiss one of them as always of secondary importance. Clearly, ideas, rhetorical skills, and the moment matter, but so do inner drive, ambition, and whatever personal insecurities are displaced onto a political agenda and into the political arena. All these factors help distinguish leaders from followers.

Presidents and Prime Ministers is an important, original work that makes a major contribution to our understanding of political leadership. It is likely to stimulate debate on how best to understand and evaluate the critical components of successful leadership in increasingly democratic polities. I highly recommend this book.

Russia's New Politics: The Management of a Postcommunist Society. By Stephen White. Cambridge, UK, and New York: Cambridge University Press, 2000. 386p. $65.00 cloth, $25.00 paper.

Allen C. Lynch, *University of Virginia*

After a highly successful series of revisions of his original *Gorbachev in Power* (1990), which ultimately became *After Gorbachev* (1993 and 1994), Stephen White has written an entirely new book that takes stock of contemporary Russian politics after nearly a decade of postcommunist experience. He draws upon a mass of new and newly available data, including archival materials, survey data, memoirs and periodical literature, interviews with political principals, and a series of Russian elections at the national, regional, and local level. White has written a very solid companion to any course on the structure, dynamics, and economic and social consequences of Russian and, to a certain extent, postcommunist politics.

The book is very much about Russia as distinct from Russia in comparison to other transitory postcommunist, or postauthoritarian, states. Those who need to look at the national Russian scene in some depth need not go much farther: White provides an invaluable description and analysis of Russian parties (such as they are) and electoral behavior, the strong bias toward presidentialist government, the course of "economic reform" and its social consequences,

and Russia's relationship to the international system. He also offers a particularly thoughtful concluding evaluation of Russia's grappling with democratic forms of political representation and participation.
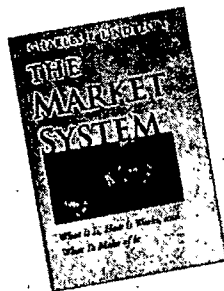
The book is full of sound judgments based upon an exhaustive reading and sifting of Russian primary and secondary source materials, which occupy 86 pages in the endnotes. The confines of the book are quite clear: There is very little on regional and federal aspects of Russia's recent political transformation and little except for passing references to postcommunist experience elsewhere. Still, it is hard to think of another work that provides such an excellent overall orientation on Russian politics by one author within one set of covers.

White comes to a thought-provoking conclusion. He asserts that Russians "were almost as far from representative and accountable government by the late 1990's as they had been at the end of the Soviet period" (p. 69), and although he clearly sees the Gorbachev period as the promising, if flawed, attempt to turn a failing system around (pp. 16–33), he is less persuaded than others that there was a decisive break with the past in 1991 (p. xi). This bears more discussion. It is true, as White avers, that the same people appear to be in power in the 1990s as in the 1980s and that there is an impressive continuity from the late to the post-Soviet period in terms of national and especially regional leaderships. But as sociologists David Lane and Cameron Ross have shown in persuasive detail (*The Transition from Capitalism to Communism. Ruling Elites from Gorbachev to Yeltsin*, 1999), the key dividing line influencing the Soviet elite's post-Soviet prospects was that between those in the administrative class whose life chances depended upon a regulatory state system and those with administrative access to liquifiable assets on the world energy, metals, and raw materials markets who, in the context of the collapse of the party-state induced by Gorbachev, were potentially able to benefit from a move to markets.

These fault lines were already deeply embedded in late Soviet society. From this point of view, as Steven Solnick's work (*Stealing the State*, 1998) anticipates much of Russian politics since the late 1980s has focused on employing the forms of constitutional government to legalize the initially creeping—and at the end rapid—comprehensive and essentially spontaneous seizure of Soviet state assets by strategically situated Soviet elites and those, such as former math professor and used car salesman Boris Berezovsky, with the cash on hand to allow privileged access to the system at the moment of the greatest "fire sale" in history.

What is of note in this context is that there were no institutions in late Soviet society capable, even if willing, of constraining this process. Thus, as White notes, there were no "pacts" to structure the Russian transition as in a number of other East European and postauthoritarian transitions; Russia has broken with communism but not with the shadow of the predominant (if currently tenuous) Russian state. Parties remain extremely feeble as articulators of public or even sectoral interests, and society, remarkably, remains largely disengaged from a political process that has presided over a breathtaking social and economic disenfranchisement of perhaps the majority of the citizens, even though the latter have the right to vote. In short, Russian politics and society are characterized by very weak institutions and a strong tendency toward decomposition, exactly as George Kennan ("The Sources of Soviet Conduct," *Foreign Affairs* 25 [July 1947]: 579–80), Alexander Solzhenitsyn (*Iz pod glyb*, 1975), and Martin Malia (*Comprendre la Revolution Russe*, 1980) anticipated. That a middle-ranking member of the former KGB, one of the few institutions of late Soviet society to preserve its corporate integrity relatively intact, should have become the second president of the Russian Federation, and by what amounts to a palace coup, speaks volumes about Russia's enduring totalitarian legacy.